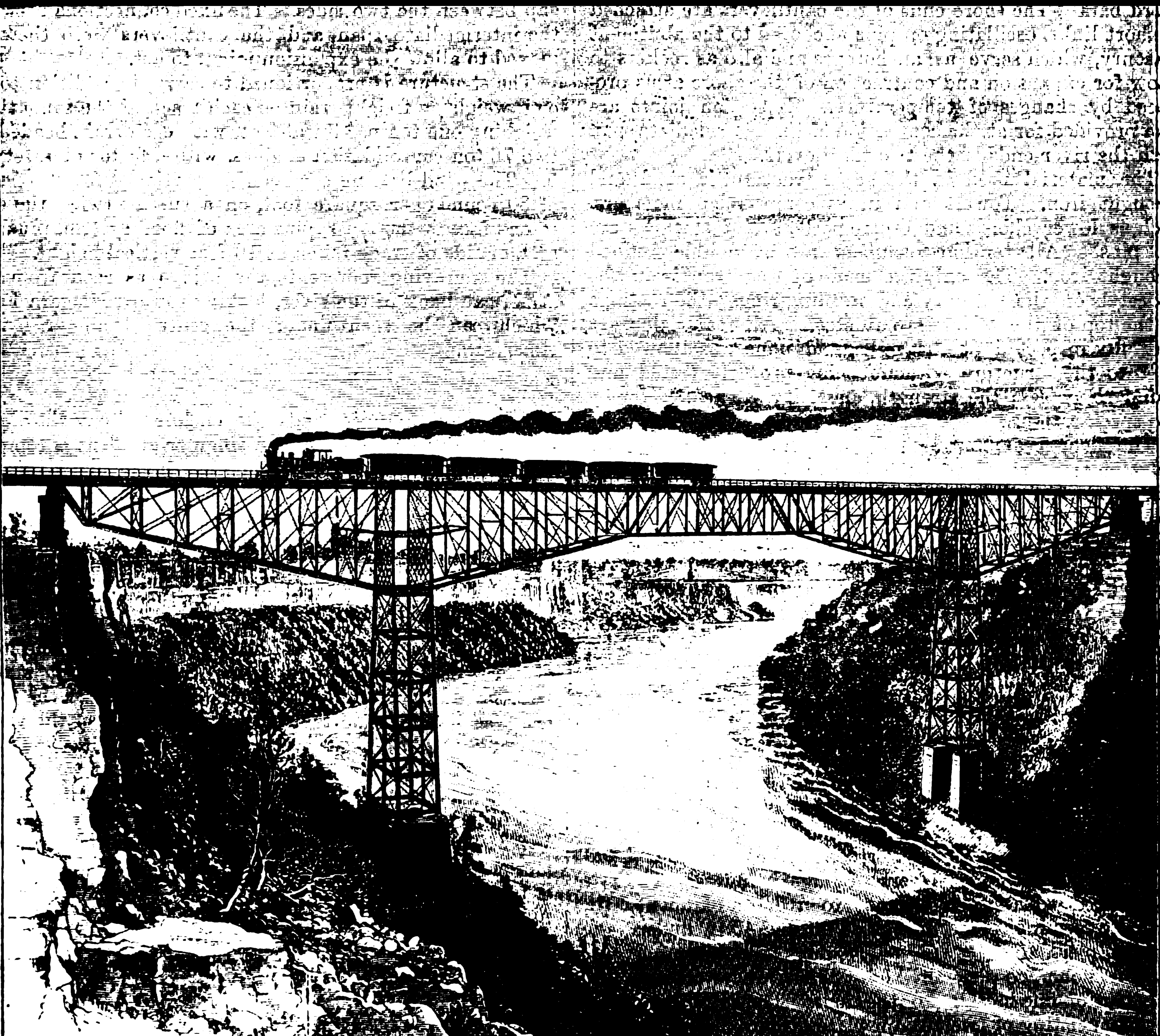

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AN ILLUSTRATED MAGAZINE OF SCIENCE

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CONDUCTED BY RICHARD A. PROCTOR.

"Let Knowledge grow from more to more."—*Tennyson.*

VOLUME V.

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MIGRATIONS OF BIRDS.

By GRANT ALLEN.

IN Mr. Darwin's unpublished fragments on instinct, lately printed by Mr. Romanes as an appendix to his admirable work on "Mental Evolution in Animals," there occurs a passage of some interest, which has given me food for thought on many occasions since I first read it, and which may serve as the text on which to hang a few reflections about the possible *modus operandi* of migration in birds. It runs as follows:—"How a small and tender bird, coming from Africa or Spain, after traversing the sea, finds the very same hedgerow in the middle of England where it made its nest last season, is truly marvellous."

Now, I suppose we may accept the fact as stated, on Mr. Darwin's authority; for he was not given to use mere rhetorical phrases in this way without having first satisfied himself that they accurately represented observed truths; and we may therefore proceed to consider how far the phenomenon in question is really explicable on familiar lines. It seemed to me, when I first read this passage, that such a geographical instinct as appears at first sight to be implied in the statement was almost inconceivable in a small bird; and its very inconceivability set me thinking by what mechanism the bird could possibly manage thus to wing its way accurately over trackless seas and barrier mountains to a single well-remembered English hedgerow. The more one thought about it, however, the more clear did it become, that this case was one where that famous "scientific use of the imagination," about which Professor Tyndall has so often discoursed to us, might find a fair field for its proper application. Let us just try to throw ourselves for a moment into the mental altitude of the bird—endue ourselves, as it were, in the avian personality—and see how far the apparent difficulties vanish or minimise themselves when we fairly face the actual position of the keen-eyed migrating creatures themselves.

The bird passes his whole existence in taking what we significantly call "a bird's-eye view" of things in general. Living perpetually on the wing, now high in air, now low by the fields, his mental picture of the country in which he dwells must always be far more map-like and wide-spread than our own, besides having much more variety of perspective and relative position. For us, the view is constantly bounded all round by hills and downs, trees and hedges, houses and walls. To get from one point to another, we do not usually go in a straight line, "as the crow flies," but wind in and out, among lanes and foot-paths, with our ultimate object usually out of sight, and finding our way by a long series of more or less perfectly remembered twists and turning-points. But the bird has no need to burden his memory with such a vast stock of clues, as mere means to his ultimate end; he need only rise high enough in the air, and he sees at once the actual spot for which he is making, and sails down towards it immediately in a straight line.

Again, we have all observed how much better a notion we have of the geography and topography of a country when we have ascended its chief hills or mountain-tops. People who walk much on the tops of downs in England get to have a very distinct picture of all the surrounding fields, plains, valleys, hills, spurs, watercourses, towns, and villages in their true relations to one another. They see the land "spread out like a map before them," and they learn to recognise the neighbouring landmarks from several distinct points of view. As a rule, if we live on the plain (as most of us unfortunately do) we know only one front or profile of each particular hill or boundary-line of the landscape; but if we walk about much on the hill-tops, we get to know the other alternative aspects of the same heights, and to recognise the various elements from whatever side. A stroll of ten miles along the summit of the North Downs, for example, from Dorking to Guildford, will give one a whole series of panoramic views over the Weald, the Thames Valley, and the Hampshire Hills, from the South Downs to the Chilterns in one direction, and from Hind Head in Hants, to Crowborough Beacon in Kent, in the other. The greater part of six counties, with all their salient points variously grouped in different perspectives, is thus embraced in the prospect from what would be (for a bird) a single morning's short flight.

Now, while we human beings can only thus rise to the height of a few hundred (or at least a few thousand) feet, at rare intervals, under exceptional circumstances (where hills or mountains happen to exist), a bird can so rise at every point, and can look down upon the country-side, not only from the hill-tops, but also over every part of the plains or valleys. Its mental picture of the surroundings of its own nesting-place must therefore be a very wide and generalised one; a little comparable, perhaps, to the mental picture which a sailor has of the path up the English Channel, or the mouth of the Thames. It conceives of its home, no doubt, as lying about the centre of such and such a saucer-shaped hollow, surrounded by such and such a shifting combination of girdling hills. Given the Thames Valley, let us say, as seen from the North Downs, with the Chilterns in the distance, it knows that its nest lies to the west of the great smoke which overhangs London, and to the east of the long blue line which marks the rolling uplands of Salisbury Plain. In order to reach it, it need not go along winding roads or between high hedges; it soars on the open in the general direction where the nest lies, and at each stroke of its wings it remembers more and more of the minor landmarks which aid it in discovering the exact spot it is seeking in the great hollow spread out in wide array below.

Now, let us put the case of a bird which nested last year in England—at Bagley Wood, near Oxford, for example—and let us suppose it has wintered in Africa, and is desirous of returning in spring to its old familiar summer quarters. The first part of the journey it will doubtless make with all the other English migrants of its own kind, either *viâ* Sardinia, Corsica, and France, or *viâ* Gibraltar and the Spanish

Peninsula. With these earlier steps we are not here concerned; for our question is not how the bird finds its way back to England at all (that problem has been already plausibly solved by Dr. Weismann), but how it finds its way back to its own particular Bagley copses. The general body of migrants, then, reaches the neighbourhood of Boulogne or Dieppe, let us say, by following its previously-remembered path. Now, from Boulogne even human eyes, supported by human legs on top of the Napoleon column, can see the English coast from the South Foreland to Beachy Head; and from Dieppe a bird poised high in air ought to be able equally well to see it from near Brighton to Dungeness. Even if it did not, however, in the latter case, there are two ways in which it might still accurately guide itself across the Channel. In the first place, if it remembered its course southward last year (aided by the general consent of its fellows) it would know that it flew in a certain direction as regarded certain points on the French coast by reversing that direction—in other words, by steering due north, at right angles to the line of cliffs, it would make or sight the English coast. (It wouldn't matter much even if its course was three or four points of the compass wrong either way, for in any case it would at last sight land in about the required direction.) But in the second place, we know now that from great heights in the air, the sea is to some extent transparent (as aëronauts have told us), and its banks and deep pools can be seen through the water, or, at least, discriminated from one another by varying colour and other marks. It is not so very difficult, therefore, to see how a bird can fly across arms of the sea, especially shallow ones; and it is known that their course generally follows the ridge of shallowest soundings in each instance.

Well, let us suppose our bird chooses the Boulogne route. Then, strange as it sounds at first sight, I believe three very simple landmarks would suffice to land him at Bagley Wood. For, from Boulogne, he could distinctly see Beachy Head, easily recognisable as being the tallest white cliff on the whole south coast, this side of Dover. For Beachy Head he and a portion of his fellows steer direct, then; and when they are above it, at, perhaps, 150 feet elevation in the air, they could once more dimly descry the North Downs, with St. Martha's Down, near Guildford, for their goal, well marked as they approached it by its high-perched chapel tower. Hither, accordingly, one detachment of the main body might make—the Midland counties contingent—while others struck off for Crowborough, Hind Head, Box Hill, or other points, whose general lie could be readily discerned from their coign of vantage, though, of course, the exact summits would not be clearly made out till they neared them somewhat more closely. From St. Martha's, even the human eye can see Nettlebed, in Oxfordshire; and once at Nettlebed, our bird would be at his own door, so to speak; for he would be looking down into the very basin with every feature of which he must be familiar from a whole summer's long experience. When one reflects that for a bird, with its free power of flight, these few landmarks between Boulogne and Bagley Wood must be hardly more difficult or wider apart in consciousness than are three successive headlands to a sailor, it is not so mysterious after all, perhaps, that it should find its way straight from the one point to the other without serious mishap. It ought also to be borne in mind that at least some birds (for example, vultures) are known to possess very keen powers of sight; and there can be little doubt that most of them surpass in this respect the ordinary human vision.

At the same time, I don't wish to suggest in any way that this little rough sketch of an explanation touches the central problem of migration at all. I offer it only as a partial elucidation of a single episode or incident in the main theme. Whether there exists a migratory instinct at all has been lately doubted by Mr. Wallace; and he may possibly be quite right in supposing that birds travel in a good many directions, but that only those which travel towards the south and towards the land ever survive. Still, if it does sometimes really happen that birds return year after year (as has frequently been reported) to their accustomed nest, that fact in itself calls for explanation; and it seems to me that a sufficient realisation in one's own mind of the ordinary visual altitude of birds enables us largely to get over the obvious difficulty, and to see that the apparent miracle may be really explicable on just the same principles as those by which we explain our own sailing along a known shore with a few easily remembered and conspicuous sea-marks.

ASTRONOMICAL COLLISIONS.*

By Prof. C. A. Young, Princeton, N.J.

(*Continued from page 389, Vol. IV.*)

If ever two great worlds do really meet in this way, it is possible to predict some of the consequences. To use the technical language of science, "their energy of molar motion will be converted into various forms of molecular and potential energy;" which, translated into the vernacular, means that there will be evolution of heat and light, while at the same time the solids present will be wholly or in part liquefied, the liquids vaporized, and the vapours and gases rarefied and expanded. The intensity of the action will depend, of course, mainly upon the mass and swiftness of the colliding bodies; but an easy calculation shows that if our earth were ever to meet another globe like herself and moving with the same velocity, heat enough would be generated by the shock to transform them both into a huge ball of vapour; unless, indeed, the central core of the earth is much colder and more refractory than usually supposed. At any rate, the quantity of heat developed would be sufficient to melt, boil, and completely vaporise a mass of ice fully 160 times that of both the colliding worlds—an ice planet 97,000 miles in diameter.

If, however, the impinging masses were, to begin with, mainly gaseous (as the sun seems to be), the effect might be curiously different. Heat would, of course, be generated, just as in the case of solid bodies; but, as a consequence, apparently most paradoxical, the resulting nebula might actually be cooler than either of the bodies before the encounter; of course, it would be immensely expanded in volume. Just as a gaseous mass, contracting under its own gravity from loss of heat by radiation at its surface, continually rises in temperature, so a similar mass, expanding against its own gravity from accession of heat within, may fall in temperature—nay, must fall, if the body is composed of "perfect" gas. Of course, immediately after the collision, and before the ultimate expansion of bulk was attained, the temperature and brilliance of the mass would be for a time vastly increased, but the final result would be as stated.

In a preceding paragraph it was said that we have not on the astronomical records a single certain instance of any collision between bodies of considerable size. But there have been several cases of a most remarkable phenomenon, which perhaps may owe its explanation to such encounters. Stars, never before visible, or else known only as faint and

* From the *North American Review.*

minute, have suddenly blazed out, shining sometimes with the brilliance of Sirius, or even Venus, and afterwards faded away to their original insignificance. Such was the famous star of 1572, so carefully observed by Tycho Brahe. As recently as 1866, and again in 1876, similar things have happened, though these later stars were less conspicuous. The star of 1876 (in the constellation of Cygnus) had long been known and catalogued as a telescopic star of the ninth magnitude, with nothing to distinguish it from any of the common herd. On Nov. 24 it suddenly shone out as bright as the pole-star. Schmidt, of Athens, who discovered it, had been observing that very region of the sky only four days before, and no such object was then visible. The intervening days were cloudy, so that we cannot tell precisely the hour when it first blazed up, but evidently its increase of brightness must have been extremely rapid. Immediately after Schmidt's observation it began to fade, but it was two weeks before it fell below the sixth magnitude and became invisible to the naked eye, and it was more than a year before it resumed its original faintness. Its whole course was carefully watched with the spectroscope. At first, and for some months, like the star of 1866, it showed conspicuous in its spectrum the lines of hydrogen, together with other lines of uncertain origin. As its brightness decreased the hydrogen lines faded still more rapidly, leaving other lines more prominent, until, at last, the spectrum, instead of becoming that of an ordinary star, came to consist simply of three bright lines—the spectrum of a nebula—and it continues such to this day. Whether this is, or is not, the same spectrum it had before the outburst no one can say, as it had never been observed with the spectroscope before.

Now, I am very far from asserting that this was a case of collision; and yet it is clear enough that the whole course of phenomena was very much what might have been expected if it were. At any rate, the collision hypothesis is held and defended by several astronomers of authority, and the principal reason for preferring a different possible explanation lies in the fact that these "temporary stars," as they are called, are far too common to be probably due to stellar encounters. We have on record eight since the Christian era, and no less than three of them since 1840.

It is hardly necessary to say that if any retinue of planets attends such a star, its sudden blaze cannot fail to carry disaster and destruction to all life upon them. We can conceive that forms of life may be possible under almost any imaginable physical conditions, provided only the course of events by which they have been reached has been gradual enough to allow the necessary adaptations. One would not dare to deny the possibility of life of some kind on the airless moon, or even on the sun itself. But all we know makes it certain that no planetary life could survive the tremendous and catastrophic change of conditions involved in a sudden thousand-folk increase of the solar radiation.

Thus far we have been considering only collisions of stellar masses. But space is filled also with minor particles separated from each other only by intervals of a few hundred miles; and these, in the form of meteors and shooting-stars, are rushing through space, dropping continually upon the larger worlds, increasing their size, and adding to their store of heat and energy. Every year the earth encounters nearly three thousand millions of them, according to the estimate of Professor Newton, ranging from the merest particles to masses of several hundred-weight. Very probably, also, the comets belong to the same category, being really nothing but larger meteors, or flocks of small meteors, or perhaps even only puffs of meteoric dust. Nearly all the meteors which strike the earth are very minute. Perhaps a hundred or so reach the ground each year as recognisable masses of stone or iron, weighing from an ounce or two to some hundreds of pounds, but all the rest are dissipated in the upper air, and never come down unless as impalpable dust, not to be certainly identified. The whole amount of matter falling daily upon the earth from outer space is probably about one hundred tons on the average (it is variously estimated from twenty-five to five hundred). A hundred tons is in itself a very considerable quantity, but utterly insignificant as compared with the mass of the earth, and entirely incapable of appreciable effect upon our temperature. Assuming even the largest estimate (five hundred tons a day), and also that the average velocity with which meteors enter our atmosphere is fifteen miles a second (probably considerably too large), we find that the heat annually received from them by the earth is only about fifty-three calories for each square mètre of her surface,—less than would be imparted by two minutes' perpendicular sunshine, and only about $\frac{1}{30,000}$ of the heat actually received from the sun in a year. Retaining the same extravagant estimate of 500 tons a day, it appears that the earth's diameter would grow an inch in about one hundred millions of years, and that her distance from the sun would be reduced about 83 feet in a million years, in consequence of the resistance experienced in moving through the meteoric swarms.

It would not, however, be just to the general reader to dismiss the subject without fair notice that some most eminent astronomers hold views at variance with those above expressed. The late Professor Peirce maintained to the end that the heat of the sun is chiefly due to the impact of meteors, and also that the earth itself receives as much heat from meteors as from the sun, a necessary result if the solar heat is really so produced. Of course, we have no space to discuss the matter here, and must be content with merely saying that the quantity of matter which falls upon the earth, if his idea is correct, ought to be easily noticeable, amounting to about 50 tons a day on each square mile. It involves also serious difficulties in the planetary theory.

So far as human beings are concerned, the most important question connected with our subject is whether anything is to be feared from comets, as a consequence either of collisions with the earth or of their fall into the sun. It may be said, just as of the stars before, first, that cometary collisions, either with sun or earth, must be very rare occurrences; and, secondly that they are practically certain to happen some time or other. Babinet computed, on the one hand, that a comet would strike the earth on the average about once in fifteen million years; on the other, we know at least three comets whose orbits cut the earth's path so closely that, if they should ever reach the crossing at the same time as the earth, a collision must occur. These comets are known as Biela's, Tempel's, followed by the Leonid meteors in its train, and the comet of 1862, which precedes the Perseids. One of the three, however (Biela's), has probably ceased to exist as a comet, and it is quite possible that the other two may vanish in the same way before we meet them.

As to cometary encounters with the sun, no comet has yet been known actually to strike the sun, but several have grazed very near it. The great comet of last year brushed through the corona, and came within three hundred thousand miles of the photosphere, and there is no assignable reason why some other comet should not actually pierce it.

As to the consequences of a comet's collision with the earth, it is impossible to predict them with scientific certainty; probably, however, they would be insignificant.

We know absolutely that the whole quantity of matter in a comet (technically, its "mass") is extremely small compared with that of the earth; but just how small no one can say. It is impossible to contradict authoritatively either the man who says he could carry a comet home in his pocket if properly packed, or one who asserts that a comet's nucleus is equivalent to an iron ball 100 miles in diameter. An attempt has been made to get at the density of the central nucleus on the assumption that it holds to itself the enormous volume of the head and envelopes by its gravitational attraction, as if the surrounding nebulosity were an atmosphere in equilibrium around the nucleus; but the assumption is more than doubtful, and the derived conclusion, of course, is of little value. It seems, on the whole, more probable that a comet is throughout only a cloud of dust and vapour—a mere smoke-wreath—than that there is at the centre any solid kernel of preponderant mass.

If a comet really has at the centre any great mass of stone or iron, or even a close-packed swarm of aërolites weighing a ton or two apiece, collision with it would of course be a most serious matter, spreading devastation and conflagration possibly over half the globe at once; not amounting, probably, to anything like a destruction of the world, but far more disastrous than any earthquake or volcanic eruption. It is far more likely, however, that the encounter with a comet would be entirely harmless—simply a most magnificent and brilliant shower of shooting-stars lasting for an hour or two—a phenomenon which one might well desire to see.

As for the effect of a collision between a comet and the sun, in respect to which there has been much disquieting speculation of late, the probability is that we on the earth should never know it, unless we happened to be expressly watching the event. For a few minutes, just as the nucleus was piercing the photosphere, there might be some unusual flash of brilliance and a fine outburst of solar prominences, followed, perhaps, and very probably, by intense magnetic and auroral disturbances on the earth; but the main thermal transformation of the impinging energy would be effected far below the visible surface of the sun, and would result merely in a slight expansion of its bulk, far too slight to be detected by terrestrial astronomers.

THE AMATEUR ELECTRICIAN.

BATTERIES VIII.

THERE is, perhaps, no greater fallacy than the idea that pervades large crowds of would-be inventors that a battery can in the matter of cost compete with a steam-engine as a source of energy. Of course, if we desire to ring a bell by electricity we should be mad were we to attempt it by means of a steam engine and dynamo, but we should be none the less fitting candidates for Hanwell were we to attempt to light the streets of London by means of any primary battery yet constructed. Nevertheless, there are many who have approached within a measurable distance such an aspiration. Most things in this world have a location or a function to which they are most advantageously adapted. To make them occupy any other position, or perform any other duty, is to more or less seriously handicap them in their struggle for existence. The electrical world knows no exception to this general rule. Where small quantities of energy are required, especially in out-of-the-way places, whether for bell-ringing, plating typing, telegraphy, &c., there is nothing to compare with the primary battery; but where large quantities of energy are required the battery should be at once relegated to the remotest distance as a thing to be shunned. The reason for this is not far to seek. The cost of producing energy by means of coal consumed in a steam-engine rapidly increases in proportion as we reduce the quantity of energy produced. Thus the proportionate cost of a one-horse-power engine is nearly, and often quite, three times as much as that of a ten-horse-power engine. For much less than a horse-power the cost (notwithstanding the relative cheapness of the fuel) and the trouble of maintenance increase out of all proportion, and an engine which would also be expensive to construct is therefore never used to produce such small quantities of energy. The advantages of batteries are that they are very cheap in construction, regular (within certain limits) in the amount of energy produced, capable of being fitted up in almost any place, and enable us by means of fixed wires to utilise the energy at places more or less remote from its source. Against these advantages there is to be considered the short duration of most forms of battery. When used extensively the cost of the zinc and chemicals is an important item. The energy evolved when a pound of zinc is consumed is about one-tenth of that resulting from the combustion of a like quantity of coal. A pound of zinc costs, say 4d., while a pound of coal costs but ·1d. (at 18s. 8d. per ton), consequently, to produce a horse-power by the combustion of zinc would cost, theoretically, 400 times as much as it would to produce the same energy by the combustion of coal, and we may safely maintain that no battery has yet been constructed which can be less than 50 times as costly as its competitor, and this only when a large allowance is made for more economical consumption in the battery than in the engine. A trifle may be regained by the sale of by-products, but it is only a small amount, and oftener than not quite negligible. It is, therefore, no wonder that the more prominent electricians and chemists have long regarded the utilisation of batteries for electric lighting as luxuriously extravagant. And this being the position taken by such men "it is hardly to be expected that men who, in some cases, are neither electricians nor chemists can hope to succeed." Where special circumstances exist, as for example in surgical or dental operations, in the sick-chamber, and for experimental purposes the battery may be more convenient.

We have been induced to say this much because undue prominence has lately been given to efforts which are being made in sundry places to introduce primary batteries as a source of energy for electric lighting. In one case, fifteen cells, which appeared to be modifications of the bichromate type, were used to illuminate twelve or fifteen five-candle-power lamps. The cost was stated to be a farthing per hour per lamp. This was apparently the estimated cost of the solution, but no account seems to have been taken of the zinc consumed. The chief merit in the battery referred to was its ingenious construction, affording as it did excellent facilities for charging and discharging. We cannot offer a definite opinion as to the relative value of the cell, as the constitution of the solution is kept secret. The plates are zinc and carbon, and the cell, if it has the durability claimed for it, may prove a good one.

Turning to the less ambitious forms, that known as the "granule" cell is a useful one. It is a compromise between the Leclanché and the bichromate. In an outer earthenware jar is placed a porous pot, the intervening space being occupied by a carbon plate and crushed (or granule) carbon, which together form a negative plate of unusually large surface. A zinc cylinder or rod is placed in the porous pot, which is then filled with a solution of sal-ammoniac. A solution

of bichromate of potash and hydrochloric acid occupies the outer cell, or the spaces between the pieces of carbon. The large forms generally have a tap at the bottom of the outer jar, to facilitate the removal of the spent bichromate solution. Such a cell, on account of the large amount of surface exposed, offers but very little internal resistance while the electromotive force of the cell is high—about two volts. The absence of acid in the porous pot implies an absence of local action, although that will only be a question of time, as sooner or later some of the acid solution in the outer jar will, by the ordinary laws of diffusion, find its way into the porous pot; even then, however, local action may be prevented if the influx of acid is not too rapid.

On reference to our description of the Leclanché cell, it will be seen that when the zinc enters into chemical action with the sal ammoniac (NH_4Cl), ammonia (NH_3) is set free, and is dissolved or absorbed by the water. Now, ammonia is a substance which has the power of neutralising acids; so that when the acid enters the porous pot it becomes neutralised before it reaches the zinc, and in that way prevents local action being set up. Polarisation, or the accumulation of bubbles of hydrogen, is to a great extent prevented, the large carbon-surface helping materially to bring about this result. Our experience of the cell, and the experience of many others, is that it is a good one.

DOUBLE PERSONALITY.

A VERY remarkable case of double personality is described in a letter of Dr. Dufax to M. Azam. (*Revue Scientifique*, 1*st December*, 1883, *p.* 703.) He states that his *confrère*, Dr. Sirault, of Ouzin, had a young servant whom he often threw into a state of somnambulism. One day he was surprised to find this girl in the prison of Blois, where he was medical attendant. Upon inquiring he found she had left Dr. Sirault and entered the service of a lady, who accused her of theft. She protested her innocence with tears and sobs. Dr. Dufax thought her case might resemble that of his former patient, Mdlle. R. L., which he described in 1876 (*Rev. Sci.*), and who was in the habit of disposing of various objects during somnambulism, and supposing them lost when she woke up. On being asked whether she had such attacks in the prison the young girl knew nothing about it, but an attendant was able to inform the doctor that she rose every night, dressed herself, and walked round the dormitory. Having often seen M. Sirault send her to sleep by putting his hand on her forehead, Dr. Dufax operated in the same way, and then learnt from her, in reply to inquiries, that she had never thought of robbing her mistress, but considered some articles of value would be safer if placed in another cabinet. She intended to tell her mistress of their removal, but she forgot all about it in her waking condition, and never saw the lady in the somnambulic one. The doctor mentioned the circumstance to the *Juge d'Instruction*, who smiled incredulously, but permitted the girl to be taken to the lady's house where she immediately found and restored the missing objects. The girl was, of course, released, and the lady made what excuses she could for her hasty conduct.

THE TELEPHONE ON FRENCH RAILWAYS.—The French railway companies in general have decided to adopt the telephone as a warning apparatus, instead of the telegraph. The Western Company made this change some time ago, but the other companies have only now determined to follow its example.

HOW TO MAKE USEFUL STAR MAPS.*

By RICHARD A. PROCTOR.

THERE is no readier way of learning to know the stars than by occasionally mapping a constellation or a star-group, until one has filled a portfolio with useful sketches of the heavens. In well-constructed maps, on a large scale, and comprising but a small region of the heavens, we can mark in the place of any double star or other object of interest we may wish to examine, and so immediately learn whereabouts to look for it on the heavens. One cannot do this with ordinary star-atlases; because each map covers so large a part of the heavens that the scale must be small or else the maps unwieldy; while too often, from the same cause, the maps are in places so distorted that even if one marks in a star by the data given in the list of objects, one can yet form no clear notion of its position with respect to neighbouring objects. Besides, it is pleasant to have large-scale sketches of favourite groups or constellations. In such sketches one can do what is quite impossible in small-scale maps, viz., give to the large stars something like their true proportionate magnitude (magnitude being the only representative we have for brilliancy) without their covering a space as large perhaps relatively as four or five moons.

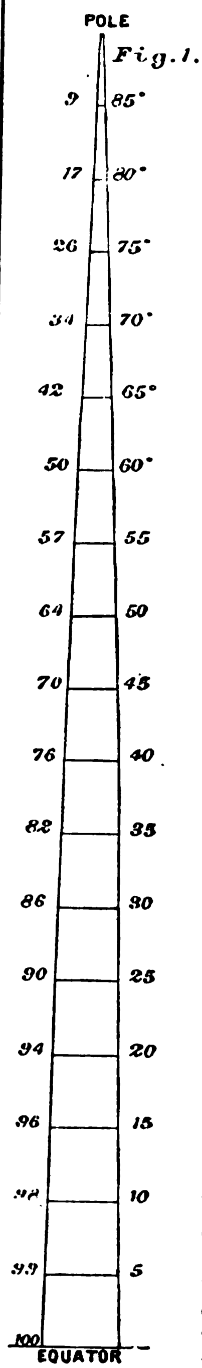


Fig. 1.

I propose to give some simple rules, by following which any one can lay down in a few minutes the meridians and parallels corresponding to any part of the heavens; afterwards filling in the stars at his leisure.

First we must consider the true shape of a strip of the globe, between the pole and the equator, and bounded by meridians five degrees (say) apart. This is shown in Fig. 1. We see that the bounding lines, though slightly curved in reality, may be looked upon as straight for any short portion of their length without introducing sensible error. It is on this fact that the mode of construction I am about to describe depends. If any portion of the strip represented in Fig. 1 had really straight edges, we could bring another similar portion alongside of it, without gaps or overlapping, another next to that, and so on, until we had space enough for a constellation or star-group belonging to that part of the globe. By *making* the sides straight, which we can do, as I have said, without appreciable error, we get such a space, the meridians and parallels of which are sensibly correct; and there is no accumulation of error as in other modes of mapping. Each space has the same error of shape that its next neighbour has—no more and no less.

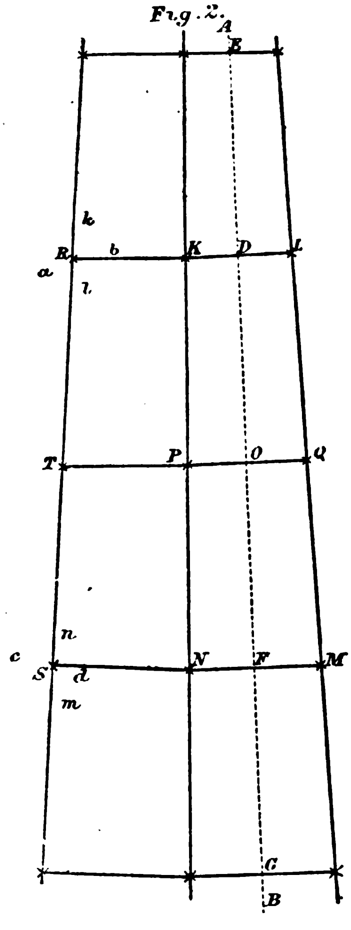
* Reprinted from the *British and Foreign Mechanic*, a long-since deceased weekly journal.

The numbers down the left side of Fig. 1 indicate the proportionate length of the cross-lines opposite them; the numbers down the right hand side indicate the distance from the equator in degrees.

Now suppose we wanted to make a map of a part of the sky the middle of which is 55° from the equator—there or thereabouts. And suppose further that the length of the region we wanted to map measures, from north to south, about 20°. Then the shape of a strip between two meridians, 5° apart, should be the same as the shape of the part of Fig. 1, between 45° and 65°. But we must straighten the sides. How can we do this with the least possible error? If we joined the ends of the cross-lines opposite 65° and 45° by straight lines we should have a small but still appreciable error; and so we should if we drew through the ends of the cross-line opposite 55° straight lines touching the curved boundary lines. We shall diminish the error to one-fourth, or make it practically inappreciable if, instead of either plan, we join the ends of the cross-lines opposite 40° and 60°.

Fig. 2 shows this done on an enlarged scale.

Fig. 2.



We draw first the central line A B. Along it we measure off four equal parts, C D, D E, C F, and F G. The length of any one of these parts is to be our basis of measurement. Divide one of the parts C D into ten parts, and one of these again into ten; this will enable us to measure off any number of hundredths we please; but those who prefer it may make a plotting scale in the usual way.

Draw the perpendicular cross-lines K D L and N F M, and take K D and D L each equal to 50 hundreths of D C, —the number 50 being found opposite 60° in Fig. 1. Similarly take N F and F M, each equal to 64 hundredths of D C, draw the lines K N and L M. These are two of our meridians.

Bisect K N in P, and take off parts equal to K P, or P N, along the produced line. Do the like with L M. The points obtained in this way belong to the parallels of our map. (It need hardly be said that if the range of the map is greater, the line we should get corresponding to K N would have to be divided into more than two parts).

Now notice that all we require further is to repeat the figure we have obtained as often as may be required to give the map the necessary breadth to include our star group. I give the geometrical construction for the purpose; but would advise the mappist to apply the much easier and more exact method given afterwards.

With K as centre and radius K L describe the arc *k l*; with N as centre and radius N M the arc *n m*; and with radius N L or K M, and centre successively at K and N describe the arcs *c d* and *a b*. The points R and S thus obtained belong to the meridian next to K N: we join them, and divide as we divided K N and L M. Next on the side L M we repeat the process. Then on the side R S, and so on, on alternate sides, until we have as many meridians as we want. The construction has also given us the points along our parallels, and we can join these as shown in the figure. Our map is then ready for filling in the stars.

(*To be continued.*)

EVIDENCES OF THE GLACIAL PERIOD.

(*Continued.*)

OUR last article on this subject was devoted entirely to the consideration of the inorganic evidences of the Glacial Period afforded by moraines, polished and striated rock surfaces, erratic blocks, perched boulders, scored and rounded mountains, and dome-shaped rocks; this article will be devoted, as promised, to the additional organic and collateral evidences of the same.

First as to the organic evidences.

In many parts of England are found sands and gravels, often at a considerable height above the sea-level, containing *sea* shells of an *Arctic* character. This, of course, is evidence that the land where such sands and gravels are found was once depressed beneath the sea-level, while the Arctic shells found therein show that the waters of the sea at that time possessed an Arctic temperature. These sands and gravels are called "glacial drift." They are found up to a considerable height on the Welsh mountains, and, wherever they contain sea shells of a kind now found only in Arctic regions, furnish conclusive evidence that the place where they are found must during some period have been submerged beneath the sea-level in an ocean of a very cold and Arctic nature—that is, during the glacial period. The occurrence of Arctic sea shells in this drift is, therefore, the chief point to be noticed under this head.

Of course, there are other sands and gravels besides those thrown down at the bottom of the sea. For example, there are "river gravels," formed by the action of rivers. Very often these "old river gravels," as they are called, are found near the banks of rivers, but high above the present level of the stream, the river having cut its bed deeper and left its old gravels, as its were, stranded. Very many of these old river deposits are found throughout England, and often constitute our present gravel-pits. This brings me to notice the bones and remains of animals which have been found so abundantly in these river gravels, the most im-

portant point in connection with such animals being that many of them were specially adapted to a severe and rigorous climate. For instance, the mammoth, a species of elephant, was covered all over with long, thick hair, evidently furnishing a very warm cover against the cold winters. We know that the mammoth was covered with this long, coarse hair, because the remains of one were discovered in Siberia which had been so well preserved in the ice that the hair was still intact; so that it is obvious that the mammoth was an animal specially suited for a very cold climate. Now, remains of the mammoth have been found in England in the greatest profusion; indeed, the tusks and teeth of this animal have been discovered in such plenty as to render impossible the slightest doubt that the mammoth once roamed over England in the greatest abundance. Such being the case, the evidence afforded by the remains of these huge animals, with their coarse, hairy covering, concerning the existence of a glacial period is very strong. Similar evidence is afforded by the remains of the hairy rhinoceros, woolly bear, &c., which abound in the old river gravels of England and the Continent.

Besides the old river gravels, so plentiful in our own land, many dried-up lakes are also to be found, especially in Yorkshire, between Bridlington and the Spurn Point, in the district known as Holderness. In these lake deposits are found the bones of extinct animals, notably those of the great extinct elk, a somewhat allied species to the existing reindeer. As everyone knows that reindeer now inhabit such cold countries as Norway, Lapland, &c., the inference afforded by these remains found in Yorkshire, Ireland, and elsewhere in favour of the existence of a glacial period is also very strong.

Besides river gravels and lake-deposits, a great number of caves are found in England and the Continent, in which caves have been discovered innumerable bones of the mammoth, hairy rhinoceros, woolly bear, reindeer, &c., &c. The presence of these remains in the English and Continental caves in such plenty plainly points to a time when the climate was more suited to those animals than at present. Such a time would be a glacial period such as we have already found good reason to believe actually occurred from our examination of several peculiar features in the scenery of the Northern Hemisphere generally. So that the organic evidences afforded by the remains of Arctic shells and the bones of Arctic animals furnish a valuable corroboration of the inorganic evidences afforded by moraines, polished and striated rock surfaces, erratic blocks, perched boulders, scored and rounded mountains, and dome-shaped rocks.

In addition, however, to the organic and inorganic evidences which we have seen unite to demonstrate the occurrence of a glacial period, we also find certain collateral evidences tending in the same direction. These collateral evidences are furnished chiefly by the facts connected with the geographical distribution of animals and plants, chiefly of plants, to which we will confine ourselves here. It is a well-known fact that the same plants occur on all, or nearly all, the mountain summits of Europe, no matter how widely these mountains may be separated from one another. How, then, have the same plants come to occupy stations so far apart? This question, so difficult to explain by any other means, becomes quite easy to answer on the assumption—supported by the other evidences we have mentioned—of the occurrence of a glacial period; for during such glacial period an Arctic fauna and flora would cover the whole of the lowlands of Europe north of the Alps and Pyrenees; but as the warmth gradually returned, the Arctic forms of life in the neighbourhood of mountains would gradually ascend such mountains higher and higher, while their brethren of the plains would at the same time be gradually driven northward by the returning warmth. Thus, when the warmth had fully returned, the same plants and animals which had during the glacial period lived together all over the European lowlands, would be found in the present Arctic regions of the North, and on many isolated mountain summits far distant from one another, just as we actually do find them at the present day. There are other facts in the geographical distribution of species which only the glacial period can explain; but the example given will be sufficient to show that the facts of such distribution point clearly to a glacial period as the cause of the present geographical distribution of species, particularly of plants in Europe.

We thus see that in addition to the inorganic and organic evidences which have led us to believe in the occurrence of a glacial period, the facts of the present distribution of species furnish strong evidence in the same direction, so that we have at least *three distinct lines of evidence all leading us up to the same conclusion.* These several evidences may for the sake of clearness be arranged in a tabular form, thus:—

I. INORGANIC EVIDENCES.

Moraines, polished and striated rock-surfaces, erratic blocks, perched boulders, scored and rounded mountains, and dome-shaped rocks, indicating ice-action.

II. ORGANIC EVIDENCES.

Remains of Arctic shells in drift.

Remains of Arctic animals in old river gravels, lake deposits, and caves, including those of the mammoth, hairy rhinoceros, woolly bear, reindeer, elk, &c.

III. COLLATERAL EVIDENCES.

The facts afforded by the present geographical distribution of species, especially of plants in Europe, &c.

It must be seen from the above that the belief in a glacial period is one supported by a strong chain of evidence—evidence, indeed, so strong as to render the occurrence of such a period not merely a matter of speculation or theory, but of established scientific proof. If the writer has succeeded in summarising these evidences so as to bring them home to any one not acquainted with the subject in a short and intelligible manner, his object will have been achieved.

RECREATION IN SKILL.*

I NAMED a third chief element in recreations: the opportunities which they give for the exercise of skill in something which is different from our regular work. This may be in either mental or muscular work or both. One who has been all day busy in reading may refresh himself in composition; another turns from teaching to learning, and enjoys it the more for the contrast; another, most wisely, from the routine of business to some difficult research in science or in history; another from literature to music or the fine arts, whether in the practice or in the critical study of them. All these changes are the daily recreations of large numbers of the more cultivated classes; and they are matched in the instances of those who turn from their day's manual work, or the routine of life in offices or shops, to the mental work offered to them in colleges and evening classes. The labour of their study may be greater than that of their work; it may increase their fatigue; yet after sleep they may be conscious of an

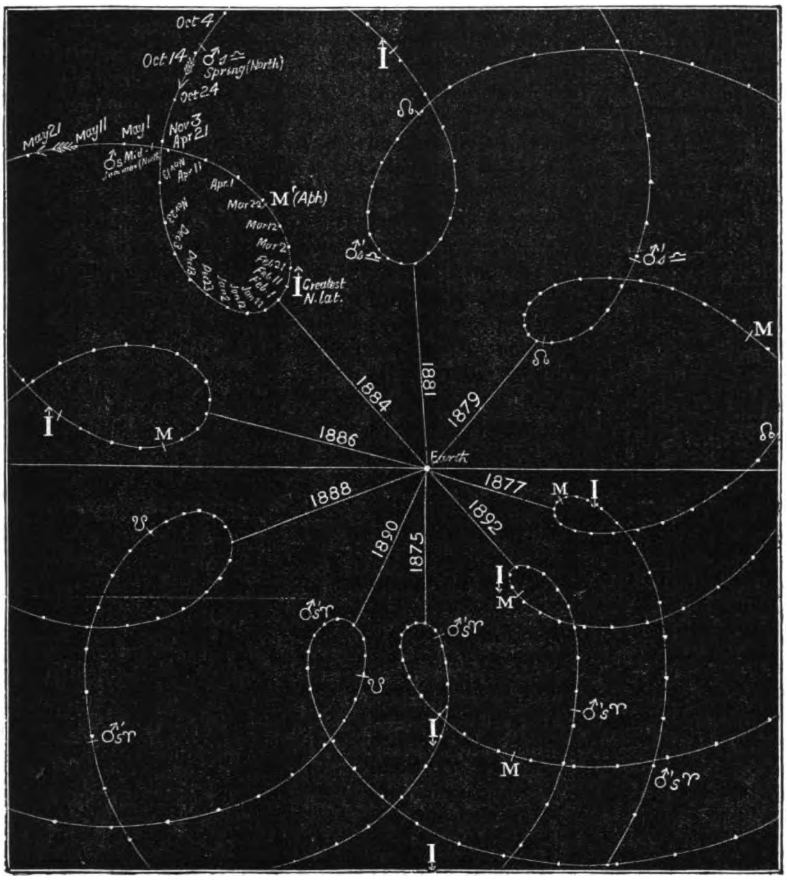
* From an article by Sir James Paget in the *Nineteenth Century*.

increased fitness for the business of their lives by reason not only of the knowledge they have acquired, but of their mental recreation.

But the most obvious examples of this part or method of recreation are in muscular exercises; in athletics; in the acquirement and exercise of skill in cricket or lawn-tennis, or on cycles of whatever wheels, or in the finer manual skill at billiards or in music. Any of these may be studied as patterns of active recreation; for they include all the three chief conditions for refreshment. Think, for example, of the delight and power of a game of cricket after a day's work for one employed at a desk, or in reporting, or bricklaying. The same charm of wonder and the same kind of contrast may be found in a thousand other instances. We enjoy the surprises of conjuring tricks, which to the conjuror himself, I suppose, give no stirring pleasure; and of fireworks, and the stories and actings of perilous adventures. More worthily we may enjoy and be refreshed by the marvels of skill in art, in music, or in singing.

When we listen to a long-sustained high note—such as Albani can sing or Joachim can play—we are refreshed not only by the beauty of the sound, but by the wonder that it can be produced, and it is this that most refreshes us when long afterwards we can recall the sound. It is the marvels of the ever-changing scenery, the decisive skill of the last climb, the final certainty, the awful beauty of the scene. Here is complete contrast with the routine life of the past months. He may never before have been so thoroughly tired, but next day he may be conscious that he never before was so thoroughly refreshed in both mind and body. After a vacation spent in pleasures such as these he may come home and rightly call himself "very fit" for the old work; he may even be so conscious of his fitness that he may enjoy the showing of it, and may love the work that he hated.

It would be easy, if it were not for their number, to show in how many of the most popular active recreations the three things which I have indicated are, in different proportions, combined. They may be overlooked or forgotten in the mere enjoyment of change, or of pleasure, or of partial freedom from responsibility; but there they are, and I believe that the happiness and utility of recreations may be nearly estimated by their amount: there are lives in the monotony of which they alone may sustain a good spirit of enterprise, of reverence, and of willing effort.



The path of Mars relatively to the earth during the opposition-period of 1884, with the corresponding loops for several past and future opposition-periods.

THE PLANET MARS.

This planet is drawing nearer day by day and will continue to do so until Feb. 1, when it will be at its nearest. We shall show its course among the Zodiacal stars the week after next, giving next week that part of the Zodiac along which Jupiter is travelling, and the week following the next Zodiacal map in which the paths of Jupiter and Mars for the next two or three months will both be shown. The Zodiacal maps showing the sun's course in January and February will be given later, as the planetary paths require earlier delineation.

We give this week a picture showing the actual course, relatively to the earth, which Mars is at present pursuing; and for comparison the loops followed during several past approaches and to be followed at future returns of the planet. The picture explains itself.

The Foreign Trade of India in 1882-3. —The trade statistics of India during the official year 1882-3 reveal some very encouraging facts, which are rendered still more satisfactory by the increasingly favourable returns of India's foreign trade during the last five months. Another symptom of the increasing wealth of India is manifested in the import of gold. In 1878-9 there was as export of the precious metal of £896,173; but now, and for some years, there has been a steady and growing import of gold, and during the year under review the flow of gold into the country has risen to nearly five million sterling (£4,930,871). Of the entire trade that could have been conveyed that way, more than 86 per cent. passed through the Suez Canal, while 30 per cent. of the trade of India is with China, Australia, and other countries which cannot naturally avail themselves of the Suez Canal. During the last five months the wheat trade shows an increase in value of more than three millions sterling.

THE UNIVERSE OF SUNS.

By Richard A. Proctor.

(*Continued from page 384, Vol. IV.*)

KEPLER'S quaint system described in the last number but one is illustrated in Fig. 1. Wright's may be regarded as in some sort illustrated by Fig. 2, only it must be understood that in this illustration the distances between the various galaxies are very much too small, or rather the galaxies themselves are on an enormously exaggerated scale as compared with the distances between them.

Five years later, Kant published his remarkable treatise "The Natural History and Theory of the Heavens." It was in this work that the philosopher of Königsburg predicted the discovery of planets exterior to Saturn. His ideas respecting the universe of stars were admittedly suggested by Wright's "Theory of the Universe" which he had read in a Hamburg journal of the year 1751, —though he is unable to indicate, he says, "to what extent his system is a reproduction or amplification of Wright's."

Passing over some remarks,—worthy of Kant's genius,—on the nature of the ideas we should form respecting the Creator and His Works, Kant proceeds to lay down the following theses:—

(1.) The stars,—not only those visible to the unaided eye, but all which the telescope reveals,—are suns, the centres of systems, as important as the solar system, and ruled like that system by gravitation.

(2.) But the action of gravity not only rules within such systems,—it extends from system to system. Thus all the stars are associated into a single system—the galaxy. They are in fact planets (that is, moving orbs) of a higher order.

(3.) As in the solar system there is a general level near to which all the planets travel, so in the system of stars we recognise a condensation towards a certain mean plane. This condensation explains the appearance of the Milky Way, which forms the Zodiac of the stellar system. All the stars travel on paths but slightly inclined to the mean plane of this stellar Zodiac, just as all the planets travel on paths but slightly inclined to the mean plane of the planetary Zodiac.

(4.) As the planets travel around a luminous central body it is probable that a vast luminous central orb exists round which the stars perform their circuits. Such an orb might be 1,000,000* times larger than our sun and yet if removed to a distance 100 times greater than that of Sirius (estimated on the supposition that Sirius is equal to the sun) this enormous orb would appear no larger than Sirius appears. In fact there are reasons for believing that Sirius is actually the central orb of the galaxy. For the Milky Way seems widest in the constellations of the Eagle the Fox and the Swan†, so that the sun must lie towards these constellations. Hence the central orb must lie in

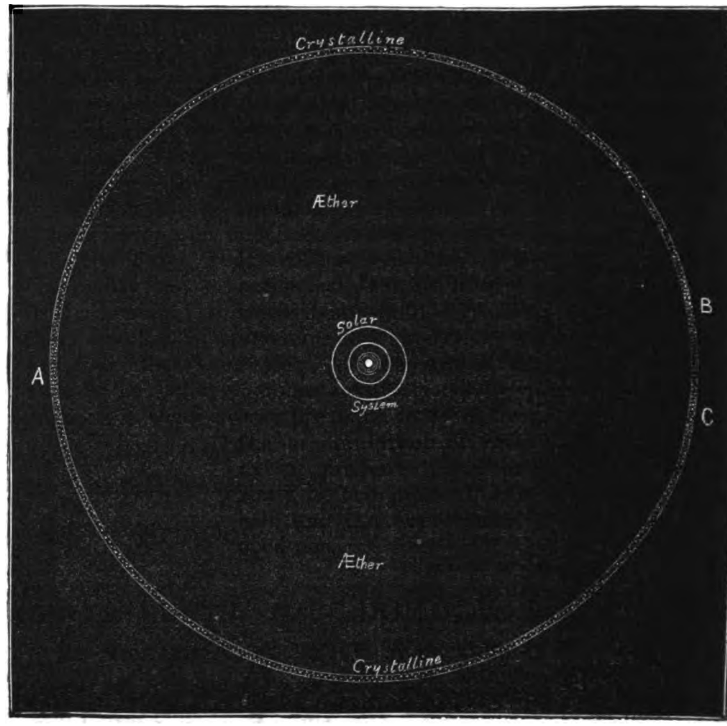


Fig. 1.—Kepler's Idea of the Universe (the Crystalline is shown in section only).

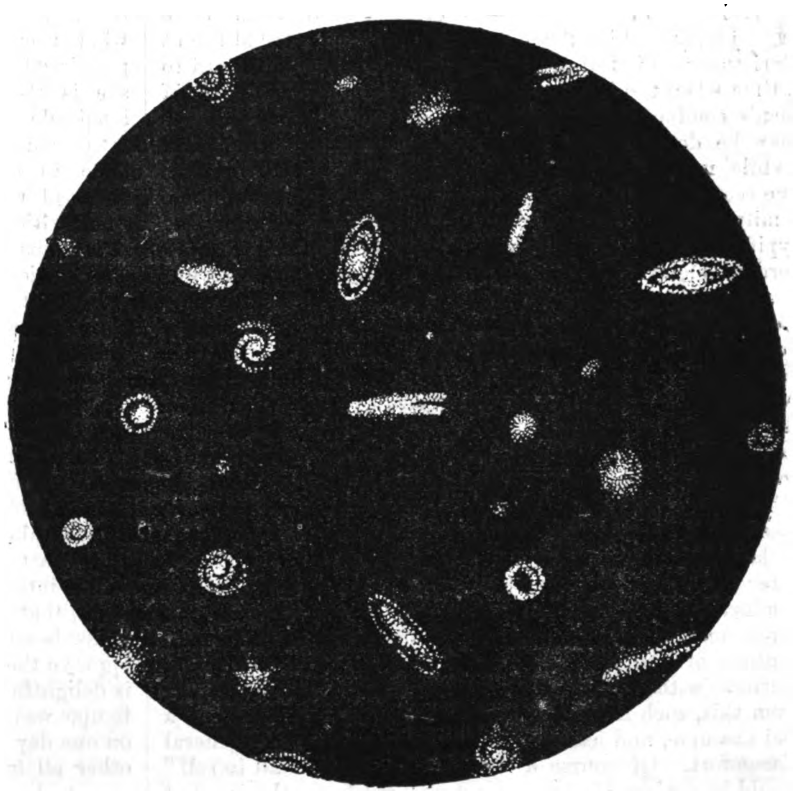


Fig. 2.—Illustrating Wright's Theory of the Galaxy and its Fellow Star Systems.

* Kant wrote 10,000,—which is, however, obviously erroneous.

† This argument fails in the presence of the actual fact—unknown to Kant—that the Milky Way is widest in Scorpio and the neighbouring constellations. Struve remarks well also, that the argument is only sound if the Milky Way is regarded as of the nature of a ring,—a theory which Kant does not appear to have entertained.

the part of the heavens directly opposite to these constellations. But Sirius is so placed, and as this star is the brightest in the heavens, we may infer that it is the central orb and rules by virtue of its superior size the motions of the rest. It is true that Sirius does not appear to lie in the level of the Milky Way; but this is readily explained by supposing that the sun is removed to some distance from this level.

(5.) The oval nebulæ which the telescope does not resolve into stars, are systems resembling the Milky Way, not only in general features, but also (as their elongated form indicates) in having a mean plane towards which their stars congregate.

(6.) The same sort of relation probably exists amongst the different Milky Ways which is recognised among the different suns of the Milky Way. These other Milky Ways are members of a new system of a yet higher order. We trace here the first terms of a series of worlds and systems, and these first terms of an infinite series enable us to infer the nature of the rest of the series.

It is scarcely necessary to point out how closely in many respects the ideas of Kant resemble those of Wright. It may, indeed, be said that Kant's theory only differs from Wright's in passing beyond the limits of observed facts. All Wright's theory, excepting his opinion respecting a central sun, was based upon observation, though in saying this I do not assert that all his inferences were just. Kant's conceptions respecting an infinite progression of systems, or rather of orders of systems, though undoubtedly imposing, cannot be regarded as involving any real addition to our knowledge.

(To be continued.)

CHRISTMAS HAPPINESS.

By Thomas Foster.

THE tide of happiness—as distinguished from mere jollity—which flows over a part of our social life at Christmastide is full of hope to those who look forward to a time when the increase of happiness will be the guide of men's conduct at all times and seasons. One sees what may be done to make happiness greater when even for awhile men definitely set that object before them. And one sees also how that end is to be achieved. A sensible family group on Christmas day or at New Year's tide typifies not inaptly the human family as it might be if discordant elements were eliminated, whether by processes of evolution, by the influence of example, or (less hopefully, I fear) by the inculcation of sound and healthy philosophy. In such a group the object aimed at by all is that, for the time at any rate, all shall be as happy as possible. And where this object is definitely held in view the way of reaching it is not missed. No one seems to think he is going to add best to the happiness of those around him by subtracting from his own. He feels, even if he does not think it in any conscious way, that it is an essential part of the happiness of the rest that he also shall be happy, and something tells him (instinct or reason, who cares to decide?) that to be happy and cheerful he must be comfortable and contented. Directly as well as indirectly carelessness in this respect will diminish the comfort and content of the rest. There is plenty of room for self-sacrifice without wilful rejection of comfort; but apart from this, such neglect of self is annoying to others, sets a bad example, and leads presently to ill-temper and general discomfort. Of course a "wretch concentred all in self" would be a miserable presence at a Christmas gathering, but next to him in unpleasantness would be one who would insist on being uncomfortable, who would not take his share of pleasant things but looking askance at these as selfish indulgences for him, would make others feel that they also ought to go without their "cakes and ale" (by which, Oh sad Blue-ribbonist, I speak as in a figure, not necessarily including strong drinks, but typifying pleasant things in general). This would be bad enough even if the self-sacrificing one remained bright and cheerful to the end of the feast. But he would not. The same spirit which would make him refrain from enjoying pleasant things would probably from the beginning, but certainly ere long, make him refrain from saying or doing them.

But fortunately every sensible person at Christmas-time knows and feels all this. Even those who are untroubled by any fear of self-indulgence, whether in ways or in will, feel that their only course is to put aside their selfishness for the time,—and often enough (though Dickens rather exaggerated matters in this direction) they are surprised to find how much happiness to self may be derived from attention to the happiness of others.

Herein we see illustrated the doctrine for which the great teacher of our age has been so much abused, a doctrine readily pictured as mere selfishness, but in reality teaching that pure selfishness and pure self-abnegation alike diminish the happiness both of self and others, and inculcating care to increase the happiness of others, with due regard for the well-being of self,—for this reason, if for no other, that an ill-cared for self is a nuisance to all.

These points alone remain now to be considered in my discussion of the "Morality of Happiness,"—in two chapters on The Care of Self and Regard for Others.

THE NEW YEAR.

EVEN in these advanced days men are but children. Else why should the return of the earth to a particular part of her path round the sun set them counting up what they have done since she was last there, or expressing hopes or wishes about what may happen before she is there again? Why should the earth's reaching longitude 100½ degrees from the first point of Aries set grown men at work either reckoning what they have done in the past, or else making fresh pavement for the road to Hades? We have all been acting as though some critical time had passed. Our papers have been full of summaries for 1883 which nobody has read, and of anticipations for 1884 in which no one believes. We have had the events of 1883 in the daily press, the art of 1883, the science of 1883, the literature of 1883 in *Academy*, *Athenæum*, art journals, and so forth; our *Graphics* and other illustrated papers have signalised this particular season with the love tales and ghost stories supposed to be appropriate to the dying out of the old year and the coming in of the new one; *Truth* has turned from customary self-contemplation in its mirror, to publish its Essence of Envy for 1883; our poorer public has gone through that British begging feat which makes us all so proud of our people on Boxing day, —and last, but by no means least, every one seems to have fallen into the pleasing belief (a very profitable one to some) that by clogging postal traffic with tons of painted cards, bearing for the most part idiotic mottoes, we best approve the love we bear to friends and kinsfolk. All this is delightful of course; but why just now? In what particular way does January 1 differ from December 31, that on one day we should all be looking backwards and on the other all looking forward,—and that this odd change of aspect should be made the occasion for so much that is childish and unmeaning?

GAMBLING SUPERSTITIONS.*

By RICHARD A. PROCTOR.

IT might be supposed that those who are most familiar with the actual results which present themselves in long series of chance-games would form the most correct views respecting the conditions on which such results depend—would be, in fact, freest from all superstitious ideas respecting chance or luck. The gambler who sees every system—his own infallible system included—foiled by the run of events, who witnesses the discomfiture of one gamester after another that for a time had seemed irresistibly lucky, and who can number by the hundred those who have been ruined by the love of play, might be expected to recognise the futility of all attempts to anticipate the results of chance combinations. It is, however, but too well known that the reverse is the case. The more familiar a man becomes with the multitude of such combinations, the more confidently he believes in the possibility of foretelling—not, indeed, any special event, but the general run of several approaching events. There has never been a successful gambler who has not believed that his success (temporary though such success ever is, where games of pure chance are concerned) has been the result of skilful conduct on his own part; and there has never been a ruined gambler (though ruined gamblers are to be counted by thousands) who has not believed that when ruin overtook him he was on the very point of mastering the secret of success. It is this fatal confidence which gives to gambling its power of fascinating the lucky as well as the unlucky. The winner continues to tempt fortune, believing all the while that he is exerting some special aptitude for games of chance, until the inevitable change of luck arrives; and thereafter he continues to play, because he believes that his luck has only deserted him for a time, and must presently return. The unlucky gambler, on the contrary, regards his losses as sacrifices to ensure the ultimate success of his "system," and even when he has lost his all, continues firm in the belief that had he had more money to sacrifice he could have bound fortune to his side for ever.

I propose to consider some of the most common gambling superstitions—noting, at the same time, that like superstitions prevail respecting chance events (or what is called fortune) even among those who never gamble.

Houdin, in his interesting book, "Les Tricheries des Grecs devoilées," has given some amusing instances of the fruits of long gaming experience. "They are presented," says Steinmetz, from whose work, "The Gaming Table," I quote them, "as the axioms of a professional gambler and cheat." Thus we might expect that, however unsatisfactory to men of honest mind, they would at least savour of a certain sort of wisdom. Yet these axioms, the fruit of long study directed by self-interest, are all utterly untrustworthy.

"Every game of chance," says this authority, "presents two kinds of chances that are very distinct—namely, those relating to the person interested, that is the player; and those inherent in the combinations of the game." That is, we are to distinguish between the chances proper to the game, and those depending on the luck of the player. Proceeding to consider the chances proper to the game itself, our friendly cheat sums them all up in two rules. First: "Though chance can bring into the game all possible combinations, there are nevertheless certain limits at which it seems to stop: such, for instance, as a certain number turning up ten times in succession at roulette; this is possible, but it has never happened." Secondly: "In a game of chance, the oftener the same combination has occurred in succession, the nearer we are to the certainty that it will not recur at the next cast or turn up. This is the most elementary of the theories on probabilities; it is termed the *maturity of the chances*" (and he might have added that the belief in this elementary theory had ruined thousands). "Hence," he proceeds, "a player must come to the table not only 'in luck,' but he must not risk his money except at the instant prescribed by the rules of the maturity of the chances." Then follow the precepts for personal conduct:—"For gaming prefer roulette, because it presents several ways of staking your money—which permits the study of several. A player should approach the gaming-table perfectly calm and cool—just as a merchant or tradesman in treaty about any affair. If he gets into a passion it is all over with prudence, all over with good luck—for the demon of bad luck invariably pursues a passionate player. Every man who finds a pleasure in playing runs the risk of losing.* A prudent player, before undertaking anything, should put himself to the test to discover if he is 'in vein' or in luck. In all doubt he should abstain. There are several persons who are constantly pursued by bad luck: to such I say—*never play*. Stubbornness at play is ruin. Remember that Fortune does not like people to be overjoyed at her favours, and that she prepares bitter deceptions for the imprudent who are intoxicated by success. Lastly, before risking your money at play, study your 'vein,' and the different probabilities of the game—termed, as aforesaid, the maturity of the chances."

Before proceeding to exhibit the fallacy of the principles here enunciated—principles which have worked incalculable mischief—it may be well for us to sketch the history of the scamp who enunciated them—so far, at least, as his gambling successes are concerned. His first meeting with Houdin took place at a subscription ball, where he managed to fleece Houdin "and others to a considerable amount, contriving a dexterous escape when detected. Houdin afterwards fell in with him at Spa, where he found the gambler in the greatest poverty, and lent him a small sum—to practise his grand theories." This sum the gambler lost, and Houdin advised him "to take up a less dangerous occupation." It was on this occasion, it would seem, that the gambler revealed to Houdin the particulars recorded in his book. "A year afterwards Houdin unexpectedly fell in with him again; but this time the fellow was transformed into what is called a '*demi-millionaire*,' having succeeded to a large fortune on the death of his brother, who died intestate. According to Houdin, the following was the man's declaration at the auspicious meeting:—'I have,' he said, 'completely renounced gaming; I am rich enough; and care no longer for fortune. And yet,' he added proudly, 'if I now cared for the thing, how I could break those bloated banks in their pride, and what a glorious vengeance I could take of bad luck and its inflexible agents! But my heart is too full of my happiness to allow the smallest place for the desire of vengeance.'" Three years later he died; and Houdin informs us that he

* But a score or so of copies of the "Borderland of Science" remain now in stock, and it is not proposed to reprint that work, at least in its present form. But as the essays on "Gambling Superstitions," "Coincidences and Superstitions," and "Ghosts and Goblins," relate to subjects frequently discussed by correspondents, these will here be reprinted, as occasion serves. They supplement what has been said on these and kindred subjects in former numbers of KNOWLEDGE.

* This naïve admission would appear, as we shall presently see, to have been the fruit of genuine experience on our gambler's part: it only requires that, for the words "runs the risk," we should read "incurs the certainty" to be incontrovertible.

left the whole of his fortune to various charitable institutions, his career after his acquisition of wealth going far to demonstrate the justice of Becky Sharp's theory, that it is easy to be honest on five thousand a year.

It is remarkable that the principles enunciated above are not merely erroneous, but self-contradictory. Yet it is to be noticed that though they are presented as the outcome of a life of gambling experiences, they are in reality entertained by all gamblers, however limited their experience, as well as by many who are only prevented by the lack of opportunity from entering the dangerous path which has led so many to ruin. These contradictory superstitions may be called severally,—the gambler's belief in his own good luck, and his faith in the turn of luck. When he is considering his own fortune he does not hesitate to believe that on the whole the Fates will favour him, though this belief implies in reality the *persistence* of favourable conditions. On the contrary, when he is considering the fortunes of others who are successful in their play against him, he does not doubt that their good luck will presently desert them, that is, he believes in the *non-persistence* of favourable conditions in their case.

(*To be continued.*)

Reviews.

CENTRIFUGAL FORCE.*

By Richard A. Proctor.

I FANCY that the particular school of paradoxists believing in a flat earth is for a while tolerably well silenced. But there are other paradoxes not quite so preposterous which still remain to show how certain scientific matters, carelessly explained, cause doubt and difficulty in some minds, and excite in others the hope of acquiring fame before which the fame of Copernicus, Galileo, Kepler, and Newton shall wax pale and dim. Among the matters thus misunderstood, centrifugal force holds a leading place. The idea conveyed by the account of planetary motions given in certain old-fashioned books on astronomy, and repeated by some modern writers who ought to know better, is that a planet moves around the sun under the combined action of two forces—one the centripetal force tending towards the sun, the other the centrifugal force tending from the sun. The student imagines that in some unexplained way the centrifugal force waxes and wanes as the centripetal force does, so that the two remain constantly balanced; and presently finds himself perplexed by the thought that there is no obvious reason why the centrifugal force should thus vary. Nay, inquiring a little further, he begins to wonder what the centrifugal force is, whence it comes, why it acts always directly against the centripetal force. Further, since the planets in their courses are sometimes much nearer than at others to the sun, it seems obvious to superficial inquiry that having drawn them closer, and when they are closer having a greater pull on them, the sun should draw them in to his very surface. If in the game known as "the tug of war," one side had gained such an advantage over the other as to have already pulled it part of the way over the centre mark, and if, as the result of such partial success, the energy of their pull were increased, it would seem as though the contest were already decided: with part of its work already done, and with a gain of energy for the rest of the work, the side thus partially successful must presently complete its victory.

A paradoxist who not long since found foothold with his fardel of fallacies in the respectable pages of *Fraser's Magazine*, has recently been moved to publish a treatise calling attention to these difficulties. "What a scene of scientific confusion," he says, "is here presented to our view! When once gravity begins to overcome a rival force, its career of conquest cannot be arrested, except by the arrival and intervention of a third independent power, and the introduction of this third power is not properly and scientifically accounted for under the old system."

The truth really is, that the expression "centrifugal force," like the expression "force of inertia," belongs to an old system of nomenclature which implies more (according to our present usage) than it was really intended to signify. When Newton spoke of *Vis Inertiæ*, he did not mean any form of what we understand by "force." For by force we mean whatever causes a body to change its condition of rest or motion, whereas Newton's force of inertia meant that condition of rest or motion on which force acts to cause change. And in like manner with Newton's *Vis Centrifuga* or the Centrifugal Force of the olden writers. It is no force at all in our modern sense. It is not even properly called centrifugal, seeing that in many cases the tendency in question acts to bring a body nearer to the centre. For instance, when a planet is at the point midway in its elliptical path between the remotest and nearest points (meaning from the sun's centre) and is passing towards the latter, if suddenly the centrifugal force were to cease, the body would continue to move in a straight line, and for the first part of its motion in that straight line would draw nearer to the sun. In the case of a comet this would be still more marked. A comet of long period, returning towards the sun, has, throughout its course, up to the very point of nearest approach, a direction of motion which, quite apart from any pull by the sun, tends to carry it sunwards, and if the comet were left to itself when half-way back it would move for years in a course drawing it nearer to the sun.

If students of this subject would remember that instead of the centrifugal force which perplexed them in the old works, they have, in reality, to deal only with a particular case of what was of old called *Vis Inertiæ*, or force of inertness—no force at all, but a tendency to go on (if the body is already in motion) with unchanging velocity in a straight line, or to remain at rest if already at rest—the difficulty which Mr. Newton Crosland and others have found so perplexing will disappear. There is no contest between two rival forces (in the modern sense) when a planet pursues its constantly-curving path under the action of solar gravity. If the sun ceased to act, the planet would simply move on in the direction in which it was travelling at the moment when the sun's centrifugal force ceased its action. Those who imagine, again, that in such matters as the elliptic paths of planets and so forth the theory of gravitation encounters constant difficulties, and that, as Mr. Crosland suggests, these have to be met by shuffling and evasion, would do well to learn that the theory of gravity was established solely through its agreement with facts. Granted a force of given magnitude, residing in the sun's mass, and a planet moving at a given distance in a given direction (at the moment) and with a given velocity, the steady working of that force of attraction, according to the law of gravitation, on the planet, as it moves onwards, gives the observed elliptical orbit. Difficulties arose because of the disturbing influences of the planets on each other, of the earth on the moon and the

* This article, reprinted from the *Newcastle Weekly Chronicle*, may be regarded as a review of the "New Principia," by Mr. Newton Crosland. (Trübner & Co., London.)

moon on the earth, because, again, of the peculiarities of shape in the different planets, by which, though their orbital motion is not affected, their pose is disturbed, so that they reel like gigantic tops. But these difficulties have brought about some of the greatest triumphs which the Newtonian system has attained. Even where they have not been fully and successfully encountered, they in no sense affect the principles of Newtonian astronomy, any more than the practical difficulties which a surveyor may encounter (and by which he may perchance be foiled) invalidate the mathematical principles on which the surveyor's work proceeds.

THE FACE OF THE SKY.

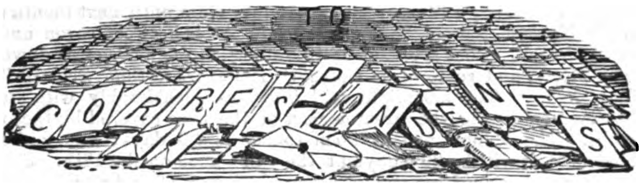
FROM JANUARY 4, TO JANUARY 18.

BY F.R.A.S.

SPOTS, &c., continue to diversify the sun's disc, which should hence be examined on every cloudless day. Map I. of the "Stars in their Seasons" represents the present aspect of the night sky. Minima of that remarkable variable star Algol in Perseus (same map) will occur at 1h. 35m. a.m., on the 11th; 10h. 24m. p.m. on the 13th; and 7h. 13m. p.m. on the 16th. Mercury is, for all practical purposes, invisible. Venus is very badly placed for the observer, but, as she sets after the sun, may possibly be caught with the naked eye, close to the horizon and over the S.W. by W. point of it. Seen in a telescope she is still gibbous, with a diameter of less than 12″, so that she is an uninteresting object. Mars is becoming more favourably placed for the observer every night. He rises a little before 7 o'clock to-night and very soon after 5 by the 24th. His apparent diameter increases from 15″·1 to-day to 16″·6 a fortnight hence, and his outline is very nearly circular. The polar snow and principal markings on his disc may now be very well seen with adequate optical power. Starting from a point to the right of the "Sickle," formed by α η γ and ζ Leonis ("The Stars in their Seasons," Map IV.) he travels backwards into Cancer. As Jupiter — speaking technically — comes into opposition to the sun at 3 a.m. on the 20th, he is now about as well placed for the observer as he possibly can be. Presenting a diameter of about 43″, he is a truly noble object even in a comparatively small telescope; and every increase of optical power reveals more and more curious detail on his surface. His path is a retrograde one in Cancer, to the right of the Præsepe (same map). The phenomena of his satellites observable before 1 a.m. are tolerably numerous during the next fourteen days. Beginning with to-night (4th), the shadow of Satellite II. will pass off Jupiter's disc at 8h. 19m., as will Satellite II. itself at 9h. 6m. p.m. On the 7th, Satellite I. will disappear in eclipse at 11h. 21m. 14s. On January 8 the shadow of Satellite I. will enter into Jupiter's face at 8h. 39m., followed by the satellite which casts it at 8h. 56m. p.m. Then at 10h. 31m. the ingress of the shadow of Satellite III. will occur. The shadow of Satellite I. will pass off at 10h. 59m., as will Satellite I. itself at 11h. 16m.; and at 11h. 37m. Satellite III. will follow its shadow on to the planet. On the 9th, Satellite I. will reappear from occultation at 8h. 23m., p.m. On the night of the 11th of January the shadow of Satellite II. will begin its passage across Jupiter's face at 8 o'clock, followed by Satellite II. itself at 8h. 25m. The shadow will leave the planet's opposite limb at 10h. 54m., and finally the Satellite will do the same thing at 11h. 19m. p.m. On the evening of the 13th, Satellite II. will reappear from occultation at 6h. 21m.; as will Satellite IV. the next night (the 14th) at 9h. 22m. The shadow of Satellite I. will enter on to the planet's limb at 10h. 33m. on the 15th, as will the Satellite which casts it six minutes later. Then at 12h. 53m. the shadow will leave Jupiter's face, followed by Satellite I. itself at 12h. 59m. p.m. On the 16th, Satellite I. will be eclipsed at 7h. 43m. 48s. p.m., will pass through the shadow behind Jupiter, and reappear from occultation at 10h. 6m. The shadow of this same Satellite (I.) will quit Jupiter's disc at 7h. 22m., on the 17th, followed by the Satellite only three minutes afterwards. On the 18th, the ingress of the shadow of Satellite II. will happen at 10h. 35m. p.m.; as will that of the Satellite at 10h. 38m. The egress of both will not happen until between 1 and 2 o'clock the next morning. Saturn may still be well seen through all the most convenient hours of the night; but, of course, the nearer he is to the meridian at the time of observation, the better. There is no sensible difference in the opening of his rings. He is still situated to the right of ϵ Tauri ("The Stars in their Seasons," Map I.). Uranus will scarcely come into view until next month; while Neptune should be looked for in Aries between seven and eight o'clock. Pons's Comet is travelling through Pegasus down into Pisces, and so towards Cetus ("The Stars in their Seasons," Map XII.). The moonlight will interfere considerably with its observation during the greater part of the time covered by these notes. The Moon's age at noon to-day is 6·0 days; and, quite obviously, it will be 20·0 days at the same hour on the 18th. She is fairly well placed for the observer during the first two-thirds of this time. Several occultations of stars will happen during our specified period. On Jan. 6, the sixth mag. star 54 Ceti will disappear at the Moon's dark limb at 8h. 15m. p.m., at an angle of 157° from her vertex, to reappear at her bright limb at 9h. 18m. p.m., at a vertical angle of 291°. Two sixth magnitude stars will be occulted on the 8th. The first is B. A. C. 1119, which will disappear at the dark limb at 3h. 44m. p.m., at a vertical angle of 73°, and reappear at her bright limb, at an angle of 239° from her vertex at 4 h. 44 m. p.m. The second star occulted is B. A. C. 1206, which will disappear at the dark limb at 10 h. 50 m. p.m. at an angle from the Moon's vertex of 160°, reappearing at the bright limb at an angle of 280 from her vertex at 11h. 48m. p.m. On the 11th, 26 Geminorum, a 5½ magnitude star, will disappear at 3h. 37m. p.m. at the dark limb, at a vertical angle of 58°, reappearing from behind the bright limb at 4h. 26m. p.m. at a vertical angle of 228°. Before the Moon rises on the 13th she will have occulted the 6th magnitude star A^1 Cancri; this will reappear at her dark limb at 6h. 12 m. p.m. at an angle of 158° from her vertex. Afterwards, at 7 h. 20 m. p.m., A^2 Cancri, another star of the 6th magnitude, will disappear at the bright limb of the Moon at an angle of 5° from her vertex, to reappear at her dark limb at 8h. 9m. p.m., at a vertical angle of 253°. Thirdly, on the same night 60 Cancri, a 6th mag. star, will disappear at the bright limb at 11h. 55m. p.m., at an angle of 41°. from the vertex of the moon. It will not reappear at the dark limb until eight minutes past 1 o'clock on the following morning, its emersion occurring at a vertical angle of 259°. At noon to-day the moon is a little to the S.E. of λ Piscium. She is travelling through Pisces until between 7 and 8 p.m. on the 6th, when she enters Aries. It occupies her until 11 a.m. on the 8th to cross this constellation; and at the hour last named she passes into Taurus. She is traversing Taurus until 9 p.m. on the 10th, when she quits it for the extreme northern part of Orion, her passage over which takes her twelve hours to accomplish, after the lapse of which time she emerges into Gemini. Across this she travels until 10 p.m. on the 12th, when she enters Cancer, which she quits at noon on the 14th for Leo. At 1 p.m. on the 15th she descends into Sextans, re-emerging into Leo between 5 and 6 a.m. on the 16th. At 8 a.m. on the 17th she quits Leo for Virgo. She is still in Virgo when our notes terminate.

A COMPLIMENT TO THE BRUSH ELECTRIC COMPANY.—The Emperor of Brazil has presented to this company a large artificial flowering plant, made entirely of the feathers of South American birds, as a token of his gratification at the completing of a very successful system of electric lighting in the city of Campos, Brazil.—*Engineering.*

DANGERS OF OVERHEAD WIRES.—The following is taken from the *New York Nation*:—"After the experience in Fifth Avenue, on Monday evening, there is no necessity for further expert testimony concerning the danger from electric lighting wires. A runaway in the Avenue broke down one of the electric lamp-poles, and trailed the wire upon the pavement. A passing horse stepped upon the wire; there was a flash of purple light, a report like a pistol-shot, all the lights in the Avenue went out, and the horse fell dead. A moment later another horse stepped upon the wire and dropped dead, like the first. The Avenue was immediately closed to travellers of all kinds. Of course, if a person had stepped upon the wire, he would have shared the fate of the horses. It was a stormy night, and there were few people upon the Avenue, or the consequences might have been much more serious. A wire which kills instantly every person who treads upon it, would be a deadly thing for a crowd to surge over. Clearly the only safe place for these wires is under ground, and the sooner they are put there the better."



"Let Knowledge grow from more to more."—ALFRED TENNYSON.

Only a small proportion of Letters received can possibly be inserted. Correspondents must not be offended, therefore, should their letters not appear.

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VARIABLE AND RED STARS IN CYGNUS—DOUBLE STARS IN TAURUS AND ORION—SATURN—PONS-BROOKS' COMET—THE REMARKABLE SUNSETS.

[1077]—The constellation Cygnus is conveniently situated for telescopic observation during the early winter months, and it affords a rich treat to sweep with a low power its magnificent fields, the milky way, with its star-dust and innumerable specks of light, forming a beautiful background to the lucid stars of this asterism, and presenting some wonderful configurations, with which the eye is never wearied. Cygnus is noted for its variable stars, and also so-called new stars; the first observed in 1600 by Kepler and others; the second, in 1686, by Kirch, in the neck of the Swan. More recently, Schmidt's Nova, discovered 1876, Nov. 24, when it blazed to 3 magnitude. In October, 1877, it had faded to 10½ magnitude. In October, 1878, still further down to 14 magnitude. In October, 1881, it had dwindled to 16 magnitude, when I last examined it, and is now only visible in the largest telescopes.

Mr. Birmingham, of Millbrook, Tuam, well known as the discoverer of many red and variable stars, found 1881, May 22, a deep red or crimson star in R. A. 20h. 37m.; D + 47° 42′ ±, or about 2° 51′·7 north of α Cygni. It was then about 9 magnitude, being near one of its maxima. It appears to change from about 8½ to below 12 magnitude. Its period is probably over twelve months; but I am not aware whether this has been yet determined. Its colour is very intense, being obvious when even near its minimum. There is a minute *comes* about 60″ north of it.

Observers interested in unequal double stars will find one about 3′ preceding the above variable star. The pair lie about 7′ north of Σ 2707 (a triple star, or rather quintuple, for there are two 11-12 magnitude stars *n.f.* which make up a peculiar curved group), which point to the pair to which I refer. The magnitudes are about 6 and 12; P 200° ±; D 7″ ±. The principal star is preceded by a string of 10 to 12 magnitude stars forming a sinuous curve winding round to the north. I believe this pair will be found to be a test for a 5-inch telescope.

There is another unequal double star in Taurus in R. A. 4h. 24m. 40s.; D + 14° 25′ 30″. It is marked "A" in diagram (Fig. 1). The star "C" has also a faint *comes n.f.* at 20″ ±. The *comes* to "A" is closer, being 12″ ± at 70° ± P angle. It lies 1° 13′ south of, and about 3m. following θ' Tauri. A 5-inch telescope should show the comites of both A and C.

Those observers with telescopes of 8 inches to 10 inches aperture, who would wish to test the light-grasping power of their instruments, might try a faint double star which I picked up in November, 1878, in the field with ι (Iota) Orionis in R.A. 5h. 30m.; D − 5° 59′. It was measured by Dr. Copeland on January 15, 1879, mag. 11·2 and 13·4; P. 288°·4; D. 10″·8. By some mistake these measures were appended in Webb's "Cel. Objects," 4th Edition, p. 361, to OΣ 98 (14, alias i), instead of following Iota Orionis (Σ 752) on p. 359, where the pair is only mentioned. The diagram of the field (Fig. 2) will assist in identifying the double which is marked "A," the triple star Iota being south of it, with a 9½ magnitude star between them, and north of it about 30″ is a 9¾ magnitude star.

The great northern declination of Saturn during the present opposition renders it favourable for observation. On Dec. 9, at 10.30 p.m., the definition was superb, and the image of the planet with the 4·3 inch refractor, and a power of 113, was like a line engraving. The "crape ring" was visible in the ansæ, with this low power, in the moonlight. The boundary between the bright equatorial region and the darker southern portion of the ball was distinctly marked by a dark *streak*, which appeared to be *beaded*, but my intention to employ a higher power was interrupted by having to turn to another object.

The Pons-Brooks comet has been visible to the naked eye for the past three weeks, and has now travelled into Cygnus. When I first saw it with the 4·3-inch refractor on Sept. 10, it presented a very faint and diffused nebulous mass without a trace of nucleus. It was not found in the low-power field of the place indicated in the first rough ephemeris, and might readily have been overlooked at that time, in the light of the young moon, with even ten inches of aperture. On Sept. 22 and 25 it showed signs of condensation towards the centre. On Oct. 1 it had brightened, so that it was easily seen with a 2⅜-inch O.G. of 36 inches focus, and a terrestrial eyepiece of about 50 power. During the middle of November an incipient tail developed on the *n.f.* side, though very narrow and faint compared with the large coma surrounding the nucleus, but the nebulosity of the coma appeared to extend farther in the direction of P. angle 150°, the tail lying in about 60° P. angle. Any apparent fluctuations in brightness during the end of September I attributed to slight haze, invisible to the eye, though observations elsewhere would seem to point to other primary causes.

The remarkable sunsets and sunrises which have been seen over the greater part of the earth (though, by the way, I have not any mention of it from the United States) were first noticed here on Nov. 25, and on several occasions since when the sun set in the ordinary clear sky. The upper glow seemed to be from very high strata, lying fair above the level of the highest cirrus clouds, and if it be cosmic or volcanic dust which reflects this glow it must be far above the ordinary height of snow or rain clouds; and, therefore, the experiment suggested by Mr. A. C. Ranyard in your paper would scarcely be conclusive, even if undertaken with all the care with which it appears to have bestowed upon it in the interesting observations of Captain Noble and Mr. M. Mattieu Williams.

I would rather incline to accept the theory that the dust is terrestrial, and not cosmical, because in the latter case its effects would most likely be observed simultaneously over the earth, whilst dust from the tremendous eruption of Krakatoa would take some time to travel to the opposite hemisphere. I refer to the *finer* dust traversing the region *above* the ordinary surface currents, and not to the heavier and grosser matter which appears to have been carried in one instance 970 miles in about forty hours, in a westerly direction; and in another case about 1,050 miles in a south-south-easterly direction in about seventy hours.

Might not the enormous quantity of mixed gases of a light specific gravity, which must have been ejected to a tremendous height, and carried with them a great mass of the finer impalpable dust, have floated, as it were, with some higher universal currents (of which we know very little), and carried slowly above the surface winds, though not necessarily in a uniform stratum over the earth?

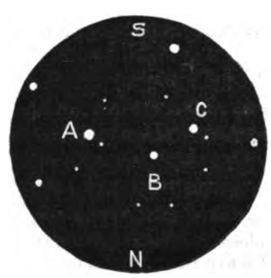


Fig. 1.

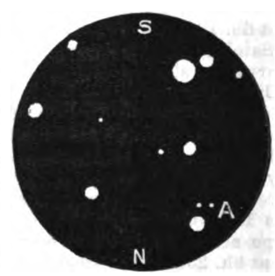


Fig. 2.

On Dec. 16, the crimson glow had not disappeared here two hours after sunset, no clouds being visible. Venus shone out beautifully low down in the horizon from 4.15 to 4.45 p.m. through the glow, and appeared of a bluish tinge by contrast. ISAAC W. WARD.

THE AFTERGLOW.

[1078]—Did any one of my fellow-readers of KNOWLEDGE witness the sunset on Christmas Day? I ask, because here, in Sussex, the dense pall of fog which obscured everything turned of a distinct crimson at and after the time at which the visible sun would have been setting, and long after dark the fog was markedly red. This last effect was very weird and uncanny. I can only imagine that the afterglow must have been vivid in the extreme to have penetrated such dense mist as blotted out our Christmas landscape here. WILLIAM NOBLE.

Forest Lodge, Maresfield, Uckfield, Dec. 28, 1883.

THE COLLECTION OF DUST FROM SNOW.

[1079]—I hope that if another fall of snow should occur, several of your readers will collect and examine the dust brought down by the snow-flakes. Clean snow should be collected as soon as possible after its fall. No doubt the first flakes that come down bring with them more atmospheric particles than those which fall subsequently; but as these fall upon the ground, it will be well to leave them and collect only the surface snow, which may be cut off with a clean piece of glass. The snow should be collected in a bath or large earthenware pan, which for the sake of precaution should be first rinsed out with water. As much snow as possible should be collected, as experimenters will probably be disappointed with the amount of water they obtain, and also, if the collection is carefully made, with the small quantity of solid matter which it contains.

The best way of collecting the solid particles is by allowing them to settle in a tall vessel. The surface water should then be carefully drawn off, and the remainder should again be allowed to settle in a smaller vessel, such as a test-tube. Ultimately, the remaining water should be evaporated at a low heat, so as to avoid boiling. This method of collecting the particles from the water is preferable to filtering, even with Swedish filter-paper—and blowing away the ashes of the paper after burning, as changes in the dust due to heat are thus avoided. I am anxious to examine the matter brought down by snow, so as to eliminate with greater certainty the terrestrial matter collected from the lower air. I hope shortly to give an account of the snow residues and Krakatoa dust which various friends and correspondents have kindly sent me.

Lincoln's Inn. A. C. RANYARD.

RED SKIES IN AMERICA.

[1080]—I am glad to see the article upon "Extraordinary Sunsets" in the last number of KNOWLEDGE, just received. It seems strange to me, however, that no accounts of the phenomenon have yet reached you from this side; but I suppose you will have plenty of them in due time. Seeing the request of Mr. Ranyard for particulars, I thought I would send you a few points, and hope that some of you will be able to explain this knotty subject, as it seems to be a puzzle to our scientists upon this side. My attention was first called to the matter about the latter part of November—somewhere about the 20th, I think. I was in the office, when one of the clerks came in and said that there "was a tremendous fire somewhere up town." I at once went out, and saw the whole western heavens illuminated. The sight was such a strange one that we went on top of the building, and it was at once evident that it was no fire. As near as I can recollect the "red glare" extended for upwards of 40° from the horizon, and for 60° or so along it. All the large buildings, steeples, &c., of the city stood out clear and distinct upon the bright background of the sky, as if lit up by some immense conflagration. This was about six o'clock in the evening. A little before seven o'clock the phenomenon had pretty well disappeared. People in the streets were watching it, and wondering what was the matter in the sky.

One old "darkie" whom I first asked upon coming out of the office where the fire was, answered: "Dat's no fire, mister, dat's de elements; de world must be coming to an end." The general impression among the masses seemed to be that something was wrong in the sky.

I have not much of an eye for colour, but to me it appeared fiery red, without any green whatever. The next day I saw in the papers that one of the fire departments in a Northern city had been called out by mistake, having taken this "red glare" to be an immense fire.

This strange light has been hanging around the sun, more or less, ever since, although to me the phenomenon has never been as marked as upon that occasion. The past three or four days have been cloudy, with occasional snow (the first snow of the season), and as it is now clearing up, I shall watch this evening to see if the snow-fall has had any effect upon this strange light, as suggested by Mr. Ranyard.

I enclose one of our papers, giving a general account of the phenomenon, with the various theories advanced here, to account for it.

Can you not take up this matter, and see if a satisfactory explanation cannot be given? It seems difficult to believe it merely atmospheric, as the phenomonon was such a general one.

Baltimore, U.S. W. H. NUMSEN.

LETTERS RECEIVED (SUB-EDITOR).

ROSE.—LATIMER CLARKE.—W. HUME.—F. W. W.—F. C. H.—J. THOMPSON.—T. COOP. Effect due to irradiation.—E. GRAY. Webb's "Celestial Objects."—J. MURRAY. It is so; but Mr. Proctor did not wish to hurt your feelings by saying so.—A LADY MATHEMATICIAN. Thanks; but of course not forwarded to Editor—seems so childish, you know.—RICHARD ROTH.—IGNORS. May be obtained from them, but is not present as alcohol.—D. C. Many thanks on the Editor's behalf, and good wishes heartily reciprocated.—J. C. FLEWETT. SUFFERER wishes to know where and how distilled water can be obtained. COSMOPOLITAN—J. H. DAVIS—J. H. BRIDGER—C. DE G.—SELLS. The Editor's "Universe of Stars," close of last essay.—ELMIN—J. K. N. Mr. Ranyard has promised a paper on this subject.—C. A. E. It is proposed to have some papers on physics (to begin shortly), and that difficulty will be considered among others; but is it clear you can locate a sound, when one ear closed, and sound not of known nature and origin?—W. R. No truth whatever in the theory of Mr. Jenkins.

Our Chess Column.

BY MEPHISTO.

THE BOOK OF THE TOURNAMENT.

(Conclusion.)

THE Irregular Opening which, next to the Ruy Lopez, played the most important part in the opening *debuts* of the tournament, is not dealt with in a satisfactory manner by the masters, either in their actual play or in analysis. It is an unsatisfactory feature in the analysis of first rates that they quite ignore the student who wishes to benefit by their experience in the openings. In looking through the notes we only find two general references in regard to the well-known position arising from the development of P to Q4, QB4, Kt3, and K3, with the B on Kt2, and K2, or Q3, Kt on Q, and KB3, and Castles for both players. Zukertort says:—"Early exchanges of the centre Pawns in all close openings of this sort, whether made by the first or second player, are not advantageous. It would be impossible (?) to frame a strict rule on the point—in fact, the delicate handling of Pawns and other developing movements in close openings must entirely depend upon the judgment of the player; but, *cæteris paribus*, the game should be fully developed on both wings before entering on any direct line of action." This is correct enough in a general sense, but we think the defence ought always to limit itself to the position given above, and no other. It is a curious point that in none of Zukertort's games was this position attained. The memorable game between Zukertort and Blackburne was likewise an Irregular opening, but Blackburne placed his Kt on Q2. We take this opportunity to express our disapproval of the various personal references, and between line readings contained in the book. Chess analysis should, as far as possible, stand on its merits. Steinitz comes in for a good deal of this sort of thing. We all remember that Steinitz repeatedly and strongly condemned the move of P to QR3 which Zukertort considered necessary, and played in his match against Blackburne, only to desist on his defeat by the latter player at Berlin. Steinitz likewise pointed out that the defence should adopt the line of play indicated above. Several writers on openings take the same view, but, with more discretion than courage, they do not follow the position up. We hope to treat on this opening in course of time.

On the whole, the book will prove an inexhaustible source of pleasure to every Chess player, and remain a standard work, as far as the games are concerned, for all times. The Editor tells us in his preface that he did not intend to furnish all the games with profound notes. It would be an interesting work to arrange a separate small volume dealing analytically with the openings of the games only.

PROBLEM No. 110.

BY HENRY BRISTOW, p. 382.

POSITION.

White.—K, K6; R's, KB3, K7; B's, KKt4, QKt6; Kt, QKt3; P's, KKt3, Q5, QB2, 4.

Black.—K, K5; R, Q5; B, K sq; Kt, Q8; P's, KKt2, 4, Q3, QB2, 6, Kt2.

Solution—1. R to B sq.

If 1. P takes B. 2. K takes P, mate. If 1. K to K6. 2. K to B5, mate. If 1. R to Q6. 2. B to B5, mate. 1. Kt to B7 or Kt7. 2. R to Ksq. 1. Kt to K6. 2. B to B3. If 1. P to Kt3. 2. K to B3, mate. 1. B moves. 2. K moves, and mates acordingly.

In consequence of the great pressure on our space the "Answers to Correspondents" are unavoidably held over.

Our Whist Column.

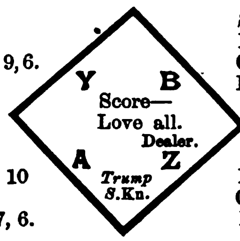
BY "FIVE OF CLUBS."

THE following game is taken from an old number of the *Westminster Papers*. It illustrates the important principle that the first thought of the weaker hands should be to save the game.

THE HANDS.

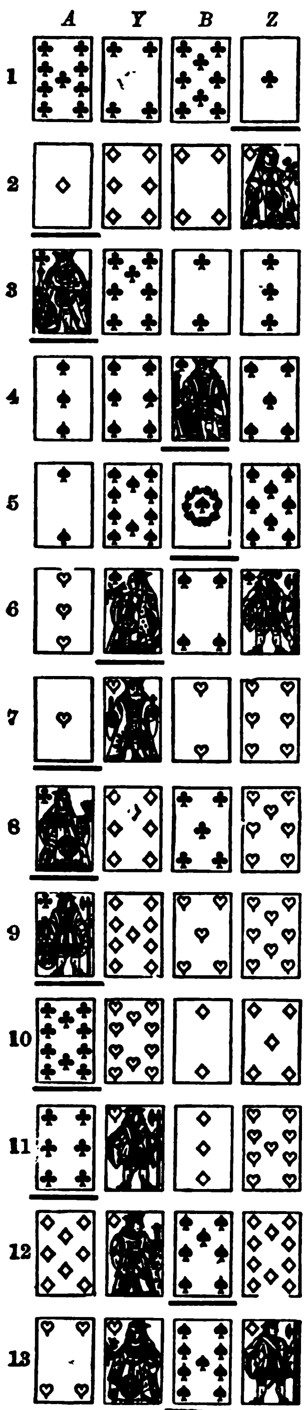
A.
Spades—3, 2.
Hearts—A, 4, 3.
Clubs—K, Q, Kn, 10, 9, 6.
Diamonds—8.

B.
Spades—A, K, 9, 7, 4.
Hearts—5, 2.
Clubs—8, 5, 2.
Diamonds—4, 3, 2.



Y.
Spades—Q, 10, 6.
Hearts—K, Q, Kn, 10
Clubs—7, 4.
Diamonds—K, 9, 7, 6.

Z.
Spades—Kn, 8, 5.
Hearts—9, 8, 7, 6.
Clubs—A, 3.
Diamonds—Q, Kn, 10, 5.



NOTE.—The card underlined wins the trick, and card below leads next round.

NOTES AND INFERENCES.

1. *A* leads from the lowest of his long head sequence, that *B* may play the Ace if he has it, and so get out of the way. *B* begins a signal. It is true he has no strength outside trumps, but *A*'s lead implies great strength in Clubs. As original leader *A* would not lead the best of a short suit, and whether the Nine is the lowest of four-card suit, or the penultimate, *A*'s Club suit must be very strong. *Z* has nothing between Eight and Ace.

2. *B* again begins a signal.

3. The signal is completed. *B* having, for reasons not obvious, signalled with the Eight instead of the Five, *A* might infer that *Z* holds the Five, and that therefore he cannot be led to in Clubs by his partner. *Y* would form the same inference, for it is clear to all that *A* led from a Quint to King, and the Six of Clubs ought not to be with *B*.

6. The two remaining trumps are with *B*. Theoretically *A* should have discarded a Diamond; but it is not a matter of much importance. For if *A* gets a lead he must make all his Clubs, and *B* the remaining tricks with trumps.

7. *Y* fal's into a fatal error. A single trick will save the game, and a single trick in Diamonds is almost certain, while probably two may be made. On the contrary, if *A* holds the Ace of Hearts, *A* and *B* are bound to win.

8 to 13. *A* makes all his Clubs, *B* the two remaining tricks with his trumps, and *A B* make five by tricks.

NOTE ON *Y*'S PLAY AT TRICK 7.—*Y* knows that four winning Clubs are with *A*; two long trumps with *B*; Knave, Ten, and another Diamond (at least) with *Z*. He has reason to think that *B* has no Club, and that (from his discard) *A* either has two Diamonds or none; also, perhaps, that Heart Ace is with *B*, unless *B* signalled with absolutely no strength outside trumps. In any case if *B* holds but one Diamond, leading Diamond King saves the game; whereas if *A* holds Heart Ace leading a Heart is the one (apparently) sole way of losing it. Had *B* no Club, it would still have been wrong to lead Hearts.

THE first two lectures of Dr. Andrew Wilson's course on Anatomy and Physiology were given in Prince's Hall, Piccadilly, London, W., on the evenings of Thursday, Dec. 27, and Monday, Dec. 31. The lectures were well attended. The subject of the first lecture was "The Skeleton;" the second night's subject being "How we Move." Lime light illustrations were used throughout the lectures, the beautiful diagrams and views eliciting the admiration of the large audiences; while the fluency of the lecturer, his well-known facility of illustration, and his complete grasp of his subject, together contributed to make each evening's lecture at once an enjoyable recreation and an intellectual treat of no mean order. In dealing with "The Skeleton," Dr. Wilson built up his discourse largely from the standpoint of comparative anatomy; and showed the community of type which exists between the human and all other vertebrate forms. Spine, skull, trunk, and limbs were successively passed in review and duly illustrated; while the microscopic structure of bone, its growth, and its repair by the periosteum or bone-sheath were noticed at the close of the first lecture. In his second discourse, Dr. Wilson, again starting from lower life, showed how all forms of movement in animals might be regarded as due to the contractility of protoplasm or of its elaborated representatives. Joints and muscles were duly passed in review, and the movements of plants were also noted. The curious little *cilia*, by means of which animalcules move, and which exist in the human lungs, windpipe, and other situations, were described at length. By means of these protoplasm filaments currents in air or in fluids are created, and various movements executed. The remaining lectures by Dr. Wilson fall to be delivered on the evenings of Thursday, Jan. 3, and Saturday, Jan. 5, 1884. No more instructive lectures on subjects connected with the structure and functions of living beings have been delivered, and we hope to hear of the extension of Dr. Wilson's labours to the southern provinces in continuation of his Scottish work.

CONTENTS OF No. 113.

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CONTENTS OF No. 115.

EFFECT OF MARRIAGE ON LIFE.*

IN the year 1867 a statement was made by Dr. Stark, Registrar-General for Scotland, which attracted a good deal of notice. He announced, as the result of his investigations into the relative death-rates of married and unmarried men, that the mortality is very much greater among the latter than among the former. Since then several years' statistics have been published by Drs. Stark, Drysdale, and others, in this and other countries, and they appear to corroborate the doctrine that marriage may be regarded as a sort of life-insurance. It is not with any desire to invalidate the truth of this doctrine that we propose here to point out the great uncertainty of such statistics as these. There are enough arguments in favour of matrimony without introducing false ones. An old proverb tells us of the advisability that no cask should rest on an alien basis, and this is especially the case where the basis of our figurative cask is already wide enough to secure stability. The advocates of marriage will therefore, we trust, look upon us rather as an ally than as a foe if we exhibit, as we think we shall be able to do, the unsubstantial nature of the argument based on such statistics as we have referred to above.

Let us in the first place see what the evidence is on which the argument is founded. A single case will suffice. Take the earliest by combining the results of two years' observations. Dr. Stark thus compares the mortality per thousand of married and unmarried men:—

Ages.	Husbands and Widowers.	Unmarried.
20 to 25	6·26	12·31
25 „ 30	8·23	14·94
30 „ 35	8·65	15·94
35 „ 40	11·67	16·02
40 „ 45	14·07	18·35
45 „ 50	17·04	21·18
50 „ 55	19·54	26·34
55 „ 60	26·14	28·54
60 „ 65	35·63	44·54
65 „ 70	52·93	60·21
70 „ 75	81·56	102·17
75 „ 80	117·85	143·94
80 „ 85	173·88	195·40

* This article, excepting a few words relating to its more extended application, and bringing it down to date, was written in the summer of 1867—our readers may probably guess by whom. —SUB-ED.

At first sight, it might seem that nothing could be clearer or more satisfactory than this evidence. We see that between the ages of 20 and 25, the death-rate of the unmarried men is nearly twice that of the married men. After this the ratio gradually diminishes, so that when we come to the quinquennial period between 45 and 50, the ratio, instead of being 2 to 1, is only 21 to 17, but still it is a ratio of *excess;* and so, up to the last recorded period, we find the same evidence in favour of the married men's prospects.

And again, let us take another view of the matter. It is easy to determine the mean age of the married men and of the bachelors at death. We find that *the former age exceeds the latter by fully* 19 *years!*

Here, then, we seem to have the most striking evidence in favour of matrimony as an agent in producing longevity. It would seem almost that all we need fear would be the undue extension of the argument. If one wife does so much to prolong a man's life, what effect, it might be argued, should two, three—nay, a dozen wives, for that matter—not produce? Passing over this view, as a manifest invention of that enemy of social happiness, the confirmed old bachelor, let us seriously inquire what force there really is in the evidence adduced; for the evidence is *not* wholly without force, only it has been asked to bear rather more than it is capable of doing.

There are two most important rules in the application of statistics, for want of attending to which many have fallen into serious error. First, we must assure ourselves that there is nothing in the examples collected which savours of *selection;* and secondly, we must have a *sufficient number* of examples.

As respects the second rule, we do not think there is any reason to complain of the evidence. For although the period over which the results extend is not a very long one, yet the wide range of country included in the registering is fully sufficient to make up for the defect in point of time. In fact, the close accordance observed by Dr. Stark between the results of the first two yearly periods dealt with was quite sufficient to prove that a more extended series of observations was not needed. *Results of this sort only repeat themselves when they are severally founded on a sufficiently wide range of statistical inquiry.*

But, as respects the first rule, we think there is very strong reason for suspecting the evidence before us. We must note, in the first place, that it is one of the most difficult things in the world to free results from "selection" in some form or other. Take the simple instance of tossing up a halfpenny; is the chance perfectly equal that head or tail will turn up? It seems so, but it is not necessarily so. As the halfpenny turns over and over in the air, there may be an irregularity—imperceptible to the sense—due to the unequal distribution of the metal on the two faces. And here we see the importance of the second rule mentioned above. Any irregularity in the figure of the coin will show itself *in a sufficient number of trials, as certainly as by the most accurate measurement and the most careful examination.*

We had lately a remarkable instance in our own experience of the difficulty of removing all trace of selection.

We wished, for a particular purpose, to distribute a number of dots or points, *perfectly at random,* over a square surface. This may seem a simple matter—but we did not find it so. It may be suggested—"Take a handful of grains and throw over the surface at random; then mark the place of each." The fact, however, that the grains *did* form a handful, and were *spread out, will* show itself. Another method may be suggested:—"Prick a number of holes without directing the motion of the

hand by the will." But how are we do this very thing? If we close our eyes we shall naturally make for the central parts of the surface, for fear of missing the surface altogether, and here at once is "selection"; and if we open our eyes it is absolutely impossible not to aim each stroke with *some* object, however much we may persuade ourselves that we are striking quite at random.

The method we finally adopted was this. We divided each side of the square into 100 parts, which we numbered in order, and drawing lines through the points of division we divided the square into 10,000 small squares. We then took a book full of figures (in fact that inviting work, a table of logarithms), and opening at random placed the point of a pencil at random on the page. The figure nearest to the point we marked down, and we took out in this way 4,000 figures. We now took the first four figures —8, 0, 1, 7 say, and did thus with them,—the first two gave the number 80, the next two the number 17, and we accordingly marked a dot on the *eightieth* row of squares, in the *seventeenth* square of the row. Thus we had 1,000 points distributed as we thought quite at random. But on a closer inspection we suspected the influence of selection: and where does the reader suppose we detected it? *In the shapes* of the figures used to represent numbers. In taking the number nearest to the pencil point we had omitted to notice (when the point *seemed* half-way between two figures) that the 1's, the 4's, and the 7's, do not cover *quite* as much space as the other figures. And in fact, when we came to count over our list of numbers we found there was a marked deficiency of these, and a marked excess of 8's, 5's, and 2's. *This excess showed itself* in the arrangement of the dots over the square surface.

We seem to have wandered a long way from our bachelors and married men, but if we have succeeded in showing how subtle an influence selection, conscious or unconscious, is capable of exerting, it will be found that our digression is in reality very much to the point.

It may be asked, "If Dr. Stark took the mortality of the whole population, how can there have been any selection?" We answer by another question, "Is there nothing in the state of bachelorhood itself which affords suspicion of selection?" In answering this question we wish to avoid possible misconception. In dealing with averages, individuals are not to be considered. And therefore, if we say anything of bachelors, as a class, which may seem disparaging, individual bachelors are not to be on that account offended; though, perhaps, many of our readers would not be greatly troubled even if offence were given to a few of the single-minded.

Well, then, it appears to us that if we look on bachelors as a class, we shall see evidence that they are not on a par with married men.

(*To be continued.*)

THE CHEMISTRY OF COOKERY.

By W. Mattieu Williams.

XXVI.

THERE is one more constituent of animal food that demands attention before leaving this part of the subject. This is the fat. We all know that there is a considerable difference between raw fat and cooked fat; but what is the *rationale* of this difference? Is it anything beyond the obvious fusion or semi-fusion of the solid?

These are very natural and simple questions, but in no work on chemistry or technology can I find any answer to them, or even any attempt at an answer. I will therefore do the best I can towards solving the problem in my own way.

All the cookable and eatable fats fall into the class of "fixed oils," so named by chemists to distinguish them from the "volatile oils," otherwise described as "essential oils." The distinction between these two classes is simple enough. The volatile oils (mostly of vegetable origin) may be distilled or simply evaporated away like water or alcohol, and leave no residue. The fixed oils similarly treated are dissociated more or less completely.

Otherwise expressed, the boiling-point of the volatile oils is below their dissociation point. The fixed oils are those which are dissociated at a temperature below their boiling-point.

My object in thus expressing this difference will be understood upon a little reflection. These volatile oils, when heated, being distilled without change are uncookable; while the fixed oils if similarly heated suffer various degrees of change as their temperature is raised, and may be completely decomposed by steady application of heat in a closed vessel without the aid of any other chemical agent than the heat itself. This "destructive distillation" converts them into solid carbon and hydro-carbon gases, similar to those we obtain by the destructive distillation of coal.

If we watch the changes occurring as the heat advances to this complete dissociation point we may observe a gradation of minor or partial dissociation proceeding gradually onward, resembling that which I have already described as occurring when sugar is similarly treated (see No. XIII. of this series).

But in ordinary cooking we do not go so far as to carbonize the fat itself, though we do brown or partially carbonise the membrane which envelopes the fat. What, then, is the nature of this minor dissociation, if such occurs?

Before giving my answer to this question I must explain the chemical constitution of fat. It is a compound of a very weak base with very weak acids. The basic substance is glycerine, the acids (not sour at all, but so named because they combine with bases as the actually sour acids do) are stearic acid, palmitic acid, oleic acid, &c., and bear the general name of fatty acids. They are solid or liquid, according to temperature. When solid they are pearly crystalline substances, when fused they are oily liquids.

To simplify, I will take one of these as a type, and that the one which is the chief constituent of animal fats, viz., stearic acid. I have a lump of it before me. Newly broken through, it might at a distance be mistaken for a piece of Carrara marble. It is granular like the marble, but not so hard, and, when rubbed with the hand, differs from the marble in betraying its origin by a small degree of unctuousness, but can scarcely be described as greasy.

I find by experiment that this may be mixed with glycerine without combination taking place, that when heated with glycerine just to its fusing-point, and the two are agitated together, the combination is by no means complete. Instead of obtaining a soft, smooth fat, I obtain a granular fat, small stearic crystals with glycerine amongst them. It is a *mixture* of stearic acid and glycerine, not a chemical compound; it is stearic acid and glycerine, but not a stearate of glycerine.

A similar separation is what I suppose to occur in the cooking of animal fat. I find that mutton-fat, beef-fat, or other fat when raw, is perfectly smooth, as tested by rubbing a small quantity, free from membrane, between the finger and thumb, or by the still more delicate test of rubbing it between the tip of the tongue and the palate. But dripping, whether of beef, or mutton, or poultry, is

granular, as anybody who has ever eaten bread and dripping knows well enough, and the manufacturers of "butterine," or "bosch," know too well, as the destruction or prevention of this granulation is one of the difficulties of their art.

My theory of the cookery of fat is simply that heat, when continued long enough, or raised sufficiently high, effects an incipient dissociation of the fatty acids from the glycerine, and thus assists the digestive organs by presenting the base and the acids in a condition better fitted (or advanced by one stage) for the new combinations demanded by assimilation. Some physiologists have lately asserted that the fat of our food is not assimilated at all—not laid down again as fat, but is used directly as fuel for the maintenance of animal heat. If this is correct, the advantage of the preliminary dissociation is more decided, for the combustible portion of the fat is its fatty acids; the glycerine is an impediment to combustion, so much so that the modern candle-maker removes it, and thereby greatly improves the combustibility of his candles.

It may be that the glycerine of the fat we eat is assimilated like sugar, while the fatty acids act directly as fuel. This view may reconcile some of the conflicting facts (such as the existence of fat in the carnivora) that stand in the way of the theory of the uses of fat food above referred to, according to which fat is not fattening, and those who would "Bant" should eat fat freely to maintain animal heat, while very abstemious in the consumption of sugar and farinaceous food.

The difference between tallow and dripping is instructive. Their origin is the same; both are melted fats—beef or mutton fats—and both contain the same fatty acids and glycerine, but there is a visible and tangible difference in their molecular condition. Tallow is smooth and homogeneous, dripping decidedly granular.

I attribute this difference to the fact that in rendering tallow, the heat is maintained no longer than is necessary to effect the fusion; while, in the ordinary production of dripping, the fat is exposed in the dripping-pan to a long continuance of heat, besides being highly heated when used in basting. Therefore the dissociation is carried further in the case of the dripping, and the result becomes sensible. I have observed that home-rendered lard, that obtained in English farmhouses, where the "scratchings" (*i.e.*, the membranous parts), are frizzled, is more granular than the lard we now obtain in such abundance from Chicago and other wholesale hog regions. I have not witnessed the lard-rendering at Chicago, but have little doubt that economy of fuel is practised in conducting it, and, therefore, less dissociation would be effected than in the domestic retail process.

Some of the early manufacturers of "bosch" purified their fat by the process recommended and practised by the French Academicians MM. Dubrunfaut and Fua (see *Comptes Rendus*, vol. 71) during the siege of Paris, when they and others read papers on the manufacture of "siege butter" without the aid of the dairy. This consisted in frying the refuse fat from slaughter-houses until the membranous matter and other impurities were carbonised, and thus could be strained away. I wrote about it in 1871, and consequently received some samples of artificial butter thus made in the Midlands. It was pure fat, perfectly wholesome, but although coloured to imitate butter, had the granular character of dripping. Since that time great progress has been made in this branch of industry. I have lately tasted samples of pure "bosch" or "oleomargarine" undistinguishable from churned cream or good butter, though offered for sale at 8½d. per lb. in wholesale packages. In the preparation of this I understand high temperatures are carefully avoided, and by this means the smoothness of pure butter is obtained. I mention this now merely in confirmation of my theory of the *rationale* of fat cookery, but shall return to this subject of "bosch" or "butterine" again, as it has considerable intrinsic interest in reference to our food supplies, and should be better understood than it is.

GAMBLING SUPERSTITIONS.

By Richard A. Proctor.

(*Continued from page* 12.)

TAKING in their order the gambling superstitions which have been presented above, we have, first of all, to inquire what truth there is in the idea that there are limits beyond which pure chance has no power of introducing peculiar combinations. Let us consider this hypothesis in the light of actual experience. Mr. Steinmetz tells us that, in 1813, a Mr. Ogden wagered 1,000 guineas to one, that "seven" would not be thrown with a pair of dice ten successive times. The wager was accepted (though it was egregiously unfair), and strange to say his opponent threw "seven" *nine times running*. At this point Mr. Ogden offered 470 guineas to be off the bet. But his opponent declined (though the price offered was far beyond the real value of his chance). He cast yet once more, and threw "nine," so that Mr. Ogden won his guinea.

Now, here we have an instance of a most remarkable series of throws, the like of which has never been recorded before or since. Before those throws had been made, it might have been asserted that the throwing of nine successive "sevens" with a pair of dice was a circumstance which chance could never bring about, for experience was as much against such an event as it would seem to be against the turning up of a certain number ten successive times at *roulette*. Yet experience now shows that the thing is possible; and if we are to limit the action of chance, we must assert that the throwing of "seven" *ten* times in succession is an event which will never happen. Yet such a conclusion obviously rests on as unstable a basis as the former, of which experience has disposed. Observe, however, how the two gamblers viewed this very eventuality. Nine successive "sevens" had been thrown; and if there were any truth in the theory that the power of chance was limited, it might have been regarded as all but certain that the next throw would not be a "seven." But a run of bad fortune had so shaken Mr. Ogden's faith in his luck (as well as in the theory of the maturity of the chances) that he was ready to pay 470 guineas (nearly thrice the mathematical value of his opponent's chance) in order to save his endangered thousand; and so confident was his opponent that the run of luck would continue that he declined this very favourable offer. Experience had, in fact, shown both the players, that although "sevens" could not be thrown for ever, yet there was no saying when the throw would change. Both reasoned, probably, that as an eighth throw had followed seven successive throws of "seven" (a wonderful chance), and as a ninth had followed eight successive throws (an unprecedented event), a tenth might well follow the nine (though hitherto no such series of throws had ever been heard of). They were forced as it were by the run of events to reason justly as to the possibility of a tenth throw of "seven"—nay, to exaggerate that possibility into probability; and it appears from the narrative that the strange series of throws quite checked the betting propensities of the bystanders: not one was led to lay the wager (which according to ordinary gambling superstitions would have

been a safe one) that the tenth throw would not give "seven."

We have spoken of the unfairness of the original wager. It may interest our readers to know exactly how much should have been wagered against a single guinea, that ten "sevens" would not be thrown. With a pair of dice there are thirty-six possible throws, and six of these give "seven" as the total. Thus the chance of throwing "seven" is one-sixth, and the chance of throwing "seven" ten times running is obtained by multiplying six into itself ten times, and placing the resulting number under unity, to represent the minute fractional chance required. It will be found that the number thus obtained is 60,466,176, and instead of 1,000 guineas, fairness required that 60,466,175 guineas should have been wagered against one guinea, so enormous are the chances against the occurrence of ten successive throws of "seven." Even against nine successive throws the fair odds would have been 10,077,595 to one, or about 40,000 guineas to a farthing. But when the nine throws of "seven" had been made, the chance of a tenth throw of "seven" was simply one-sixth as at the first trial. If there were any truth in the theory of the "maturity of the chances," the chance of such a throw would of course be greatly diminished. But even taking the mathematical value of the chance, Mr. Ogden need in fairness only have offered a sixth part of 1,001 guineas (the amount of the stakes), or 166 guas. 17s. 6d., to be off his wager. So that his opponent accepted in the first instance an utterly unfair offer, and refused in the second instance a sum exceeding by more than three hundred guineas the real value of his chance.

Closely connected with the theory about the range of possibility in the matter of chance combinations, is the theory of the maturity of the chances,—"the most elementary of the theories on probabilities." It might safely be termed the most mischievous of gambling superstitions.

As an illustration of the application of this theory, we may cite the case of an Englishman, once well known at foreign gambling-tables, who had based a system on a generalisation of this theory. In point of fact the theory asserts that when there has been a run in favour of any particular event, the chances in favour of the event are reduced, and therefore, necessarily, the chances in favour of other events are increased. Now our Englishman watched the play at the *roulette* table for two full hours, carefully noting the numbers which came up during that time. Then, eschewing those numbers which had come up oftenest, he staked his money on those which had come up very seldom or not at all. Here was an infallible system according to "the most elementary of the theories of probability." The tendency of chance-results to right themselves, so that events equally likely in the first instance will occur an equal number of times in the long run, was called into action to enrich our gambler and to ruin the unlucky bankers. Be it noted, in passing, that events do thus right themselves, though this circumstance does not operate quite as the gambler supposed, and cannot be trusted to put a penny into any one's pocket. The system was tried, however, and instead of reasoning respecting its soundness, we may content ourselves with recording the result. On the first day our Englishman won more than seven hundred pounds in a single hour. "His exultation was boundless. He thought he had really discovered the philosopher's stone." Off he went to his bankers, and transmitted the greater portion of his winnings to London. The next day he played and lost fifty pounds; and the following day he achieved the same result, and had to write to town for remittances. In fine, in a week he had lost all the money he won at first, with the exception of fifty pounds, which he reserved to take him home; and being thoroughly convinced of the exceeding fickleness of fortune, he has never staked a sixpence since, and does all in his power to dissuade others from playing."*

(To be continued.)

PLEASANT HOURS WITH THE MICROSCOPE.

BY HENRY J. SLACK, F.G.S., F.R.M.S.

IT is probable that the news from France of the vote obtained by Paul Bert against the admission of American pork may excite alarm here, and it is well that the appearance of the little organism whose introduction he fears should be generally known. An interdiction of all pork from the United States is not at all warranted by the state of the case. The *trichina* worm has unfortunately attacked some American pigs, but the great majority are free from the pest, and there is a very simple way of avoiding this, and some similar dangers, by not eating any meat that is insufficiently cooked. The worm cannot resist proper culinary heat, and where it has done much mischief to man there has been the bad habit, as in Germany, of eating raw ham, sausages in a similar state, or acting as pig-killers and butchers sometimes do, holding the operating knives in their mouths.

Slides of muscle containing *trichinæ* can be obtained of the dealers in microscopic objects. I hold one such up to the light, and the naked eye is sufficient to show a number of very small darkish dots. A hand-magnifier of an inch focus leaves little doubt of the nature of the object, which is a small, ovalish cyst about 1·50th of an inch long.† The lowest of Browning's platyscopic lenses, magnifying ten times, enables a little curled object, like a minute twist of thread, to be seen in the clearest of the cysts. Any one acquainted with the creature in its encysted state would at once identify it with that, or a rather higher magnification, and if the least speckiness is noticed in any meat it will be well to make a further examination. If the trichinosis is well developed, the little cysts are extremely numerous, and many of them will be found strengthened and made more conspicuous by a calcareous deposit. Fig. 1, taken from the "Micrographic Dictionary," shows the cyst with the coiled-up worm magnified fifty times linear, and at each end of the cyst is a group of fat cells, which may or may not be found in any particular specimens. Fig. 2 shows the worm out of the cyst and uncoiled, magnified 100 linear. Its mouth is at the extremity of the thin end. In its encysted condition the worm is inactive and sexually immature. If it is swallowed uninjured by certain warm-blooded animals, and gets into their alimentary canals, it develops and produces a numerous offspring, which wander about and finally settle down in voluntary muscles, protected by new cysts. The reader who wishes for detailed information can consult Dr. Cobbold's "Entozoa," and especially the supplement, which contains an account of important experiments carried on at the Royal Veterinary College, under his supervision. Various continental observers contributed to the life history of this creature. To Paget Dr. Cobbold ascribes its discovery, to Owen the first scientific description; and to Leuckart the "first full, complete, and

* From an interesting paper entitled "Le Jeu est fait," in *Chambers's Journal*.

† Cobbold says they average $\frac{1}{75}$ in. in length and $\frac{1}{150}$ in. in breadth.

correct solution of the principal questions relating to the source and mode of genesis of this flesh-worm." The result of experiments in England and abroad has been to show that carnivorous animals, and especially those which subsist on a mixed diet, are most subject to invasion by the parasite. It is possible to rear the worm in herbivorous animals, but in a natural condition it is very unlikely that they will become infected by it, and most authorities will think M. P. Bert's alarm on this score quite unnecessary. Birds do not seem fit hosts for the *trichina*. It can sometimes reach sexual maturity in their intestinal canals, but the development of active wandering specimens does not follow. Experiments with five fowls, one goose, and one crow, carried out by Dr. Cobbold, all yielded negative results. If the worms meet with favourable conditions, their multiplication is enormous. In a case cited by Cobbold, Dr. Thudiccum estimated that a German who died of trichinosis was inhabited by

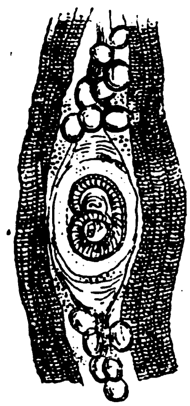


Fig. 1.

Fig. 2.

a population of 40 millions, and Dr. Cobbold thought 100 millions would have been nearer the mark, "as in places the point of a needle could not be turned between the capsules, so closely were they agglomerated." Leuckart reckoned that one ounce of the flesh of a cat he experimented upon harboured three hundred and twenty-five thousand *trichinæ*. These immense worm populations do not take long to form. In less than forty-eight hours the larval worm can attain sexual maturity, and "in six days the female parasites will contain perfectly developed and free embryos in their interior." The young worms do not get into the blood-vessels, but bore their way through the wall of the intestinal canal of their host, and through various tissues till they settle in a voluntary muscle.

Thick joints of infected pork may not in careless cooking get sufficiently heated all through to kill the worms, but in this country the risk is much lessened by the strong dislike most persons feel to pork at all underdone. Even those who can luxuriate upon boiled legs of mutton in a very sanguinary-looking condition, will not touch pork if any redness is visible.

The symptoms of *trichinosis* comprise diarrhœa, fever, and severe muscular pains. If the wandering trichinæ are very numerous, fatal results often occur, and in cases of recovery the patient may still be infected with the parasites in their quiet encysted state.

The professional experts who have to examine infected meat cut thin slices of the muscle, and stretch it carefully on a glass plate. It is then easy to view it with the microscope, and instruments with large stages and special conveniences are made for the purpose.

English pigs reared in farms upon good food are quite safe from the disease; but, without special reference to trichinæ, all pork from animals fed upon garbage, and especially upon butcher's refuse, should be avoided. In America pig-rearing and killing is carried on upon a gigantic scale. As a rule the pigs are remarkably well-fed, as maize and other suitable articles are produced in abundance. Probably, in localities where trichinosis has been common, there has been a great neglect of rational precautions, and the animals have been allowed to swallow filth that should have been kept from them.

DANCING TO DEATH.

WHEN strong and healthy men, inured to hardship by exposure, have to work unduly hard, with insufficient rest and bad food, and under frequent and marked changes of temperature, they sometimes give way. But at this season many delicately-reared girls, who perhaps are not allowed to go out for a walk in unpleasant weather, who have not like their brothers had the healthy training of regular exercise, and who, apart from training, are less fitted by nature to bear cold, heat, and overwork than men, are eager to do work and to bear exposure from which the most stalwart men might with good reason shrink. When the oarsman or the pedestrian, in training for some great race, has done his work for the day, he carefully defends himself against cold or draught. When he has indued dry clothing he takes grateful rest, and each day he has well-regulated sleep. His diet is carefully selected. Everything, in fine, is done to guard, sustain, and increase his vital energies, while the extra work involved in practical training goes on. Is there any kind of work or training much harder than the work done by many young girls in festive seasons? For three or four days each week,—six if they get their wish,—they dance (and talk) almost continuously from nine or so in the evening to two or three or later in the morning. Arms and chest,—too often, also, shoulders and bosom,—are exposed, and suffer alternations of warmth and cold such as a strong, hairy-chested sailor would not care to endure, and certainly would not endure even if he went all day bare-chested and bare-armed. At one moment a fair dancer is perspiring in the warm, moist, and not over-pure air of the ball-room, at another enjoying a refreshing but most dangerous chill in some cool room thoughtfully yet thoughtlessly prepared for the comfort of the dancers. Unwholesome food is supplied to these much-tired girls, at unsuitable and irregular times, and to be consumed under conditions very unfavourable to health. An unwholesome excitement encourages them to exertions far beyond their strength, and keeps them (later) from their rest when rest is most necessary. They grow haggard as the season advances. Worse,—in their opinion—(though in truth it is nature's warning), they cease to be attractive. They grow fretful and irritable—it is the outcry of nature's safety-valve. Well for them if at the end of the festive season they are able to recover a part of their lost energies during a more or less prolonged period of rest and dulness. Not a few sink under the strain. The wonder is that so many survive. One would expect all who have been exposed during several seasons to so severe a trial to show the effects in an after life of weakness and sickness. But although, unfortunately, this is the case with many, there are always some among those who stand many successive seasons of such folly (there

is no other word for it) who may be pointed to as living proofs that there is no real harm in the process—those, to wit, who, being exceptionally strong, have stood the strain best. Just as there are some venerable old ladies who, blessed originally with splendid constitutions, have born unflinchingly and apparently unharmed, the barbarism of tight lacing, and are pointed out by Observers as living proofs that tight lacing does no harm, but is rather beneficial than otherwise (the cause, perhaps, of their fine constitutions!) so there are always many,—so stalwart is our race,—who remaining still strong after years of dissipation, are supposed to prove that there is no harm but rather good in this "sowing of feminine wild oats."

But the wiser among us should teach these foolish ones that they are exposing themselves to a process by which the weaker die, and all—even the strongest—suffer in health and strength.

WELL-EARNED PRAISE.—The Postmaster-General has issued a circular in which he expresses his high appreciation of the energy and zeal displayed in successfully meeting the unprecedented pressure of Post-office business experienced throughout the United Kingdom during the last days of the past year.

SENSE OF DIRECTION IN MAN. — With regard to the question of the means by which animals find their way home from a long distance, a striking account in relation to man will be found in the English translation of the "Expedition to North Siberia," by Von Wrangel. He there describes the wonderful manner in which the natives kept a true course towards a particular spot, while passing for a long distance through hummocky ice, with incessant changes of direction, and with no guide in the heavens or on the frozen sea. He states (but I quote only from memory of many years' standing) that he, an experienced surveyor and using a compass, failed to do that which these savages easily effected. Yet no one will suppose that they possessed any special sense which is quite absent in us. We must bear in mind that neither a compass, nor the north star, nor any other such sign, suffices to guide a man to a particular spot through an intricate country, or through hummocky ice, when many deviations from a straight course are inevitable, unless the deviations are allowed for, or a sort of "dead reckoning" is kept. All men are able to do this in a greater or less degree, and the natives of Siberia apparently to a wonderful extent, though probably in an unconscious manner. This is effected chiefly no doubt by eyesight, but partly, perhaps, by the sense of muscular movement, in the same manner as a man with his eyes blinded can proceed (and some men much better than others) for a short distance in a nearly straight line, or turn at right angles, or back again. The manner in which the sense of direction is sometimes suddenly disarranged in very old and feeble persons, and the feeling of strong distress which, as I know, has been experienced by persons when they have suddenly found out that they have been proceeding in a wholly unexpected and wrong direction, leads to the suspicion that some part of the brain is specialised for the function of direction. Whether animals may not possess the faculty of keeping a dead reckoning of their course in a much more perfect degree than can man; or whether this faculty may not come into play on the commencement of a journey when an animal is shut up in a basket, I will not attempt to discuss, as I have not sufficient data.—CHARLES DARWIN.

HOW TO CHOOSE A TRICYCLE.

BY JOHN BROWNING,

Chairman of the London Tricycle Club.

SCARCELY a day passes without my receiving an application, either personally or by letter, asking me for advice respecting the choice of a tricycle.

In accordance with the request of several of these correspondents I propose to give some general advice on the subject, as just at the present time many persons are doubtless thinking of getting a machine to ride through the approaching season.

I would advise all intending purchasers to give their orders as early as possible. As a general rule, the order is not given until we have had some fine riding weather; then the order is given, and the manufacturer is pressed to execute it instantly. If all orders were given in this way it is evident that the manufacturers would be glutted with orders for two months in the year, and without business for the other ten.

Those tricyclists who already have machines, but wish to try other mounts, would, of course, do well to wait and see the novelties at the three great exhibitions; but those who intend riding for the first time would do best by giving their orders at the dull time, and having a machine carefully made for them exactly suited to their requirements.

Not a week passes without my being asked, "Which is the *best* tricycle?"

There is no such thing. What will suit one part of the country will not suit another, and what will suit one rider well would be almost useless to another. Be sure to deal with a manufacturer or agent of good repute or who has been well recommended to you by some one who has had good experience.

Do not apply to some celebrated rider simply because you have heard of his performances on wheels. Such men are sometimes among the worst judges of a machine. They are so strong that they can drive anything.

When you have decided on giving the order, furnish the following information to the person to whom you give it.

Your height, your weight, your age. Whether you are very powerful, of average strength, or weak. The condition of the roads over which you intend principally to ride—that is, whether they are smooth, fairly good, or rough, and whether they are very hilly, undulating only, or level. All these points ought to be taken into consideration in building a machine to suit you.

It may also be desirable for you to tell him whether you wish to ride fast, that is, at from nine to twelve miles an hour; at a moderate pace, that is, from seven to nine miles an hour, or whether you will be satisfied to get over the ground easily and comfortably, at the rate of from five to seven miles an hour. Do not believe anyone who tells you that it is easy to jog along at a pace of nine miles an hour. A strong and practised rider on a light machine has his work to do to cover nine miles in an hour on a give-and-take road.

You have next to determine what kind of machine you will have.

Be sure you have a double-driver—that is, a machine in which two wheels are driven; as a rule, all single drivers are unsatisfactory. The only exception I can make to this is in the case of the Coventry Rotary; that machine is in a class by itself. It is light and wonderfully sensitive in steering.

If you want the fastest machine you can get, and do not mind giving some little time to practising and running some risk while learning, then have a "Humber." But

I do not consider that this is a good machine for stout, elderly men.

The safest and best all-round machines are the front-steerers, known as the "Rucker," the "Salvo," the "Apollo," and the "Premier." If you wish for a rear-steerer, choose the "Rover" or the "Rucker."

Be sure the machine has a band-brake. Though some of the tyre-brakes are efficient when first made, they are very liable to get out of order. One of the spoons then comes on to the tyre of one of the wheels before the other. If in this condition the brake is applied to check the speed of the machine when descending a hill, one wheel is stopped while the other runs on; as a result of this, if the pace is at all great, the machine swerves and turns over, with the risk of seriously injuring the rider. The band-brake in a double-driver checks both wheels equally—hence its great superiority.

The next point to consider is the size of the wheels. If you want a compact, strong, and yet light machine, let these be as small as you can ride with advantage; the only point to observe being that your heels must clear the axle when you are pedalling.

If for a front steerer, this will be 40 inches for a man 5 ft. 6 in. high, and for each two inches that the rider is below this height the wheels may be one inch less, and for each two inches he is above this height the wheels should be one inch more, so that a rider 6 ft. high may ride a machine with wheels 44 in. diameter, as 43 in. are seldom or never made.

The weight of a roadster machine of this size, for a rider weighing 11 stone, ought not to exceed 80 lb., and a machine with hollow rims, small rubber tyres (some ½-inch in diameter), and laced spokes, may be made not to exceed 65 lb., but such a machine would require using carefully over rough roads.

The spring is a most important part of the machine, and Harrington's Arab Cradle Spring is the best.

Lamplough's long-distance suspension saddle I prefer to any other. Cushioned saddles are not nearly so good.

Ladies should always use a saddle in preference to a seat, and ride as high as they well can, and well over their pedals; they will then find none of the tendency to slip off that they often complain of when riding on a seat, and if the saddle is only two or three inches behind the pedals, they never need when riding show even the tops of their boots.

The handle-rods as well as the seat-rod should be adjustable to suit the rider, and the seat should have an adjustment to and fro as well as up and down. The best contrivance for enabling this to be done is an angle pin, which can be shown in print by a letter L reversed in both directions (┏). The vertical part of the L moves up and down, and the seat moves to and fro on the horizontal portion.

I must reserve my remarks upon foot-rests until my next article.

INSTINCT.—From the case of neuter instincts, of certain reflex actions, and of movements such as those of the tumbler-pigeon, it seems to me in the highest degree probable that many instincts have originated from modifications or variations in the brain, which we in our ignorance most improperly call spontaneous or accidental; such variations having led, independently of experience and of habit, to changes in pre-existing instincts, or to quite new instincts, and these, proving of service to the species, have been preserved and fixed, being, however, often strengthened or improved by subsequent habit.—CHARLES DARWIN.

ALMANAC LESSONS.

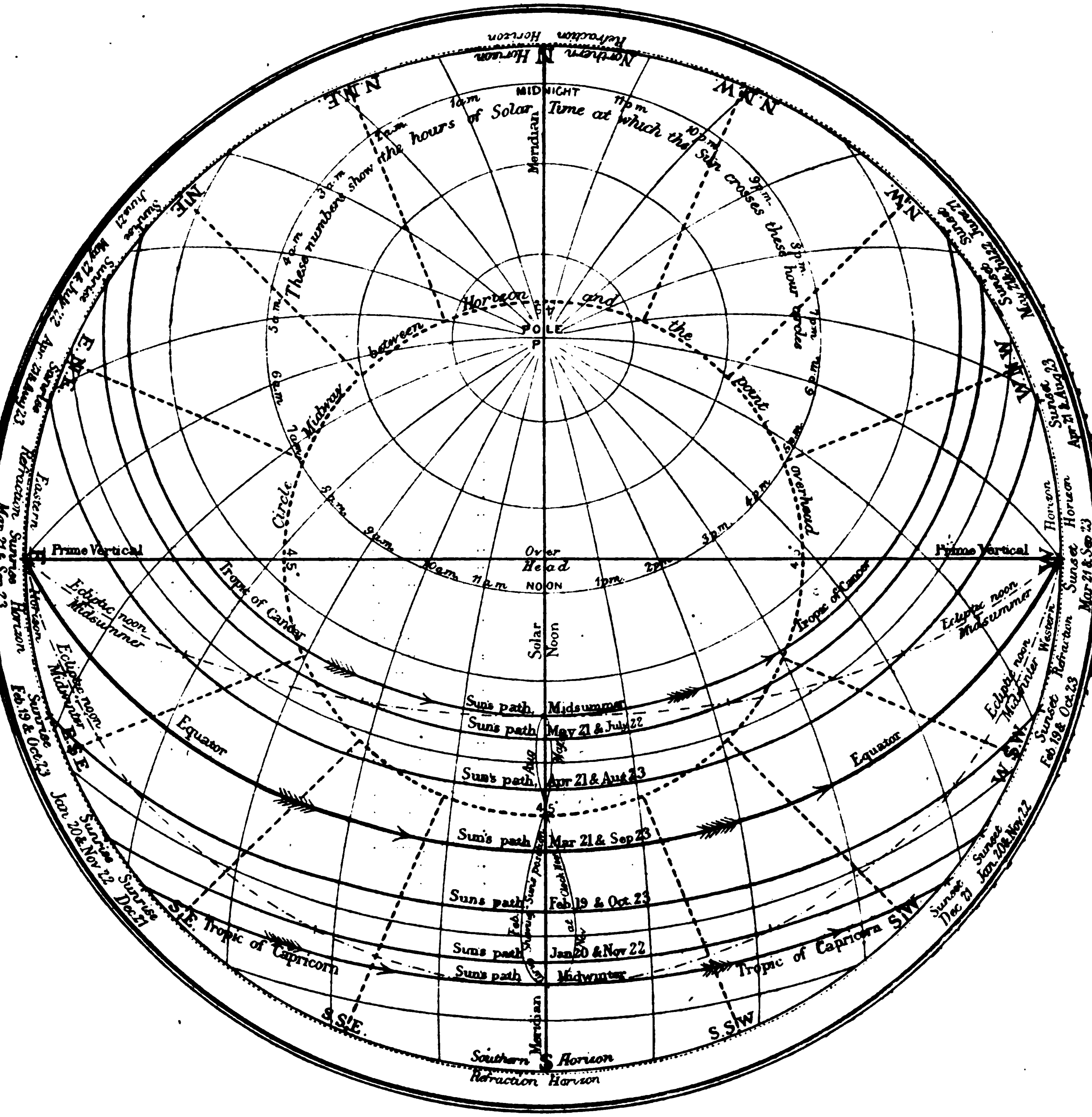
THE SUN'S RISINGS, SETTINGS, &c.

By RICHARD A. PROCTOR.

As the astronomical year may most conveniently be regarded as beginning either when the sun is crossing the equator, that is at the spring or autumn equinox, or else when he is farthest from the equator, that is at the winter or summer solstice, I have thought it convenient to reproduce the risings and settings, &c., of the sun for December, 1883. On the 22nd of that month the sun entered the sign Capricornus, or winter began. This date is marked off. In like manner Jan. 20, when the sun enters Aquarius, is marked off.

			The Sun Rises.	The Sun Sets.	Before Clock.	Hourly Variation of Equation of Time.
			H. M.	H. M.	M. S.	s.
Dec.	1	....	7 46	3 53	10 51	0·94
„	2	....	7 .47	3 52	10 28	0·97
„	3	....	7 48	3 51	10 5	0·99
„	4	....	7 50	3 51	9 41	1·02
„	5	....	7 51	3 50	9 16	1·04
„	6	....	7 52	3 50	8 51	1·06
„	7	....	7 54	3 50	8 25	1·08
„	8	....	7 55	3 49	7 59	1·10
„	9	....	7 56	3 49	7 32	1·12
„	10	....	7 57	3 49	7 5	1·14
„	11	....	7 58	3 49	6 38	1·15
„	12	....	7 59	3 49	6 10	1·17
„	13	....	8 0	3 49	5 42	1·18
„	14	....	8 1	3 49	5 13	1·19
„	15	....	8 2	3 49	4 45	1·21
„	16	....	8 2	3 49	4 16	1·22
„	17	....	8 3	3 49	3 46	1·22
„	18	....	8 4	3 49	3 17	1·23
„	19	....	8 5	3 50	2 47	1·24
„	20	....	8 6	3 50	2 17	1·24
„	21	....	8 6	3 50	1 47	1·25
„	22	———	8 7	3 51	1 17	1·25
„	23	....	8 7	3 51	0 47	1·25
„	24	....	8 7	3 52	0 17	1·25
„	25	....	8 8	3 53	After	1·25
„	26	....	8 8	3 53	0 42	1·24
„	27	....	8 8	3 54	1 12	1·24
„	28	....	8 8	3 55	1 42	1·23
„	29	....	8 9	3 56	2 11	1·22
„	30	....	8 9	3 57	2 40	1·21
„	31	....	8 9	3 58	3 9	1·20
Jan.	1	....	8 9	3 59	3 38	1·19
„	2	....	8 8	4 0	4 7	1·18
„	3	....	8 8	4 1	4 35	1·16
„	4	....	8 8	4 2	5 2	1·14
„	5	....	8 8	4 3	5 29	1·12
„	6	....	8 7	4 4	5 56	1·10
„	7	....	8 7	4 6	6 22	1·08
„	8	....	8 7	4 7	6 48	1·06
„	9	....	8 6	4 8	7 13	1·04
„	10	....	8 5	4 10	7 38	1·02
„	11	....	8 5	4 11	8 2	0·99
„	12	....	8 4	4 12	8 25	0·97
„	13	....	8 4	4 14	8 48	0·94
„	14	....	8 3	4 15	9 10	0·91
„	15	....	8 2	4 17	9 32	0·89
„	16	....	8 1	4 19	9 53	0·86
„	17	....	8 0	4 20	10 13	0·83
„	18	....	7 59	4 22	10 33	0·80
„	19	....	7 58	4 23	10 52	0·77
„	20	———	7 57	4 25	11 10	0·74
„	21	....	7 56	4 27	11 27	0·71
„	22	....	7 55	4 28	11 44	0·68
„	23	....	7 54	4 30	12 0	0·65
„	24	....	7 53	4 32	12 15	0·61
„	25	....	7 51	4 34	12 29	0·58
„	26	....	7 50	4 35	12 43	0·55
„	27	....	7 49	4 37	12 55	0·52
„	28	....	7 47	4 39	13 7	0·48
„	29	....	7 46	4 41	13 19	0·45
„	30	....	7 44	4 42	13 29	0·41
„	31	....	7 43	4 44	13 38	0·38

What appears most striking at first in the numbers here



Illustrating the Sun's Diurnal Course throughout the year. (In this Map equal distances from the centre represent equal distances on the Sky from the point overhead.)

tabulated is that the hour of sunrise grows later up to the end of December, eight or nine days after the winter solstice, when, the sun being at his greatest distance south of the equator, one would expect him to rise latest; while, on the other hand, the hour of sunset gets earlier only until about December 12th. The day indeed grows shorter until December 21-22, but in a seemingly unsymmetrical way, shortening in the morning and lengthening—though rather less—in the evening. It follows from this that true mid-day—that is the time when the sun is due south and at his highest—is changing during this part of December, getting later and later. This is shown by the next column. The sun is before the clock at the beginning of December by nearly 11 minutes; that is the sun comes to the south nearly 11 minutes before clock noon, and of course he rises earlier and sets later than he would if he came to the south at true noon. The day lasts 8 hours 7 minutes at this time, and if solar noon fell at 12 o'clock the sun would rise at 7 hours 56½ minutes, and set at 4 hours 3½ minutes. But both these phenomena take place nearly 11 minutes earlier. Every day until Christmas the sun loses more and more of this advance, until at or about Christmas-day the day is symmetrical, the interval from sunrise to clock noon being equal to the interval from clock noon to sunset. After this solar noon occurs follows clock noon, by an interval gradually increasing all through January and afterwards, as will be seen in due course, till February 11th.

This is partially explained in the accompanying diagram showing the sun's daily path in different parts of the year. The short daily course of the sun at midwinter is shown—marked Tropic of Capricorn. The actual annual path of the sun in the celestial sphere carries him along the ecliptic in a contrary direction from his diurnal course; and of course the more rapidly he crosses the hour circles *that* way the later he must be in being brought by the diurnal revotion to the meridian or due south. Now, not only is he near perihelion in December and January, and therefore moving faster than his average rate along the ecliptic, but also as the diagram shows he is traversing the hour circles where they lie nearer together than on the equator. Hence all this time, he is falling towards the east with more than average rapidity, and the solar day is therefore increasing in length. The portion of the long double loop in the meridian, between the little lines made on the sun's midwinter path, slightly to the right or west of the meridian, and where the word February is written, (in the eastern part of the lower loop) shows how the sun's position at clock noon passes at this season farther and farther east, or causes him to come later and later up to the meridian.

But this double loop illustrates so much better than any plan I have yet seen what is called the equation of time, that it will be well to have—shortly—a drawing of it on a better scale.

(*To be continued.*)

THE UNIVERSE OF SUNS.

By R. A. PROCTOR.

(*Continued from p. 10.*)

IN passing from Kant's theory to Lambert's, I would invite the reader to note with special care the points in which the two theories differ. It is admitted that Lambert's theory though published five years later than Kant's (or in 1760)* was formed quite independently; but Kant himself remarked in 1763 that "the accordance between his ideas and those of Lambert (*cet homme spirituel*) extends even to the most minute details." It will be found, I conceive, that no such close agreement exists between the two theories.

Lambert's theory consists of the following theses:—

1. The stars are suns, and like our sun they are the centres of planetary systems. Each sun with its dependent worlds forms a *system of the first order*. (It will be observed that this agrees with Kant's first thesis,—which, however, was earlier enunciated by Huyghens.)

2. Our sun belongs to a vast globular cluster of stars, *a system of the second order*, which includes all those stars, spread over all parts of the heavens, which do not belong to the Milky Way.

3. There are many systems of the second order. They are not spread throughout all space, but are all found near a certain principal plane or mean level, and being ranged one behind the other to a great depth, they form by their concourse the Milky Way. This is *a system of the third order*, not globular but flat,—forming, in fact, a disc, whose diameter vastly exceeds its thickness. The fact that this system of the third order consists of different systems of the second order, is shown, says Lambert, by the irregularity of the Milky Way, by the different richness of its various parts, and by its branching figure. (It will be noticed that Lambert here adopts a view resembling a part of Wright's theory on which Kant says nothing; but Lambert's views are much more definite than Wright's, and accord much better with the results of Sir W. Herschel's later labours.)

4. Analogy suggests that there are in the universe many Milky Ways. Perhaps the nebula in Orion is a Milky Way, nearer to us than the rest. Should this be the case, telescopic research will reveal many others, forming together *a system of the fourth order*.

5. Analogy suggests that there are *systems of a fifth order*, and of yet higher orders. But so far as observation is concerned, stellar astronomy includes—1, suns; 2, clusters of suns; 3, the Milky Way; and 4, systems of Milky Ways.

6. The common bond of different systems of the same order, as well as of the successive orders of systems, is gravitation, which produces concentric motions within all the systems.

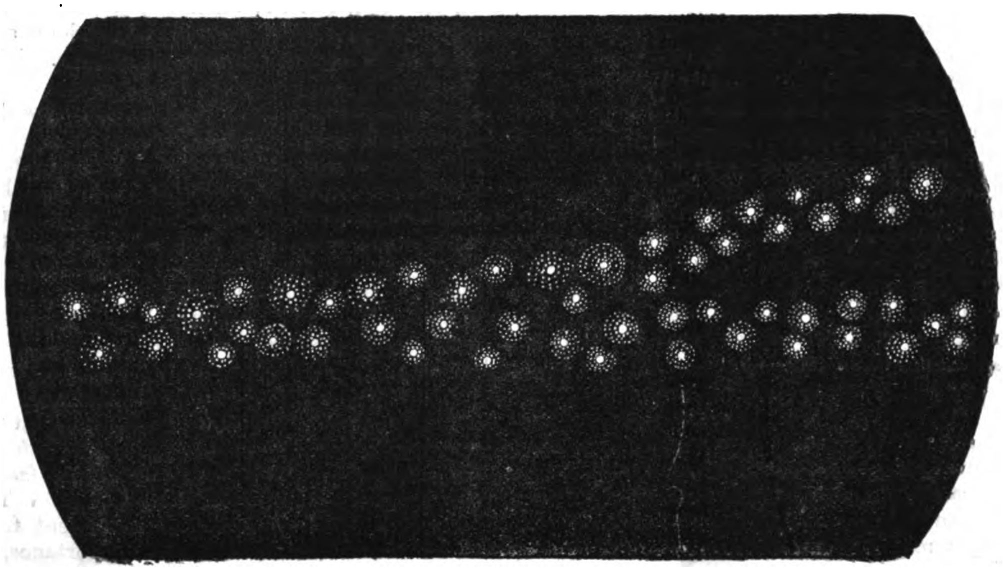


Fig. 3.—Illustrating Lambert's Theory of our Stellar System.

* More fully however in 1761. Lambert tells us that his theory was formed in 1749, when he was not 21 years old. Kant was born in 1724, four years before Lambert: but survived him 27 years, for Lambert died in 1777, Kant in 1804.

7. As in the solar system we observe that the distances between the several planets exceed incomparably the dimensions of each planet or scheme of planet and satellites, so the distances between sun and sun exceeds enormously the dimensions of the several solar systems.*

8. A similar relation prevails amongst higher orders. The cluster to which the sun belongs contains above a million and a half of stars, forming a spherical cluster, whose diameter exceeds 150 times the distance of Sirius; but this diameter is far less than the distance of the nearest cluster of suns.

Other theses which follow need not here be considered, because they relate to matters already discussed, and are for the most part based on imperfect evidence, and also (as the elder Struve has shown) on incorrect ideas. But Lambert's conceptions respecting central suns are worthy of notice. They are included in the following theses:—

9. It seems clear from the analogy of the solar system, that each of the clusters of suns must be ruled by a central orb. No otherwise, it would appear, can the motions within the cluster have the requisite stability.

10. The mass of such a central sun must be very great. Its luminosity may be faint, or it may even be an opaque body, illuminated by the suns which travel nearest to it. If suns are intended to illuminate dark orbs, we may infer that the central orb is thus illuminated. So that whereas in solar systems the central body illuminates the rest, in systems of suns the central orb receives light from the bodies over which it bears sway.

11. The central orb may one day be detected by the perturbations which it produces within our solar system,—much as the sun produces perturbations in the lunar motions. Or it may have phases, or dark spots, and so be detected by its rotation. On this account the nebula in Orion, which may be the central orb of our cluster of suns (since it lies in the proper direction) should be carefully watched.

12. Having accepted the belief that a central body sways each cluster of suns, we may infer from analogy that a central orb of yet vaster dimensions sways the Milky Way or the system of sun-clusters. And the analogy may be extended to the systems of the fourth, fifth, and higher orders.

I cannot better close the account of Lambert's remarkable speculations than by quoting the remarks in which he describes the wonderful grandeur of the universe of systems as pictured in his theory:—"How far soever we may extend the scale," he says, "we must necessarily stop at last." And *where?* At the centre of centres, at the centre of creation, which I should be inclined to term the capital of the universe, inasmuch as thence originated motion of every kind, and there stands the great wheel in which work the teeth of all the rest. From thence the laws are issued which govern and uphold the universe; or rather, there they resolve themselves into one law of all others the most simple. But who would be competent to measure the space and time which all the globes, all the worlds, all the worlds of worlds, employ in revolving round that immense body—the Throne of Nature and the Footstool of the Divinity! What painter, what poet, what imagination is sufficiently exalted to describe the beauty, the magnificence, the grandeur, of this source of all that is beautiful, great, and magnificent; and from whence order and harmony flow in eternal streams through the whole bounds of the universe!"

(*To be concluded.*)

* Lambert considered that the distance between the nearest fixed star and the sun, exceeds the earth's distance from the sun 500,000 times.

ROVE-BEETLES.

By E. A. BUTLER, B.A., B.Sc.

THOUGH a very numerous and useful group of insects, but few of the rove-beetles are generally known, and these are not respected as they should be. The largest and most familiar member of the family is called the "Devil's Coach-Horse"—a fanciful name suggested by the repulsive aspect, disgusting odour, and ferocious nature of the creature. It is a dull black, narrow, and elongate monster (Fig. 1), a little over an inch in length, often seen scuttling along a pathway, ever ready to assume the offensive, both as regards attitude and smell. Let it be menaced, say, by the end of a walking-stick, and forthwith it faces round to give battle, opening a formidable pair of sickle-shaped jaws, and bending the hinder part of its body over its back as if to suggest that, like the scorpion, it carries a sting in its tail. Though this is not the case, yet in the tail is lodged what is frequently almost as effective as a sting, a pair of soft, yellowish vesicles, which can be protruded at pleasure, and which exhale a most abominable odour, and the insect is not slow to bring them into requisition. Here, then, we have a being armed at each end, jaws in front, scent-bags behind, so that, attack him which end you will, he is ready for you.

This well-known insect, to which entomologists have given the appropriate name *Ocypus olens*, the "stinking swift-foot," may serve as the type of the group. Rove-beetles, then, are very readily recognised by their narrow and elongate form, and by the shortness of the wing-covers, or elytra, the sheaths which, as in the ladybirds of which we spoke a short time ago, cover the membranous wings. These are so small that, instead of extending over the whole abdomen, as is usually the case with beetles, they cover only the basal part of it, leaving all the rest, to the extent generally of some six or seven segments, entirely exposed. This peculiar arrangement gives the insects a superficial resemblance to earwigs, with which, in fact, the larger species are not unfrequently confounded. The wings of earwigs, however, are altogether different from those of rove-beetles, and, what is of more importance, the developmental history of an earwig is similar to that of a bug, and quite different from that of a beetle; in other words, young earwigs are very similar in form to the adult, but juvenile rove-beetles are grub-like, and wholly unlike their parents until they arrive at their final stage. The extreme shortness of the elytra, which in this group exhibit the minimum of development, has suggested the name Brachelytra (short wing-covers), by which the creatures are known among entomologists. Though the elytra are so small, the wings are generally ample, and, when not in use, have to be very neatly folded and packed away under their horny covers. The insect uses its long, flexible abdomen to assist it in this operation, turning its tail over its back and deftly tucking the gauzy wings into place.

The Brachelytra are, many of them, devourers of all sorts of decaying organic matter. They may be found plentifully in or under the carcases of small quadrupeds, birds, &c.; a keeper's tree, to which "vermin" are nailed, scarecrows, dead rabbits, mice, moles, and so on yield them in plenty. They occur in great abundance in all kinds of animal excrement and in manure-heaps, under heaps of vegetable rubbish, dead leaves, &c.; in fungi, either when fresh or when decaying, but especially the latter; in heaps of rotting seaweed, amongst the filthy accumulations on the floors of fowl-houses, and among the rubbish at the bottom of haystacks. Generally, the more rotten the matter, and therefore the more necessary its speedy removal, the more

numerous are the rove-beetles. Some, however, frequent much more attractive localities, such as moss, and even flowers. Fig. 4 represents a species that often swarms in flowers, especially those of the Umbelliferæ. It belongs to a section that, in the shortness and breadth of the body and the greater comparative length of the elytra, departs a good deal from the ordinary Brachelytrous type. Some are found under the bark of trees, some burrow in sandy places, others frequent very damp spots, such as mud-flats and the margins of rivers and ponds, running actively about, sometimes almost in the water itself. One remarkable genus called *Stenus* is specially noteworthy as coming under this last category. There is a very strong family likeness between the 65 British species of this genus, by which they can be easily recognised. They are of a deep-black or leaden-black colour, occasionally with a small yellow spot on each elytron; they have very prominent eyes, a cylindrical thorax, a long cylindrical body, flattened above, and a very hard integument; they have a peculiar impish sort of look, and the appearance of the whole genus is as though they had been most grievously afflicted with small-pox, for the whole surface—head, thorax, elytra, abdomen, and all—is always closely and deeply fitted with minute rounded depressions. One of the largest and most robust of the genus is represented at Fig. 2.

Fig. 1. Ocypus olens.
a. Nat. size.

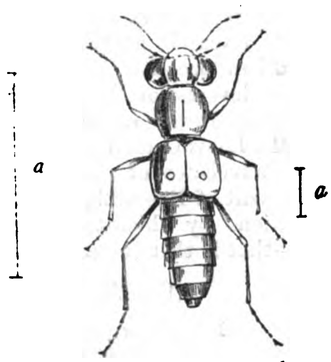


Fig. 2. Stenus bimaculatus.
a. Nat. size.

One of the most curious habits of the Brachelytra is a kind of parasitism, or rather commensalism, which is exhibited, however, by only a few species. These are always found in ants' nests, where they live on terms of the closest intimacy with the legitimate occupants, and each species is usually associated with only one kind of ant. It is difficult to say what is the exact nature of the bond that leads two such dissimilar insects to unite their fortunes; it has been suggested that the beetles may furnish some secretion capable of being used as food for the ant-grubs; but, however this may be, and whatever other services the Brachelytra may render, it seems probable that they may, at least, act the part of scavengers to these large communities. At any rate, the ants seem greatly attached to them, sometimes even gently carrying them away in their jaws to places of security, when any danger threatens. Fig. 3 represents one of these myrmecophilous beetles; it lives with the well-known Wood Ant, the huge mounds of which are familiar objects in game preserves. It should be remembered that beetles are not the only kinds of creatures that are associated with ants; some ants carefully watch over and maintain communities of aphides, or plant-lice, which yield them the honeydew of which they are fond; and, again, in some ants' nests is found a peculiar white wood-louse, which rejoices in the euphonious appellation of *Platyarthrus Hoffmannseggii*.

Rove-beetles readily take to the wing, and though ordinarily concealed at the roots of grass, under stones, or amongst their favourite food, bright summer sunshine invariably calls them forth from their hiding-places, and then they fly vigorously about seeking pastures new, as well as a suitable nidus for their eggs. Immense numbers of the smaller kinds may thus be found flying about over any recent deposits of horse-dung, or about manure-heaps, or piles of garden-refuse. Not unfrequently an exceptionally fine and warm day in summer entices enormous swarms from their places of concealment, and they invade our streets, appearing sometimes in such numbers on the pathways that the unwary steps of pedestrians leave their mangled carcases dotting the pavement in all directions. When the sunshine departs, those that are left disappear as suddenly as they appeared, retiring into nooks and crannies and obscure places, which none but themselves know so well how to find.

Most of them are extremely active, not only on the wing, but on the ground as well, where their rapid movements frequently baffle the collector who attempts to seize them by darting at them with finger and thumb. When surprised, some rush away with a sharp, jerking motion, stopping ever and anon as if to note the effects of their tactics, but most go headlong forward, with a trotting sort of gait, in the first direction that offers, making the most energetic, if not always the most intelligent, efforts to reach some place of concealment.

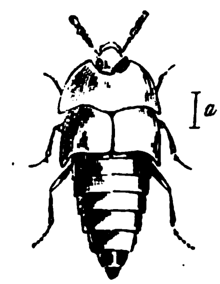


Fig. 3. Dinarda Märkeli.
a. Nat. size.

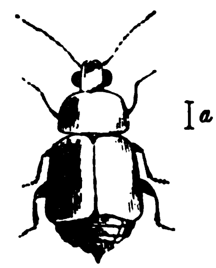


Fig. 4. Anthobium ophthalmicum.
a. Nat. size.

Upwards of 700 species of these creatures have been met with in the British Isles, and they vary in size from 1-24th of an inch to a little over an inch in length. By far the greater number, however, range from 1-12th to ¼ inch. It will, of course, be easily understood that as the Brachelytrous type has had, as it were, so many changes rung upon it, and as the insects themselves are in so many instances exceedingly minute, the distinctions between the different species are not unfrequently slight. In fact, it is often extremely difficult to discriminate between closely-allied species, and the assistance of some kind of magnifying power has generally to be evoked; the mere detection of the difference is difficult to any but the practised eye, but when once recognised they are found to be constant and reliable.

The range of coloration is not very great; black is by far the commonest colour; some species are more or less of a metallic green or bronze; others are adorned with bright red; and others again with tints varying from a pale yellowish brown, to a deep blackish brown; but, as a rule, they are not noted for either brilliance or elegance of colouring.

Many species, especially the larger ones, smell badly, as might have been expected, considering the nature of their food. They are, however, an extremely useful group of insects; they compensate for the smallness of their size by

their amazing numbers, and the amount of decaying matter annually caused to disappear from the earth through their efforts must be considerable, though on such a point it is, of course, impossible to obtain anything like reliable statistics. But if it be remembered that during the summer months there are, say in our own country alone, millions upon millions of them continually at work as scavengers wherever any putrescent matter is found, it will readily be believed that they discharge a most useful function in the scheme of nature. In the winter those that have survived the vicissitudes of the season retire into seclusion, and remain in a torpid condition till the genial warmth of the returning spring summons them again to activity, just at the time that the processes of decay are being by the same power accelerated. It must not be supposed that the Brachelytra confine their attacks to carrion and refuse; many of them, especially the larger sorts, are in the highest degree predatory, devouring with avidity even insects of their own kind, and many, no doubt, take equally readily to both living and dead food.

The *Gaceta Industrial* says a concentrated solution of bichromate of potash and glue makes an excellent cement for repairing articles of broken glass. After covering the fractured surfaces with this solution they are brought together and exposed to the action of the sun. This cement resists boiling water.

At Jonkoping, Sweden, is the oldest and largest match factory in the world. It was established 100 years ago, and there are now to be seen specimens of the matches used at the beginning of the present century, consisting of big fagots of wood furnished with a handle and a tip to dip in a bath of sulphur. The wood from which the present kind of matches is made is taken from the adjacent forests, which are divided into fifty sections. Every year one section is cut and then replanted with young trees. The trees are hewn into planks in the forest and cut into slivers in the factory. The boxes are made of the outside of the trees. The factories are on the banks of lakes which are connected with one another by wide canals. Millions of matches are turned out each day. The *Scientific American* says some idea of where they all go to may be obtained from the statement that there are at least 280,000,000 of matches burned each day in the United States, or an average of five matches for each person.

Dr. Andrew Wilson's London Lectures.—The last two lectures of Dr. Wilson's course on "Physiology and Anatomy" were given in Princes' Hall, Piccadilly, on the evenings of Jan. 2 and 5. On the former date Dr. Wilson gave a lucid description of "The Heart and its Work," and dealt with the subject of "Brain and Nerves" in the last lecture. The hall was well filled on both occasions; and the beauty of the lime-light illustrations used was the theme of universal comment. The manner in which Dr. Wilson treats his subjects is eminently calculated to awaken a lively interest in matters of high importance to the nation at large. Popularisation of the teachings of physiology, and the intelligent conception of how we live and move, of what foods are necessary and which injurious, how breathing is carried out, what a brain is, and how nerves act, are items that are all too sparsely represented in the education of young and old alike. Dr. Wilson's lectures are eminently educative, and it is in this spirit, and for the good they are calculated to effect as educative means, that we are pleased to be able to accord to them our highest praise.

A CHRISTMAS CAR[R]OL[L] ON ENDOWMENT OF RESEARCH.

Few circumstances have caused true lovers of science in recent times more pain than that outcry for the Endowment of Research, to which, indeed, some few really disinterested workers and thinkers have been persuaded to lend their voices, but which has derived its volume almost wholly from those who only value science as a means of getting well-paid offices or high-sounding titles. It is an open secret that this unseemly outcry has worked much evil. It marred the harmony which had once prevailed in the Royal Astronomical Society until now the Society is to all intents and purposes silent as to science, though still occasionally noisy in debate over sinecures. Of the three who broke the peace of that Society, one was well-meaning though mistaken, one is dead, but the third still, though defeated, raises unremitting cry for gold from the sun ("Sol gold is," says Chaucer, prophetically, "and Luna silver we threpe") with the vain promise that, by sun-watching with unsnuffed candle, he will save our colonies from famine, our sailors from storm, and our farmers from foul weather.

Of all sources from which aid was hoped for by the opponents of the Endowment of Research (or rather of the would-be plunderers of the public purse who make promise of research as their plea), none could have been deemed less probable, perhaps, than the pen of the author of those cleverest of child-stories, "Alice in Wonderland" and "Through the Looking Glass." Yet, in the following lines, Mr. Louis Carroll makes fierce and effective onslaught on the advocates of suitably distributed State aid for the Endowment of Research, and on the "method of mutual admiration" by which they have striven to advance their cause—that is themselves:—

Fame's Penny Trumpet.

Blow, blow your trumpets till they crack,
 Ye little men of little souls!
And bid them huddle at your back—
 Gold-sucking leeches, shoals on shoals!

Fill all the air with hungry wails—
 "Reward us, ere we think or write:
Without your Gold mere Knowledge fails
 To sate the swinish appetite!"

And, where great Plato paced serene,
 Or Newton paused with wistful eye,
Rush to the chase with hoofs unclean
 And Babel-clamour of the sty!

Be yours the pay: be theirs the praise:
 We will not rob them of their due,
Nor vex the ghosts of other days
 By naming them along with you.

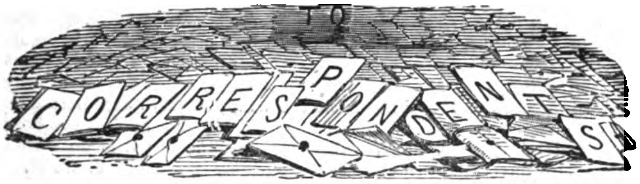
* * * * *

Go, throng each other's drawing-rooms,
 Ye idols of a petty clique:
Strut your brief hour in borrowed plumes,
 And make your penny trumpets squeak:

Deck your dull talk with pilfered shreds
 Of learning from a nobler time,
And oil each other's little heads
 With mutual Flattery's golden slime:

And when the topmost height ye gain,
 And stand in Glory's ether clear,
And grasp the prize of all your pain—
 So many hundred pounds a year—

Then let Fame's banner be unfurled!
 Sing Pæans for a victory won!
Ye tapers, that would light the world
 And cast a shadow on the Sun.



"Let Knowledge grow from more to more."—ALFRED TENNYSON.

Only a small proportion of Letters received can possibly be inserted. Correspondents must not be offended, therefore, should their letters not appear.

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PERPETUAL MOTION ON THE LARGE SCALE.

[1081]—In a recent number of KNOWLEDGE, p. 371-2, Mr. Proctor accuses me of assuming that perpetual motion prevails throughout the universe, and evidently regards such assumption as a grave delinquency; "really but a magnified mistake" of the same kind as that of the inventors who strive to create motion by means of mechanism.

I plead guilty of the assumption, and beg to state most distinctly that all the cosmical speculations that I perpetrated in the work referred to are based upon it. So fully is this assumption incorporated with all my conceptions of physical existence, that I cannot reason upon any physical problem, big or little, without it.

To those who are capable of being frightened by a phrase that has become a bugbear, this declaration may seem Quixotic, but if they will look a little deeper they may see that it is simply an expression of my conviction of the unassailable soundness of the broadest and grandest generalisation of modern science; and that if I err I do so in company with the greatest philosophers of this generation.

The word chosen by Mr. Proctor to express my fundamental idea is not a good one, but for that he is responsible. I have never used it; I have too much respect for her Majesty's English to speak of the "perpetuality" of motion, though the terms I prefer—viz., the "indestructibility," or "constancy," of motion distinctly imply it.

When Mr. Proctor can show me how motion can be destroyed, or ever has been destroyed; how the sum total of the motion existing in the universe can be increased or diminished by the equivalent to a falling feather, I will admit that all my conceptions of material existence are at fault, and all my philosophy is fiction.

That which cannot be destroyed, nor increased, nor diminished, must exist perpetually. W. MATTIEU WILLIAMS.

[The above letter should have appeared last week, but was unfortunately overlooked—having arrived too late for previous week—during pressure of Christmas work.

Mr. Williams rather oddly confuses error and offence. To point out a mistake is not to accuse of crime; and though Mr. Williams seems unaware of the possibility, he *may* occasionally err (without delinquency) like the rest of us. If any delinquency was suggested in my remarks on his mainly sound note in the *Gentleman's Magazine*, it was his attacking me (in what is sometimes held to be feminine fashion) without naming me, and without assuring himself that I had said what he attacked. For though I, jestingly, assumed he *must* mean somebody else, neither I nor any one else (if I can judge from letters sent me) mistook his real intent. I should have preferred a more candid and perhaps more careful criticism,—not caring much if it had been therefore more effective.

I am no more responsible for the words "perpetual motion" than Mr. Williams is responsible for the ridiculous expression "her Majesty's English." Nor does it greatly matter what name we give to the mistake by which Mr. Williams gets an infinite store of energy out of the sun's finite supply. Appealing to the constancy of the sum total of motion in the universe is as much to the purpose in the case of Mr. Williams's large perpetual machine as it would have been in the case of the small one rejected by Stephenson. In this matter Mr. Williams is no more in company with the greatest philosophers of this generation than he is in company with Newton in his theory of the generation of the solar system.—R. P.]

COLOUR OF THE SUN.

[1082]—I have looked at the sun every morning during the last few weeks through a small telescope without a sun-glass. On withdrawing my eye from the glass, everything I looked at had a yellow colour tending to orange, exactly similar to that seen when an indigo surface is looked at for some moments and the eye immediately allowed to rest on a white surface. A good deal has been written lately regarding the colour of the sun as observed in tropical regions. Would this yellow colour be complementary to that of our luminary? ALDEBARAN.

EXAMINATION OF AFTERGLOW.

[1083]—If any of the readers of your valuable paper have examined spectroscopically the brilliant afterglow which has been very marked lately, it would be of great interest if such observations were published, as they may tend to a satisfactory explanation of the phenomena. I have observed it through a small rain-band spectroscope, made by Browning, and was surprised at the very brilliant spectrum it produced, in which the whole of the *dark red* end was absorbed, while the other portions were particularly brilliant. In addition to this, I observed a wide black band in the centre of the visible red portion, and another about half as wide again just at the junction of the green and yellow portions. With this small instrument, I could not, of course, obtain any very definite results; but with a large one, with means of measuring the position, &c., of the bands, I believe some very interesting results may be chronicled, and I hope that if any have been obtained by observers, they will be published in your columns for the benefit of your readers who are somewhat puzzled at the late peculiar phenomena. T. H. DAVIS.

Peel, Isle of Man.

SUNSET GLOW.—SCIENCE OF THE "DAY."

[1084]—If the matter is of any interest, I would say that I watched the sunset here a few days ago, after the first snow-fall, but nothing unusual was seen. There was the ordinary twilight glow, but that was all. A casual observer would never have noticed any difference between that and the regular sunsets. Indeed, although I went home early to watch the sunset myself, I could see nothing unusual. The weather has been cloudy, with snow and rain ever since, so that no further observations could be made.

What do you think of the enclosed sample of "newspaper science"? The editor of this paper (*The Day*) has recently come in our midst, and has been attempting to teach us benighted people some new things. Among other matters he has been furnishing the public with some valuable (?) astronomical news, such as "The Comet was seen last night," "The Comet was again seen," &c. (You see it was necessary to let the public know that the Comet had not yet escaped). About two weeks ago (when the moon was nearly full), he startled us poor amateurs with the information, "The Comet was seen last night with the naked eye, and is moving steadily west." (!)

I rubbed my eyes, read it again, and asked myself the question, Can it be possible that an American newspaper editor has been the first to see the comet with the naked eye, and that, too, in full moonlight? Or, has the comet (having become alarmed at the publicity given to it by the Editor) turned tail, and gone towards the West, in its frantic efforts to escape? After thinking upon the matter, however, I concluded that he probably belonged to the advanced school of "Earth-flatteners," and having watched it with the naked eye (!), and ignoring the "damnable doctrine" of a round and rotating earth, had naturally concluded that it was moving in that direction.

He also "tackles" the sun spots, of which the enclosed is a sample.* I think he would be a valuable acquisition to the "Sun Spot Brigade" at Kensington. You ought to get him over there.

Baltimore, U.S.A., Dec. 24, 1883. W. H. NUMSEN.

[* The extract referred to runs thus:—"The *Day* of Saturday, in announcing that an unusually large number of spots could be seen on the sun, added that they were of the kind which usually accompany auroral displays and magnetic storms. The severe storm which prevailed all over the country yesterday shows the correctness of our deduction."]

THE GREEN SUN.

[1085]—Hartwig* relates that in 1831 a dry fog extended over a great part of the northern hemisphere, and gave sufficient light to enable small print to be read at midnight in August of that year.

* "The Aërial World."

During this fog the sun was observed to give light of a blue or green colour in the south of Europe and America.

It would be interesting to know whether such a fog prevailed over the Indian Ocean, or those parts of it from which the sun was recently seen of a green or blue colour.

The apparent connection between these two phenomena leads to the question, What is a dry fog? is it similar to the haze which is so characteristic of the east wind? (of which I have never seen any satisfactory explanation,) or is it due to some totally different cause? E. C. R.

[Dust-laden air is one form of dry fog].

THE LARGE METEOR.

[1086]—I think I saw the same meteor which "Excelsior" saw on Nov. 28 (see KNOWLEDGE, Dec. 14, p. 336). I chanced to be looking at Jupiter at 10.40, when I saw it come sailing up from the south, and it passed between me and Jupiter, and entered a very thin cloud near Ursa Major and disappeared. It was the finest meteor I ever saw; it was like a bar of red hot iron about 6 ft. long, with an intensely white-heated head, with purple colour where the body or tail commenced; and the body looked as thick as the head until it tapered off at the tail or end.

I thought the body or tail was only an optical illusion, caused by its rapid flight through the air. It was very low down, apparently not higher than two or three hundred yards, and its course was parallel to the ground. It did not look so large as "Excelsior" saw it, nor were there any sparks; perhaps it would increase much in apparent size and develop sparks in the twenty to twenty-five miles it would have to go before he saw it.

S. S. (Manchester).

STRANGE PHENOMENON (1068).

[1087]—Your correspondent "Lee Fore Brace," who sees "so many meteorological phenomena in your excellent paper," should have signed himself "The Modern Ezekiel," for his vision of wheels is quite as wonderful as the prophet's. These "wheels," which were 600 yards in diameter, would have been 2,000 yards in circumference, and having sixteen spokes (supposing them to have been equal distances apart) each spoke would have been separated by a space of 125 yards at the circumference, and, although the spokes resembled "the birch rods of the dames' schools," (by which, I suppose, we are to understand they spread out considerably) "the spokes could be distinctly seen all the way round," notwithstanding the fact that "each wheel made the revolution in about twelve seconds," which would give a velocity at the circumference of the wheel of 166⅔ yards *per second*. From the *nom de plume* he assumes it might be inferred that your correspondent is in the habit of "sailing close to the wind." This time I fancy he has "shivered his luff," and if he would avoid a "lee shore" he had better "go about." If I might be permitted to suggest a possible explanation of this wonderful phenomenon, I should say that between the time the "sun was over the fore-yard" and 11·30 p.m. there had been numerous accidents to the "*main brace*," and it had required "splicing" so often that a ray of light from a cabin window or elsewhere falling upon the water in the darkness assumed the appearance of having a rotary motion, hence the wheels. The "birch rods" would easily suggest themselves under the circumstances. A. McD.

STRANGE COINCIDENCE.

[1088]—One of my trawling vessels, the *Fairy*, of Lowestoft, while sailing through the gatway on leaving that port, on Dec. 11, by some unaccountable means lost her small boat, which was being towed astern. It turned bottom upwards, and broke the tow-rope. It being impossible in such a dangerous place to recover the boat, the vessel was obliged to return to harbour, in order to obtain another one. Having done so, the vessel proceeded on her voyage the same day.

At midnight that terrible storm burst upon them which caused such wholesale destruction amongst the fishing-vessels on the east coast.

Nearly every vessel that was fishing at the time lost the whole of their fishing gear, but my vessel, owing to the delay caused by the loss of the boat, had barely arrived at the fishing-ground when the storm came on, and thus saved her gear. After the storm, the *Fairy* proceeded on her voyage, and remained at sea for eight days' fishing, about 120 miles from Lowestoft. The master then shaped his course homewards, and had proceeded about forty miles, when, about 5.30 p.m., the crew felt their vessel strike something, and, on looking into the water, saw their own boat which they had lost eight days previously, Lowestoft then bearing W.N.W, distant eighty miles, showing that the tide had not in the least affected the direction in which the boat had drifted, but had drifted in exactly the same direction as the wind during the gale—*i.e.*, W.N.W. The boat had taken no harm, and was towed safely into Lowestoft harbour. The points that strike me as remarkable in this incident are:—1. That a boat sunk level with the water, and having only its keel exposed to the pressure of the wind, should have drifted so far in so short a time. 2. That the same vessel that lost the boat should strike it in the dark, when only a couple of yards on either side would have taken the vessel clear of it. J. W. H.

QUESTION IN ODDS.

[1089]—The following question in "Probabilities" came under my notice a short time ago. If of sufficient interest, I should be glad to see it inserted in your valuable paper; or, otherwise, a solution of it.

The odds against a certain horse winning a race are 2 to 1. Against another horse 40 to 1. What should the betting be against the two horses "coupled"—*i.e.* that neither will win?

JNO. A. JONES.

[Chance that former horse will win, $\frac{1}{3}$ or $\frac{41}{123}$; that second will win, $\frac{1}{41}$ or $\frac{3}{123}$. Chance that one or other will win is the sum these, or $\frac{44}{123}$; odds against the pair, 79 to 44.]

CYCLING.

[1090]—A correspondent in Edinburgh informs me that he considers my articles in KNOWLEDGE on tricycles are misleading from my not having been a bicycle rider, and proceeds to show me that it is not possible to pedal in the most effective manner on a "Humber" tricycle without you have learned to steer with your feet, without using your hands on a bicycle.

I wish to inform him, then, that I rode a bicycle round my dining-room table nearly twenty years ago; that I was constantly in the habit of riding with my hands in my pockets at that time, and even side-saddle, as a lady sits a horse, and that I often now ride my "Humber" tricycle without touching the handles, so that all the arguments in his long letter fall to the ground.

I have not omitted to notice that two correspondents have asked for information from me respecting the "Omnicycle." I have never actually had a day's ride on the "Omnicycle," but I have been three times on the road on machines of this make, and the more I know of them the less I like them. They are much heavier than any tricycle need be, they are lever machines, they are complicated, and, therefore, liable to get out of order, and it is difficult to alter the speed-gear while riding, though I know it may be done with practice.

No single tricycle which weighs about 100 lb. is likely to find much favour with JOHN BROWNING.

BICYCLES IN WINTER.

[1091]—Mr. Browning is mistaken when he thinks that bicycles slip upon ice and hardened snow in winter. Ice offers one of the best grips to rubber tyres that I know of; they never slip on it, strange as it may seem. That is the experience of many winter riders besides myself.

Mr. Browning recommends large tyres for winter riding. That would be all very well if the roads were constantly frozen, but how long do they remain so? My experience is that the smaller the tyres the better—they cut through the mud with an ease that must be felt to be appreciated.

Mr. Browning recommends low gearing for tricycles during the winter. From that I presume that he, after all, believes in large wheels for winter riding, and small ones geared up for summer. Is that so? I have a fancy that way also.

A large-wheeled machine is very steady on frost-bound roads among small ruts. S. RICE.

LETTERS RECEIVED AND SHORT ANSWERS.

A. McD. Thanks; your quaint letter inserted.—J. MARSHALL, G. R. There are several ways in which compensating pendulums of that kind are formed; see Sir E. Beckett's "Treatise on Clocks." —J. W. STAINFORTH—J. W. G. Only choice is between "cros" and "co"; but nearly every one says micros'copist.—DR. H. RICHES. Editor regrets, but unable.—C. W. F. states that distilled water can be obtained from the Salutaris Water Company, The Distillery, 236, Fulham-road, S.W.—NIGEL DOBLE. Are there cavernous openings, in a pile of blotting-paper, *par exemple?*—P. BICKNELL. Given at close of Editor's "Universe of Stars" (Longmans).—M. B. C. You have misapprehended both the Editor and Dr. Ball. The former never expressed objections to transcendental methods; the latter never did more than use formulæ which *would* have a meaning in space relations, if there were a fourth dimension in space. That is not an uncommon experience; but it is a very different thing from making use of a fourth dimension.

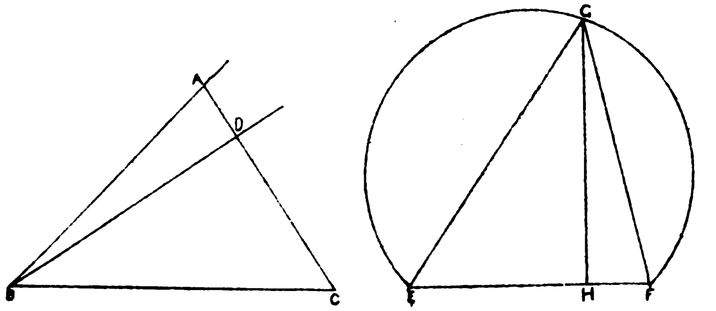
Our Mathematical Column.*

HINTS ON THE SOLUTION OF GEOMETRICAL PROBLEMS.

BY RICHARD A. PROCTOR.

(Concluded.)

AT present there remains only one class of deductions to deal with,—viz., those in which questions of *shape* are involved. There are many problems, which although sufficiently simple and easy, do not admit of being solved without a reference to the sixth book of Euclid; and there are others which are much more readily solved by means of the sixth book than without its aid.



Consider, for instance, the following example :—

Ex. 24.—A B C (Fig. 34, Plate I.) *is a given angle, it is required to draw a line* B D *so that when through any point* D *in* B D *a line* A D C *is drawn at right angles to* B D, D C *shall be equal to three times* A D.

Here is a problem clearly depending on *shape*—for instance, not on the *length* of B D or A C. We see that if we can divide any angle equal to A B C in the required manner our problem is solved; or rather, we have to construct a figure resembling A B C D as A B C D is *supposed* to be drawn.

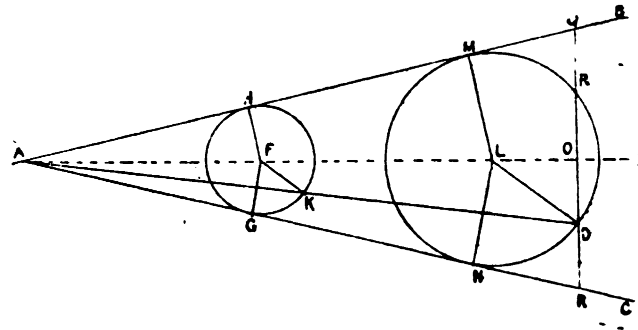
We notice that A D is one-fourth of A C and D B at right angles to A C. We therefore draw a straight line E F take H F equal to one-fourth of E F and draw H G at right angles to E F. All that is now required is that we should determine G so that the angle E G F may be equal to the angle A B C. This is readily effected since we know how to describe on E F an arc E G F containing an angle equal to the angle A B C (Euc. III. 33); the intersection of the line H G with the arc E G F gives us the required point G. We join E G, G F, then the angle E G F is equal to the angle A B C.

Now if we draw B D so that the angle A B D is equal to the angle H G F, then the remaining angle D B C is equal to the remaining angle H G E. Through any point D in the line B D thus obtained draw A D C at right angles to B D. Then obviously the triangle A B D is similar to the triangle G H F, therefore A D is to B D as F H to H G; similarly B D is to D C as G H to H E; therefore *ex æquali* A D is to D C as H F to H E. But H F is one fourth of E F; therefore H E is equal to three times H F; and hence D C is equal to three times A D.—Q.E.F.

We do not give the considerations which lead to the last lines of the proof. The considerations respecting shape which led to the construction may be looked on as obvious, although (as is often the case) it may not be quite so obvious how the *proof* is to be made to depend on properties established in the sixth book.

In a problem of the above type we cannot well avoid the use of the sixth book. I now give a problem which can be solved by the third book, but one can scarcely doubt that the solution depending on the sixth book is that which would *naturally* occur to a person dealing with the problem as a new one :—

Ex. 35.—*Let* A B, A C (Fig. 35, Plate I.) *be two straight lines meeting in* A, D *a given point. It is required to draw a circle which shall pass through the point* D *and touch the lines* A B, A C.



We first notice that the circle must have its centre in the line A E which bisects the angle B A C. For taking any point F on this bisector and drawing perpendiculars F H and F G on A B and A C respectively, we see that F H is equal to F G (since the triangles F A H, F A G are equal in all respects, Euc. I., 26). We describe a circle H G K with centre F and distance F H or F G, and touching A B and A C in H and G, Euc. III., 16. But this circle does not pass through D. It is obvious, however, that if we draw A K D, cutting the circle H G K in K, then the figure formed by the lines A H, A G, the point K, and the circle H G K exactly *resembles* that which will be formed when our problem is solved. If then we can only form a figure resembling that we have constructed but such that the circle shall pass through D, the problem will be solved. Now we see that F lies in a defined direction with respect to K,—in other words the angle A K F does not vary with the size of the circle drawn as H G K was drawn. We have then only to draw D L so that the angle A D L is equal to the angle A K F—that is, we have only to draw D L parallel to K F, in order to determine L the centre of the circle we require. The proof runs thus :—

Draw L M and L N perpendicular to A B and A C, respectively. Then, from the similar triangles A L D, A F K, L D is to A L as F K to A F. Again, from the similar triangles, A L N, A F G, A L is to L N as A F to F G. Therefore *ex æquali* L D is to L N as F K to F G. But F K is equal to F G; therefore L D is equal to L N—that is to L M. Hence a circle described with centre L at distance L D will pass through M and N, and touch A B and A C in these points, respectively, since the angles at M and N are right angles.

Note.—Of course there is no difficulty in solving this problem without the aid of any book beyond the 3rd. The obvious course of proceeding is by way of analysis. Let D N M be the required circle. A L E is the bisector of the angle B A C and can be drawn at once. R D O P Q at right angles to A E can also be drawn, and gives P a point on the required circle for D O = O P. Then one can hardly miss the relation that square on R N is equal to the rectangle under R D, R P; whence N is given, and the required circle, passing through the three known points, P, D, N, is determined.—Q. E. F.

* This paper concludes the Hints. It has been decided, in response to a great number of communications, to follow with a series of geometrical papers on the several books of Euclid, accompanied by very easy illustrative problems. It had been proposed to introduce between the Hints and these (long since prepared) papers, two series of papers of great interest on special methods: but so many readers have asked for papers on the more familiar subjects that the sub-editor has felt it but right to consider their wishes. As Mr. Proctor's papers already written extend to the middle of the third book, while the collection of illustrative problems runs as far as Prop. 34, Book I., there is no fear that the supply will run short before the Editor's two months' holiday is finished. Next week however a note on the old problem of Achilles and the Tortoise which the sub-editor found in Mr. Proctor's portfolio will appear.

DISTILLED WATERS.—It may interest readers of Mr. Dawson's article on the means of prolonging life, to know that for some years the Messrs. Packham & Co. have been engaged in the work of introducing pure waters, made exclusively from distilled water, and have established at Croydon a model factory for their production. Dr. B. W. Richardson, who took the chair at a meeting of the Society of Arts on Dec. 5, on the occasion of a paper being read by Mr. T. P. Bruce Warren, in which the use of distilled water for the manufacture of artificial mineral waters was strongly advocated, paid Messrs. Packham a visit a week or two since, and critically examined the whole of the processes, with which he expressed much satisfaction.

Our Whist Column.

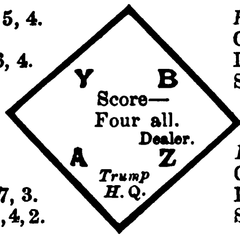
By "Five of Clubs."

The following game, from the *Westminster Papers*, illustrates the disadvantage often arising from leading trumps at the score Four all.

The Hands.

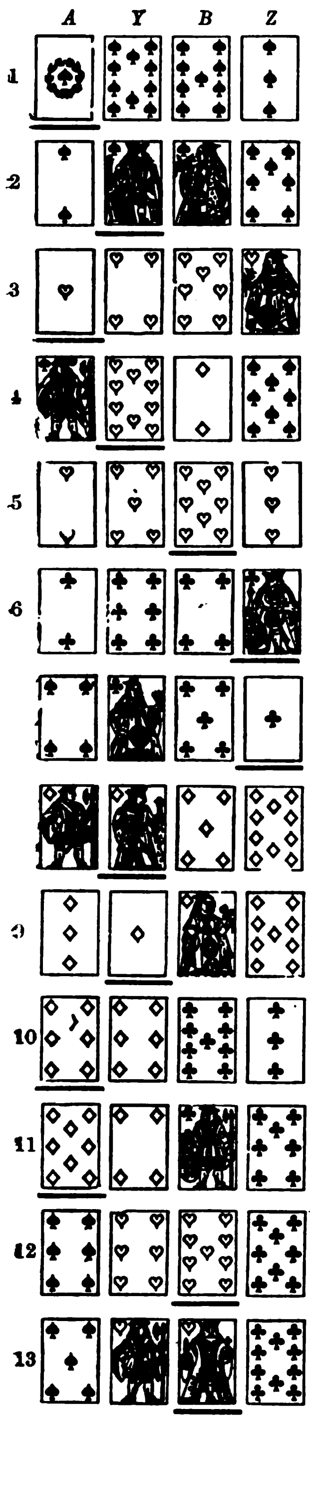
Y.
Hearts—Kn, 10, 6, 5, 4.
Clubs—Q, 6.
Diamonds—A, K, 6, 4.
Spades—K, 10.

B.
Hearts—K, 9, 8, 7.
Clubs—Kn, 9, 5, 4.
Diamonds—Q, 5, 2.
Spades—Q, 9.



A.
Hearts—A, 2.
Clubs—2.
Diamonds—Kn, 8, 7, 3.
Spades—A, Kn, 6, 5, 4, 2.

Z.
Hearts—Q, 3.
Clubs—A, K, 10, 8, 7, 3.
Diamonds—10, 9.
Spades—8, 7, 3.



Note.—The card underlined wins the trick, and card below leads next round.

Notes and Inferences.

1 and 2. *Y* and *B* hold no more Spades, and *A* holds the three lowest at least.

3. Leading trumps when the score is "Four all" is not generally advisable. In the present case *Y* should have led Diamonds—King, then Ace, then a little one; this his partner would have ruffed, leading King and Ace of Clubs, and then a little one, which *Y* would have ruffed (unless *A* played his trump Ace); then another Diamond lead would have forced the game, whether *B* played his trump King or not. This was the right line of play at the score.

4. *A* (the Editor of the *Westminster Papers*) rightly forces *Y*, and *B* plays well in passing the trick. The command in trumps thus reverts to him.

5. *B* safely plays the Eight, the Knave being certainly with *Y*.

7. *Z* sees that the tenace in Clubs remains with *B*; and apart from the discard at trick 4 he knows that *B* is weak in Diamonds. *Y*'s play at trick 4 shifted the trump strength, and *B* properly therefore discarded from his weakest suit in playing to that trick.

8. As the cards actually lie, a Club lead here would have won the game, or rather have shown *Y* an easy course to win. But *Z*, our skilful correspondent Mr. Lewis, would have played very ill—and, therefore, very unlike himself—had he led a Club here. He sees that the only chance left is that *Y* may be strong in Diamonds, but he has no just reason for supposing *Y* quite so strong as he is. The proper course is to lead through *A*, *B* being certainly weak in Diamonds. Thus *Y* gets the best chance of making two tricks in Diamonds, and winning.

But (8, 9, and 10) *Y* makes his two tricks in Diamonds, and has the game in his hands, yet throws it away. He knows certainly from *Z*'s play at trick 9 that the winning Diamonds are with *A*, and should have been able to form a shrewd guess that *both B*'s Clubs cannot be winning ones, or *Z* would not have played as he did. At any rate, the only chance of saving and winning the game lay here. Leading a Diamond at trick 10 was handing the game over to the enemy.

Our Chess Column.

By Mephisto.

OPENINGS.

Experience has proved that there is a defence to every attack, and that, therefore, an opening is only dangerous as long as it is imperfectly understood. The complicated and ingenious attacks of the Muzio and Algaier Gambits were in former times considered very formidable, and it was dangerous to meet these openings. Until within comparatively recent years the Evans Gambit attack proved stronger than the defence thereto. The combined skill of many thousands of even average players, represented by a corresponding number of games, has proved itself stronger than any of these attacks. With the increase of Chess columns to register the results of ordinary play—as in one game out of many chance or skill is certain to discover some new mode of meeting an attack—the strength of former favourite openings was gradually reduced until the attack became inferior to the defence. The Ruy Lopez is based upon sounder principles than any other opening. Analysis, therefore, as represented by practical play, has nevertheless modified the attack, as in the case of less sound openings it has completely demolished it.

We must, however, warn weaker players that but little reliance is to be placed upon the fact that such and such an opening is considered unsound. Unless the principles of it are known, it will, curious to say, have the opposite result of defeating the weaker player with greater expediency. We cannot omit to mention a comic incident which happened in our experience, showing the uselessness of relying upon the declared unsoundness of an opening to defeat a strong adversary. Scene, a Chess resort. A strong player and a Yankee contest some games together. Strong player offers the Algaier Gambit. Yankee exclaims, "Why, this attack is obsolete. You cannot play tricks upon me." Strong player continues unabashed, and defeats his mortified adversary.

Seeing, then, that the greatest danger proceeds from the ignorance of openings, we will endeavour to make our readers sufficiently familiar with the leading variation of every opening, to enable them to reach the middle of the game without falling a prey to the superior knowledge of an antagonist.

ANSWERS TO CORRESPONDENTS.

*** *Please address Chess Editor.*

Henry Bristow.—Your question in reference to No. 33 probably refers to the Black QBP. There are two grounds for making P takes P en passant impossible. Firstly, a P can only he taken en passant immediately on its being moved. Secondly, the P might have moved from Q3 to Q4, and not from Q2 to Q4, in which case it cannot be taken en passant at all.

S. Harvey.—Problems received with thanks.

S. B. B.—You probably meant R to B sq instead of B8.

W. S. Brindle, D. Matheson (not impossible), Richard Maurice.—If 1. R takes B, Black has a clever defence in 1. R takes BP, upon which there is no mate next move.

Alfred W. Brandon.—Solution incorrect.

E. R.—You can obtain diagrams of printers, 8, Salisbury-court, Fleet-street, E.C.

Correct solutions received.—Problem No. 109, Henry Bristow. No. 110, R. Willis, A. Conrad Smart, W. A. Bartels, Notnab, E. R., Kingsbridge, John, M. T. Hooton, W., H. A. N., A. Schmucke (thanks), Alex. S. Orr, George Johnson, Berrow, Rev. W. Anderson. Jas. Wade, London.

J. B.—In the first diagram Blackburne v. Appleton, p. 398, the Q got accidentally shifted from Kt3 to B3. After 34. Q to Kt8(ch), K to R2. 35. R to K8, P to Kt3. 36. P takes P, P takes P. You will see that, if the K takes P instead, 37. Q to Kt3 (ch) would win. (Thanks.)

Tom.—Problems received with thanks.

J. Dunlop.—You will find the problem quite right. Have forwarded your note. On the first move alone there may be 400 different positions in a game, each party having twenty first moves.

Contents of No. 114.

KNOWLEDGE
AN ILLUSTRATED
MAGAZINE OF SCIENCE
PLAINLY WORDED-EXACTLY DESCRIBED

LONDON: FRIDAY, JAN. 18, 1884.

CONTENTS OF No. 116.

EFFECT OF MARRIAGE ON LIFE.

(*Continued from page* 18.)

IT will not be denied that many men are prevented from marrying by ill-health or a weakly constitution. This may happen in more ways than one. A man may either, through ill-health, suffer by comparison with the hearty and stalwart, or he may feel that he is unfit to struggle with other difficulties than those he has as a single man,—that he is unable, perhaps, to provide for wife and children, or that the cares and anxieties which married life necessarily brings with it would be more than he could bear. For instance, the very thought of a crying child disturbing his night's rest would shake the nerves of an invalid. Or again, if a man is consumptive, or suffers under any other ailment which is apt to repeat itself in successive generations, he may well and wisely eschew the thought of marriage, fearing lest he should become the parent of unhealthy children. In these and in many other ways, unhealthy or weakly-constitutioned men fall into the list of bachelors. No one can fail to recognise the influence of this form of "selection" on the comparative mortality of the two classes we are considering.

Again, it cannot be doubted that very indigent persons and the members of unhealthy trades are, on the whole, kept somewhat from the lists of marrying men. Of course, hundreds of these marry in any given country; but comparing them as classes with other classes, there is, undoubtedly, such a tendency as we have mentioned. The influence of this cause, again, cannot be doubted, since the longevity of the classes we have named is undoubtedly inferior to the average longevity of the population.

Here, then, we see two causes (and many others might be mentioned) tending to add to the lists of bachelors classes of men of inferior longevity. How great the influence to be assigned to such causes may be it is not easy to determine, but we cannot doubt that the influence is an important one. At any rate, it is impossible to estimate the value of results in which allowance has not been made for influencing causes of this sort. So long as there is any suspicion that the classes from which we make our estimates are not equally balanced, no confidence can be placed in those estimates.

In fact, it is by selection of this sort that "facts and figures" have been made to "prove anything." We might prove that to have a title conduces to longevity, because the average age to which noblemen live is above the average for the whole country. We might prove that to be a lawyer or to be a Quaker is the great "elixir vitæ," for similar reasons. The argument against such assertions as these is, of course, similar to that we have applied to Dr. Stark's reasoning. The death-rate of the nobility is lower because they are as a class richer than the average population of the country, and therefore have more comforts, less occasion for entering on dangerous or unhealthy occupations, and so on. And similarly with the other cases adduced.

We might establish the very reverse of Dr. Stark's conclusion if, instead of taking the whole population of bachelors, we were to compare the death-rate of married men with the death-rate of the Catholic priesthood, or of Fellows of Colleges. And we should be making an error differing only in degree, not in kind, from that which, we submit, Drs. Stark and Drysdale made. We should be taking a set of men from which many unfavourable classes are *eliminated*, just as he has taken a set into which an unduly large number from unfavourable classes have been *introduced*.

But we have said that we do not consider Dr. Stark's evidence to be wholly without weight. We think the preponderance of deaths among bachelors is somewhat greater than was to be expected from the preponderant presence, among bachelors, of persons from the classes we have spoken of. We think, however, that what is proved by this preponderance is little more than what might have been reasonably anticipated. It would be idle to point out the variety of ways in which the life of a married man, or of a widower, has an enhanced value, since every one recognises the fact. He must be indeed unfamiliar with human nature, who is not aware that the mere love of life is no all-sufficient check upon recklessness. The consideration that others will suffer by our death, that wife or children will be left desolate, is a restraint on many men, who would but for this freely expose their lives to danger. The mere fact that marriage brings with it home duties and domestic habits, is of itself an important influence for good. That hundreds of married men neglect those duties and do not fall into those habits is true enough; but the fact that large numbers *are* kept within the home-circle, cannot but have an important influence on the death-rate. In individual instances "bachelor habits" may not affect longevity, but in taking the average of a large number, the truth will appear of the old French proverb, "Où peut-on être mieux qu'au sein de sa famille?"

That the *whole* of the preponderance exhibited by Dr. Stark is not due to the considerations just discussed seems to us to be very strikingly shown by the distribution of that preponderance among the different ages. It appears to us that the influence of the more regular habits which belong to the married state would not be likely to show itself most strongly between the ages of twenty and twenty-five, but would rather appear with a gradually increasing effect in successive quinquennial periods. On the other hand, the influence of the causes which keep within the list of bachelors large classes of short-lived persons, would undoubtedly show itself most at the earlier ages. For of men so weakly as not to be likely to survive the age of twenty-five scarcely any would marry; men whose expectation of life was somewhat greater would be somewhat less likely to remain unmarried; and so on. In other words, the list of bachelors would be more largely recruited from classes tending to increase the death-rates for the *earlier* quinquennial periods than for the *latter*.

In fact, Drs. Stark and Drysdale in their method of

treating the statistics afforded by general registration neglected those rules to which M. Quetelet, in his work on probability, called the particular attention of statisticians. The laws of probability applied to statistics afford evidence of the highest value, when suitable care is taken to exclude all influences due to selection and therefore not falling fairly within the province of probabilities. But when attention is not paid to such considerations, it becomes impossible to say what absurdities may not be proved by "facts and figures." In the case we have been considering the results are not absurd, it is true; but they are certainly exaggerated. We cannot accept Dr. Stark's conclusion that "bachelorhood is *more destructive to life than the most unwholesome trade, or than residence in an unhealthy house or district where there has never been the most distant attempt at sanitary improvements of any kind.*" But we *may* accept his opinion that "statistics have proved the truth of one of the first natural laws revealed to man,—It is not good that man should live alone." Whether the law required any proof is a question into which we need not enter; our readers must form their own conclusion on this point.

THE UNIVERSE OF SUNS.

By RICHARD A. PROCTOR.

(*Continued from page* 26.)

MICHELL did not enumerate any complete theory respecting the universe or even respecting the galaxy. We have seen how skilfully he discussed the observed laws of stellar distribution. His inquiries into this subject naturally led him into speculation respecting the arrangement of the star system into systems or groups; but he did not extend these considerations beyond those parts of the sidereal heavens which he regarded as probably nearest to us. He indeed mentions the nebulæ, but not so particularly as to determine their position in the scheme of creation.

The most important of Michell's theories is that which relates to the existence of laws of arrangement (even among the stars visible to the unaided eye), according to which the stars form systems, separated from each other by relatively vacant spaces. "We may conclude," he says, "that the stars are really collected together in clusters in some places, where they form a kind of system, whilst in others there are either few or none of them, to whatever cause this may be owing, whether to their mutual gravitation, or to some other law or appointment of the Creator." He then proceeds to inquire whether the sun "likewise makes one of some system." He considers that this is probably the case, and it is in endeavouring to separate those stars which probably belong to the same system as the sun from those which do not, that he has occasion to speak of the nebulæ. "There are some marks," he says, "by which we may with great probability include some and exclude others, while the rest remain more doubtful. Those stars which are found in clusters and surrounded with many others at a small distance from them belong probably to other systems and not to ours. And those stars which are surrounded with nebulæ, are probably only very great stars which upon account of their superior magnitude are singly visible, whilst the others, which compose the remaining parts of the same system are so small as to escape our sight. And those nebulæ in which we can discern either none or only a few stars, even with the assistance of the best telescopes, are probably systems which are still more distant than the rest." On the other hand, he infers that those stars "which being placed at a greater distance from each other compose the larger constellations, and such as have few or no smaller stars near them when examined with telescopes, belong probably to our own system." Variable stars, also, he regards as in all probability members of the system of stars to which our sun belongs. He does not assign a reason for this opinion, but from the place in which it appears we may infer that he regarded the changes of brightness among such stars as due to changes of distance, a cause which would necessarily be more effective in the case of stars relatively near to us. He judged that red stars are in reality much larger than we should infer from their brightness; "many of them also have been observed to have a proper motion of their own, which with several other concurrent circumstances tends to make it highly probable that they are some of the nearest to us," and therefore belong to our own system.

Michell makes some very interesting remarks on the probable appearance of the particular star-cluster to which our sun belongs, as supposed to be seen from the Pleiades. He considers two hypotheses respecting the extent of this cluster. According to one the cluster contains about 1,000 suns, according to the other about 350. He considers also two hypotheses respecting the relation which our sun-cluster bears to the Pleiades—making the mean distances between sun and sun in the two clusters equal in one theory, and the mean magnitudes equal in the other. We need not follow him in his reasoning on these considerations. Let it suffice to mention that adopting one set of considerations he infers that our cluster would subtend about two degrees as seen from the Pleiades, with no star bright enough to be seen by the naked eye and only about ten in a two-inch telescope; while if the other be adopted our cluster would subtend about twelve degrees as seen from the Pleiades, some ten stars would be visible to the naked eye, and all the stars down to the fourth magnitude belonging to the cluster would be seen in a telescope two inches in aperture.

It will be noticed that although Michell formed no definite theory of the Milky Way, his ideas respecting the stars spread over the celestial sphere imply a theory of the stellar system (including the Milky Way) somewhat resembling Lambert's. For Michell clearly understood that the stars are gathered into definite aggregations which are separated from each other by vacant or comparatively vacant spaces; and doubtless his opinion respecting the Milky Way was that that zone consists of a congeries of such aggregations, lying at different distances, but fore shortened so as to resemble a connected stream or series of streams. Michell's ideas differed from those of Lambert, however, in this respect, that Michell did not regard the different systems of stars composing the complete sidereal system, as either regular in shape, or similar to each other in figure, dimensions, or constitution. He judged rather from observed facts, and attentive reasoning thereon, than from preconceived opinions as to the uniformity of creation. In this respect he appears to me to have had the advantage of all who had preceded him in theorising respecting the universe. His views also appear altogether more worthy of attention than the theories commonly ascribed to the Herschels in our text-books of astronomy. Regarded as theorisers respecting the sidereal universe, the Herschels deserve the pre-eminence assigned to them, but not for the reasons commonly assigned. If Sir W. Herschel, in particular, had in reality maintained even to the close of his career as an observer, the theory of the universe commonly associated with his name, I believe that we should have to

assign a far higher place to Michell than to Herschel as well for acumen in interpreting observed facts as for skill in combining such facts into a consistent theory.*

(*To be continued.*)

GREEK FIRE.

AT what period the ancient Greek fire was invented has never been certainly determined. There are many writers who place the invention in a far antiquity. Historical details have been adduced pointing to the period of the earlier wars between the Greeks and Romans as the true era of the discovery. But we do not find any certain evidence of the use of Greek fire until the sieges of Constantinople, in the seventh and eighth centuries, though a Father of the Christian Church, writing in the fifth century, gave receipts for making a combustible substance of similar qualities from the compounds resin, sulphur, pitch, pigeon's-dung, turpentine, and the juice of the herb "all-heal."

It is related that the true Greek fire was invented by a certain Callinicus, an architect of Heliopolis, in Syria (Baalbec), in 678. The secret of the composition of this artificial flame, and the art of directing its action, were imparted by Callinicus—who had deserted from the Caliph—to the Emperor of Constantinople. From this period until the year 1291 the use of Greek fire was an important element in the military power of the Byzantine empire. The progress of the Saracens was, more than once, decisively checked by the destructive action of this powerful and terrible flame. The important art of compounding the fire "was preserved at Constantinople," says Gibbon, "as the palladium of the state: the galleys and artillery might occasionally be lent to the allies of Rome; but the composition of the Greek fire was concealed with the most zealous scruple, and the terror of the enemy was increased and prolonged by their ignorance and surprise."

The accounts which have reached us respecting the properties of the Greek fire are such as to justify the high value attached by the Byzantine emperors to the secret of its composition. It was a liquid, which was propelled by various methods against the ships or engines of the enemy. So long as it was kept from the air, or remained in large masses, the liquid appears to have been perfectly safe from combustion; but as soon as it was poured forth it burned with an intense flame which consumed everything around—not merely burning upwards, but with equal fury downwards and laterally. Water not only failed to quench it, but made it burn with new ardour. To subdue the flames it was necessary to employ, in large quantities, either sand or vinegar. Various methods were employed for propelling the liquid fire towards the enemy. Sometimes it was enclosed in vessels made of some brittle substance, and these were flung at the enemy by means of suitable projectile machines. "It was either," says Gibbon, "poured from the rampart in large boilers, or launched in red-hot balls of stone and iron, or darted in arrows and javelins, twisted round with flax and tow, which had deeply imbibed the inflammable oil." But the effectual use of the destructive compound seems to have been best secured by means of a species of fire-ships specially constructed for the purpose. Copper and iron machines were placed in the forepart of these ships. Long tubes, fantastically shaped, so as to resemble the mouth and jaws of savage animals, formed the outlet for a stream of liquid fire, which the engine—literally a fire-engine—propelled to a great distance. Hand-engines were also constructed by which the destructive compound could be spurted by the soldiers, Beckman tells us.

The secret, as we have said, was carefully kept by the Byzantines. The emperor Constantine suggested the answers which in his opinion were best fitted to elude the pertinacious questioning of the barbarians. "They should be told that the mystery of the Greek fire had been revealed by an angel to the first and greatest of the Constantines, with the sacred injunction that this gift of Heaven—this peculiar blessing of the Romans—should never be communicated to any foreign nation; that the prince and subject were alike bound to religious silence under the temporal and spiritual penalties of treason and sacrilege; and that the infamous attempt would provoke the sudden and supernatural vengeance of the God of the Christians." Gibbon adds that the secret thus religiously guarded was "confined for above 400 years to the Romans of the east; and at the end of the eleventh century the Pisans, to whom every sea and every art were familiar, suffered the effects without understanding the composition of Greek fire."

This, however, is not wholly true. The secret was preserved, indeed, from the Romans of the west, but the Saracens managed to possess themselves of it very much earlier than Gibbon's account would imply. For, at the siege of Thessalonica, in the year 904, the Saracens—we are told by John Comeniata—threw liquid fire, by means of tubes, upon the wooden defences of the besieged, and by this means principally succeeded in capturing the town.

In the Holy Wars the Mahometans freely availed themselves of the use of Greek fire. Gallant knights, who feared little the swords or lances of the Saracen host, were terrified by the uncouth aspect and the hideous noises of machines which belched forth upon them a torrent of liquid fire. Joinville tells us that "it came flying through the air like a winged long-tailed dragon, about the thickness of an hogshead, with the report of thunder and the velocity of lightning; and the darkness of the night was dispelled by this deadly illumination."

It does not by any means follow, because the invention of gunpowder rendered the ancient Greek fire no longer a very useful military weapon, that the knowledge of the secret of its composition would be of little value. We must remember that the use of firearms rendered the old-fashioned engines, by means of which the liquid was propelled, no longer available, since those who worked the engines could no longer venture near enough to the enemy. It was to this cause, we suspect, rather than to any want of efficiency in the compound itself, that the discontinuance of the use of Greek fire should be ascribed. The time had not yet come for making gunpowder itself a useful adjunct to the employment of liquid flame.

It is not so clear, however, that the ancient Greek fire was much more efficient than that which has recently come into use. Still, the inquiry into the nature of its composition is not without interest.

The Princess Anna Comnena states that Greek fire was compounded of sulphur, resin, and oil. It may be well to dwell on this point, since many writers have been disposed to consider naphtha, or liquid bitumen, to have been the principal ingredient of the Greek fire. Possibly, however, the oil mentioned by Anna Comnena may have been naphtha, and not, as one would be disposed to infer, any of the ordinary vegetable or mineral oils; for the use of naphtha in lamps is of great antiquity.

Gibbon writes:—"Naphtha was mingled, I know not in

* Of course I make no reference here to Sir W. Herschel's fame as an observer, which would remain unaffected whatever opinion we might form respecting his theories of the universe.

what proportions, with sulphur and with the pitch that is extracted from evergreen firs—that is, resin—in forming Greek fire."

It is a moot point whether Friar Bacon ever discovered the true composition of the liquid fire. Many supposed that he concealed a real ignorance on the subject, when he supplied an apparently unmeaning answer to the questions addressed to him. Others, however, assert that two of the components of Greek fire were, as Bacon said, sulphur and saltpetre, and that the third is to be detected in the logogriph—"Luru vopo vir Can utriet." We leave this anagram to the ingenuity of our readers, mentioning, in passing, that it contains the apropos words "urit voraciter," but that the extraction of these words leaves us only the combination "lupovun" from which it will not be found easy to form a word. Possibly there is a mistake in transcription to add to the anagrammatic difficulty.

Many others have tried to elucidate the question. Friar Bungay, Charles du Frêne, Ducange, and Joinville—a host, in fact, of commentators, historians, and antiquarians—have all had something to say more or less to the purpose. But the satisfactory solution of the problem has not yet been obtained, nor, perhaps, is it likely to be.

It has been well remarked by a writer on the subject, that "gunpowder blew the ancient Greek fire out of the field." But, during the American War of 1860-65, it was shown that gunpowder might be used to blow modern Greek fire into cities. Whether the example will ever become a recognised military precedent is uncertain. But it has been shown that Greek fire may be flung into a city by means of a suitably prepared shell, and that its destructive properties may thus be made available when the besieging force is four miles or more from the central parts of the city. Charleston was certainly not destroyed by General Gilmore's fire-shells; in fact, there are difficulties connected with the construction of such shells, which, though far from being insuperable, were not wholly mastered by the artillerists under Gilmore. But that an immense amount of damage was effected is shown by the fact that General Beauregard hurled from the mouth of his cannon denunciations against Gilmore for employing "the most villanous compound ever used in war."

That Greek fire will one day be employed as a fearfully destructive agent in warfare seems scarcely probable. Yet, so far from looking forward with dismay to the prospect of such an application of its properties, we may rather, perhaps, consider that prospect as favourable to the interests of peace. We may apply to this case the remarks applied by Fuller to the use of cannon:—"Though some may say that the finding of such appliances hath been the losing of many men's lives, yet will it appear that wars are now fought with more expedition, and that victory standeth not so long a neuter, before she express herself on one side or other."

A COURSE of six lectures on "Primitive Man" will be delivered by Sydney B. J. Skertchley, Esq., F G.S., M.A.I., on successive Tuesday evenings, commencing Jan. 22. With a view to extending the advantages of these lectures to working-men and women, the committee have decided to issue tickets, admitting to the gallery, at one shilling for the course, or threepence to any single lecture, and they request the assistance of secretaries of working men's clubs and trade organisations in making these lectures known amongst their members. Tickets may be obtained of any member of the Lecture Committee, at the Institute, on the evenings of the lectures, or upon application by post to the honorary secretary, Conrad Thies, 76, Graham-road, Hackney, E.

HOW TO MAKE USEFUL STAR MAPS.

BY RICHARD A. PROCTOR.

(*Continued from page 6.*)

THE plan described in our last is the mathematical plan. The plan I always adopt is less scientific, but easier and far more accurate.

Having obtained the lines K N and L M (Fig. 2), with the division-points along them,* as at first, I place a piece of

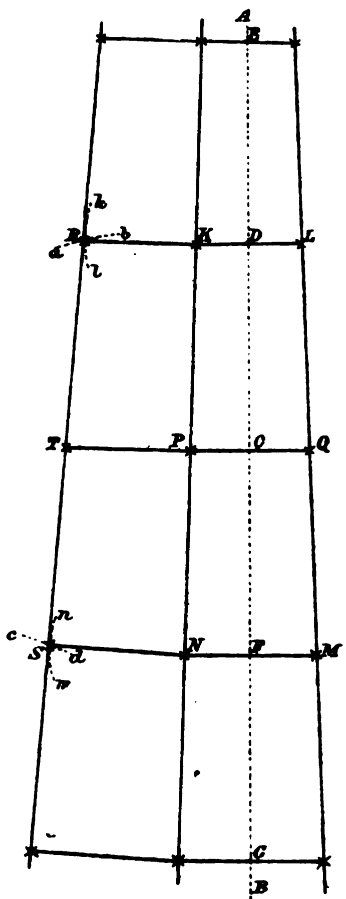


Fig. 2.

tracing-paper upon the figure and mark in all the division-points. Then I shift the tracing-paper so that the division-points at L and M fall respectively on K and L, and of course the division-points which had been at K and L now fall on R and S respectively. With a fine pointer I mark the five points along this line, *through* the tracing-paper. Then shifting the latter so that the points originally at L and M fall on R and S, I repeat the process. Lastly, I do the same on the right-hand side of A B, and the points wanted are thus obtained with the utmost ease and certainty.

The meridians being straight, nothing need be said about the way of drawing them in. The parallels are parts of circles, and when the meridians corresponding to K N and L M intersect at a convenient distance, the point of intersection is to be used as the centre of these circles. But

* It will have been noticed that K L and N M (not their *halves*, as stated at foot of p. 6) are equal respectively to fifty and sixty-four hundredths of D C. Also, obviously K L corresponds to 50°, not 40°.

commonly this is not the case. When this is so, one may either simply join R K, K L, &c., with straight lines, or if the scale of the maps is so large that this has an unsatisfactory look, the plan indicated in Fig. 3 may be followed.

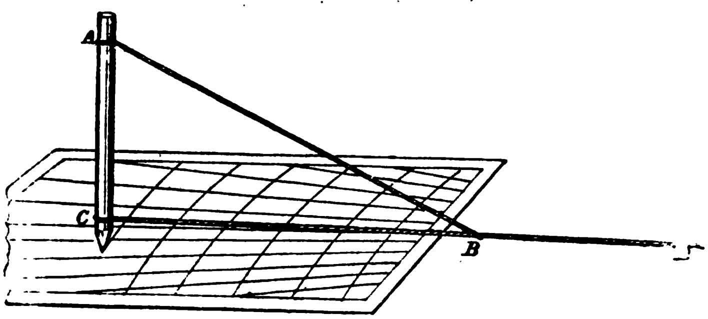


Fig. 3.

This plan ought to be given in every treatise on mapping, but I have never seen it. It is quite general, and I have repeatedly found it exceedingly useful.

Suppose A, B, C, and D to be points on a curved line, and we wish to represent the curved line more accurately than by joining A B, B C, &c.:—Place the edge of a ruler across B so as to fall equally distant (the eye being sufficient judge) from A and C, as at a and c. With the ruler thus placed draw the line $b' b''$, so that B b' may be about one-fourth of A B. Do the line at C. At A put the ruler so that B b may be about equal to A a, and draw $a' a''$. Similarly draw $d' d''$. Then join $a'' b'$, $b'' c'$, $c'' d'$, and the curved line will be represented *four times* as accurately as by joining A B, B C, &c.

Notice also that all the lines thus drawn are tangents to the true curve, and by a very simple contrivance we can (if necessary) increase the accuracy of the construction yet again *fourfold*. Suppose $c'' d'$, one of the connecting lines mentioned in the above description, e its middle point. Then if we join the middle point of C c'' with the middle point of c'' e (the eye being in all such cases quite sufficient judge), and do the like at every corner of our polygonal line, we shall obtain a construction sixteen times as accurate as that given by simply joining A B, B C, &c.

Where the scale of a map is large these methods of approximation are very important. It need hardly be said that all should be done in pencil first.

But the following plan for describing circular arcs of large section is commonly the best for the kind of maps I have been describing above, in which the parallels are circles:—

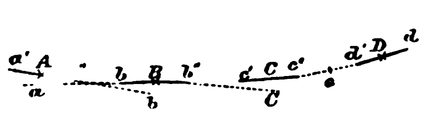


Fig. 4.

The figure (Fig. 4) explains the plan so clearly that there is little occasion for verbal description. The object of the plan is to secure fixity of position for a pencil attached to an inelastic string. The parts A B, BC are in one piece, a knot being tied at B, so that when the parts A and C are slipped to their places on the pencil (where a notch should be cut) A C B may be a right angle. To the knot the cord B D is attached. The pencil must be held so that the cords A B, B C may both be stretched; the centre for the points of the parallel, which are already marked in, is easily found by trial; then the string is there held down by the thumbnail while the upright pencil is carried round the parallel.

I have found this method very handy and accurate for all large-scale maps. Of course a pen may be substituted for the pencil, but I commonly use the pencil only as a guide for inking in the circular arcs I may be drawing.

Having completed the meridians and parallels of his large-scale chart, the student can have no difficulty in filling in the stars, either from his small-scale atlas or from any catalogue he may possess. I use the British Association Catalogue, which contains many stars not visible to the naked eye. But such a catalogue as that in my "Handbook of the Stars"—in which all the stars from the British Association Catalogue down to the fifth magnitude inclusive are collected in constellations—will be found sufficient for nearly all practical purposes. In fact I originally drew up that catalogue and had it long by me in MS. for mapping purposes. In mapping from a chart it is necessary to notice that the figure of a space on the chart may differ altogether from the true figure of the corresponding space in Fig. 1. In the maps by the Society for the Diffusion of Useful Knowledge, for instance, the angles of the spaces often differ very much from right angles. In this case, when marking in the stars from such a space, the mappist must remember to estimate the distance of each star parallel to the sides of the space in which it appears. He must, in fact, conceive two lines drawn through the star, parallel to the sides of the enclosing space, and notice in what proportion these lines are divided by the star. In the corresponding space in his map the star must divide the corresponding lines in a similar proportion.

The mappist will find it convenient to have a simple way of indicating star-magnitude. I know of none better than the following:—

Stars of the 1st, 2nd, 3rd, 4th, and 5th magnitudes by eight-pointed, six-pointed, five-pointed, four-pointed, and three-pointed figures respectively, with ray-lines between the points; stars of the 6th magnitude by a tiny cross, with fine lines between the rays.

THE MORALITY OF HAPPINESS.

By Thomas Foster.

(*Continued from page 255, Vol. IV.*)

SELF *VERSUS* OTHERS.

WE teach our children, the preacher tells his flock, but few follow the precept,—Care more for others than for self. It sounds a harsh doctrine to say, instead,—Each must care for himself before others. Yet it is not only true teaching, it is a self-evident truth. It would not be even worth saying, so obviously true is it, were it not that in putting aside the doctrine because of its seeming harshness men overlook or try to overlook the important consequences which follow from it.

If a man's whole soul—nay, let me speak for a moment in my proper person—if my whole soul were filled with the thought that my one chief business in life is to make those around me, as far as I can make my influence felt, as happy as possible, to increase in every possible way the stock of human (nay, also of animal) happiness, I must still begin by taking care of myself. For if through want of care I myself should cease to exist, I can no longer in any way serve others; nay, it is even conceivable that my immature disappearance from the scene of my proposed exertions for others' benefit might cause some diminution of the totality of happiness.

If the very thought of care for self should suggest that there can be no real love or care for others where self-care

comes first (self-evident though the proposition be that care of self *must* come first), let us replace the case rejected as imaginary, by a concrete and familiar illustration.

None can question the unselfishness of the love which a mother feels for her infant babe. None can doubt that if question arose between the babe's life and her's, her own life would be willingly sacrificed. Of course there are exceptions,—perhaps many,—but no one can doubt, and multitudes of cases have proved, that the rule holds generally. Now the nursing mother not only has, in her very love for her babe, to take care of herself, but to care for herself *first*, and to take more care of herself than, but for her pure, unselfish love for her child, she would have troubled herself to take.

Let this case suffice to show that care of self before others (not therefore necessarily more than others) besides being a self-evident duty (which many may regard as a mere trifle), may be not only perfectly consistent with regard for others and even with devotion to others, but may be absolutely essential to the proper discharge of our duties towards others. In fact, it is little more than a truism (instead of being, as many would at a first view imagine, a paradox), that the more earnest our wish to increase the happiness of others, the more carefully must we look after our own welfare.

If we take a wider view, and instead of considering a single life study the development of families and races, we still find the same lesson. As the man who wishes his life to be useful to his fellows and to increase their happiness must take care of that life, so he who would wish to benefit humanity through his family or race, must not only nourish his own life and strength, but must develop those activities which advance his own welfare and the welfare of his family. Otherwise come inevitably the dwindling of the faculties on which his own value depends, and the loss in his descendants of good qualities which they might otherwise have inherited from him. Or it may be that such qualities are inherited in less degree than had he duly exercised powers and capacities which were in a sense held in trust for them. We are apt to overlook the importance of individual action in such cases, not noticing that the progress of a race depends on the aggregate of acts by the individual members of the race.

To take a concrete instance here, as of the simpler case, —If a number of persons in any nation or at any epoch, impelled by a desire to benefit their fellows, devote their lives to celibacy, they influence in important degree the qualities of the next and succeeding generations. They diminish the proportion in which their personal qualities—presumably valuable—will appear in future generations, and relatively increase the proportion of other and less desirable qualities. This is obvious enough. It should, however, be almost as clear, that in whatever degree such persons in a community as possess the best qualities, fail to advance, in all things just, their personal interests, they diminish the influence of the better qualities, not only in their own time, but in times to come. If—to take another concrete example—all persons of the better sort, forgetting their duties to themselves and their race, enter of set purpose on lives of poverty, asceticism, and dreariness, they not only diminish in large degree the good they might do during life, but they injure their offspring, and, through them, posterity.*

Under its biological aspect then the doctrine that care of self must necessarily take precedence of care and thought for others, is incontestible—it is the merest truism—though many speak, and some act, as if the doctrine were iniquitous.

But this doctrine has its moral aspect also. The question of duty comes in at once and very obviously so soon as the actual consequences of conduct have been shown to be good or bad. But it may be well to show more definitely what the true line of duty is in regard to self. I shall therefore next consider cases where self-abnegation leads directly to the diminution of general happiness.

(*To be continued.*)

* Many would probably be startled if a just estimate could be formed of the degree in which the qualities of the civilised races of the world have suffered through the well-meant but mistaken zeal which led large classes of men in former ages to sacrifice their *power* to do good in *order* to do good.

CAUSES OF THE GLACIAL PERIOD.

WHEN we endeavoured to briefly summarise the "Evidences" of the Glacial Period, it was our privilege in every instance to have to do with facts; when, however, we come to inquire into the "causes" of that period, we enter, to a greater or less extent, the field of speculation and theory. Nevertheless, such inquiry is of interest and importance, because it incidentally enables us to assign a more or less probable date and duration of time to the Glacial Period, which are of great use in furthering our calculations concerning other questions, such as the antiquity of man, &c.

Before proceeding with our inquiry, I may premise that some of the animal remains found in the glacial deposits and in caves would lead us to infer that intervals of mitigated severity occurred during the Glacial Period. For example, in England, besides the remains of the mammoth, hairy rhinoceros, reindeer, and other Arctic animals, are also found remains of the hippopotamus, lion, and hyæna, these latter animals being not at all Arctic in character. Now these animals, not being able to endure severe cold, must either have been only summer visitors during the severity of the Glacial Period, or else they must have been permanent residents in our country during mild intervals of the same period; and as we do not know many instances of large animals performing summer migrations like birds, the latter alternative offers the best explanation of their occurrence. I mention these facts here because in our investigation of causes it will be useful to bear them in mind, and to remember that any cause or causes which would explain such mild intervals of climate during the Glacial Period would also explain the presence of these animal remains.

Now, the Glacial Period was in all probability not due to one cause alone, but rather to a combination of many causes, both astronomical and geographical. We will consider first the more probable astronomical causes:—

1. *The Excentricity of the Earth's Orbit*, to the explanation of which term I now proceed. The earth moves round the sun, not in an exact circle, but in a somewhat egg-shaped or elliptical path called its orbit. But this orbit is not at all periods the same in shape. Sometimes it is more elongated; at other times it more nearly approaches the form of a circle. When the orbit is elongated, it is said to have a great excentricity; when nearer the circular form, it is said to have a small excentricity, these variations in the shape of the orbit being due to the unequal attraction exerted upon the earth by the other planets of the solar system, especially Jupiter and Saturn. Thus, when at that part of its orbit most distant from the sun, the earth is much farther away from that body during great excentricity than during small. Consequently, any part of the earth having its winter when the earth was farthest distant from the sun during great excen-

tricity would, no doubt, have comparatively a very severe climate, and this may have been one cause of the Glacial Period. At the present time, the excentricity of the earth's orbit is very small,* the difference between the earth's distance from the sun at its nearest and farthest points being only *three* million miles; whereas, during times of great excentricity that difference may amount to *thirteen* million miles. By mathematical calculations based upon the known attraction exercised upon the earth by the other planets, and the known position and movements of these planets, it is calculated that a period of great excentricity would occur, to a greater or less degree, between 210,000 and 100,000 years before the present time. Therefore, in all probability, it was during that time that the Glacial Period occurred.

2. At the present time, however, the winter in the northern hemisphere occurs when the earth is *nearest to* the sun, or, as it is called, in *perihelion*; the effect of such nearness to the sun being nevertheless more than counteracted by the shortness of the days during the winter season, due to our hemisphere being then turned away from the sun. But there is a movement known as the *precession of the equinoxes*, caused by the attraction of the sun and moon upon the protuberant portion of the earth at and near the equator, whereby the direction of the pole is made very slowly to describe a circle in the heavens causing the equinoxes, and with them the other seasons, to come round a very little sooner than they otherwise would. And there is another movement known as the *revolution of the apsides* (the apsides being the points in the orbits nearest to and farthest from the sun) caused, like excentricity, by the attraction upon the earth of the other planets, whereby these apsides, or points in the orbit are constantly changing their places. These two changes combined cause the seasons to occur successively in every part of the earth's orbit, so that sometimes the winter of the northern hemisphere has occurred and will occur again when the earth is *farthest from* the sun; or, as it is called, in *aphelion*. Now, if this state of things occurs when the excentricity of the orbit is small, its effect upon climate is probably not very great; but when it coincides with a period of great excentricity, important results may be the consequence. For, as the earth must of necessity move more slowly the farther it is from the sun, it will be obvious that during such periods of coincidence not only would the winters be more severe, but they would also be longer in duration, and consequently large masses of snow and ice would accumulate which the short summers would be unable to melt. These short, warm summers would engender fogs, phenomena which all Arctic travellers have experienced, and these fogs would cut off the rays of the sun from melting the ice. The snow also, by reflecting a great part of the sun's rays, and the ice, by absorbing the same before it could be thawed, would prevent a large portion of those rays from ever reaching the earth's surface at all. Moreover, a cold condition of the northern hemisphere generally would probably induce a change in the winds and currents of the globe (the currents, of course, being chiefly dependent upon the winds), and instead of southerly winds and currents which have traversed tropical regions, warming our atmosphere as at present (when an opposite state of things prevails, the southern hemisphere being now the colder) there would probably be a greater number of winds and currents flowing down from the icy north to the less cold south, and these would leave the cold north colder still. So that we see all these things tend to show us that the northern winter occurring in aphelion during a period of great excentricity would be very favourable to the occurrence of a Glacial Period in our part of the world.

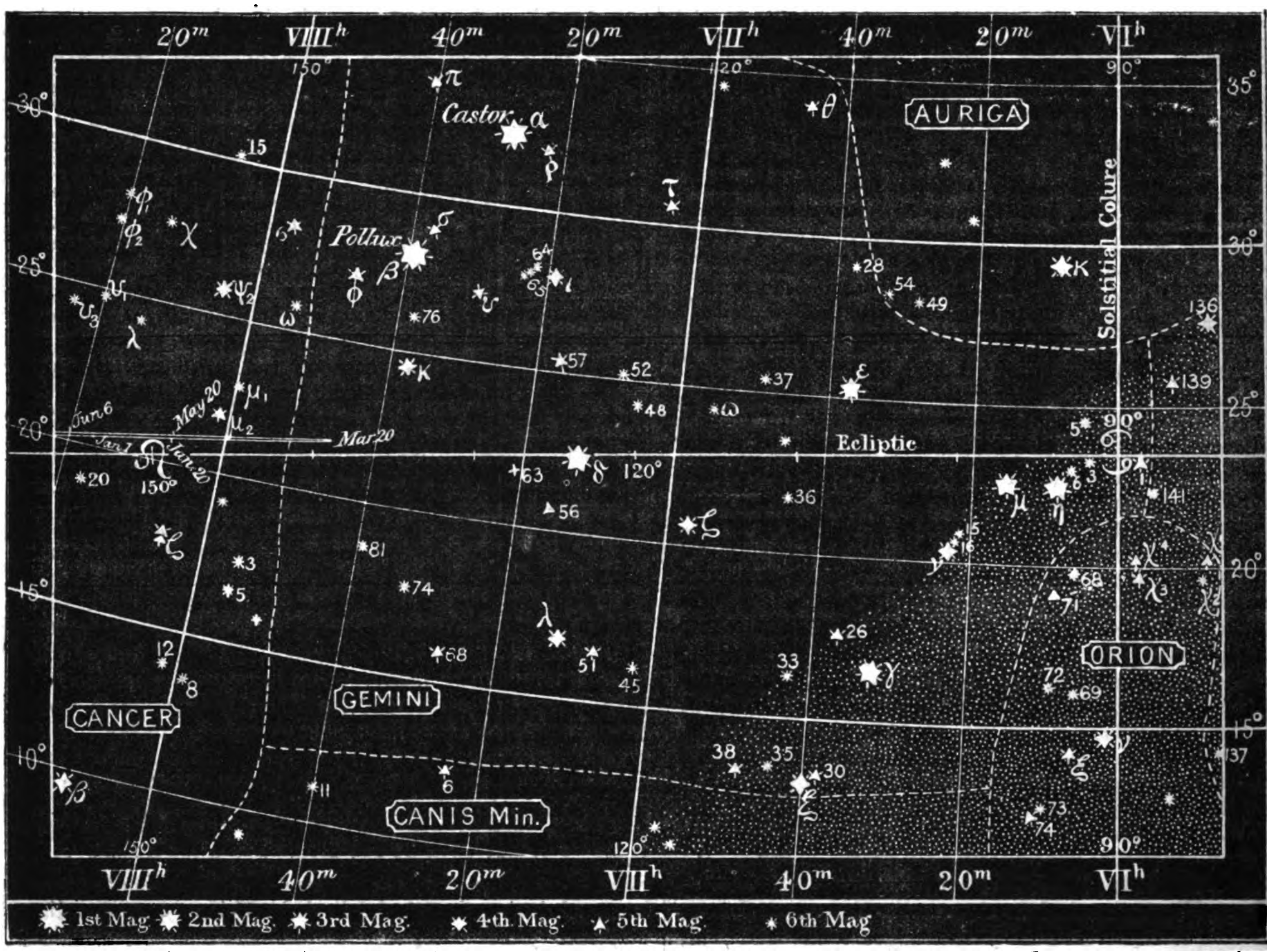
But here an interesting part of the problem presents itself to our notice. I have said that owing to two movements of the earth, known as the precession of the equinoxes and the revolution of the apsides, the seasons are brought about successively in every part of the earth's orbit. The time taken for this cycle to be completed is 21,000 years. During that period of time the seasons will have occurred at every part of the orbit; once, therefore, they will have occurred in perihelion, and once in aphelion. I have also said that a period of great excentricity is shown by calculation to have extended, to a greater or less degree, from 210,000 years to 100,000 years before the present time, that is, for a duration of 110,000 years. Now if the cycle of 21,000 years be divided into this period of 110,000 years, we shall find it will go five times, that is to say, that during the supposed duration of the Glacial Period five times would the northern winter occur in perihelion and five times in aphelion. Thus, as it is obvious that the winters occurring in and near perihelion must have been milder than those occurring in and near aphelion, we should have during the 110,000 years, the supposed duration of the Glacial Period, five somewhat lengthy periods of comparative mildness when the northern winter occurred at and near perihelion. Now we have seen that the remains of certain animals found in glacial deposits, such as the hippopotamus, lion, &c., indicate a mild climate, while those of others, such as the mammoth, reindeer, &c., indicate a cold one. May not these different climates have occurred during the Glacial Period at times corresponding respectively to the northern winter in and near perihelion, and the northern winter in and near aphelion? At any rate, I think this explanation more probable than that the differences of climate were due to the elevation and depression of land during the Glacial Period, a supposition we shall treat of more fully when we come to consider the geographical causes. There is just one other thing that I should like to mention before leaving these astronomical causes. The experience of many arctic travellers has shown us that no condition of things can be more favourable to a Glacial Period than a short, hot summer and a long, cold winter. We have seen that these are just the conditions most likely to obtain under a period of great excentricity, while the fluctuations in climate caused by the different positions of the earth during such period goes far to explain the singular fact of the great variety of animal remains found together in England and elsewhere, and the milder intervals in climate which some of these remains indicate. The name of "Interglacial Periods" has been given to these mild intervals in the climate.

In our next article we shall proceed to consider the geographical causes, and some less probable astronomical causes which have been alleged by some as playing a part in bringing about the great Glacial Period.

ROBERT B. COOK.

* See the figure illustrating the article on "The Earth's Path Round the Sun." It should be noticed that the departure of the earth's orbit from the circular *form*,—or what is called the *ellipticity*,—is very slight even when the *excentricity* is at its greatest. This will be illustrated later in a picture showing the earth's orbit when most excentric.

COPPER FROM ARIZONA.—Last year Arizona produced seventeen million pounds of copper. Thus far, during the present year, the increase has been 38 per cent., and new furnaces are being erected. The output from Arizona for the year 1883 will probably be not less than 25,000,000 lb.



Zodiacal Star-Map for January (showing Jupiter's Path).

ZODIACAL MAP FOR JANUARY.

By Richard A. Proctor.

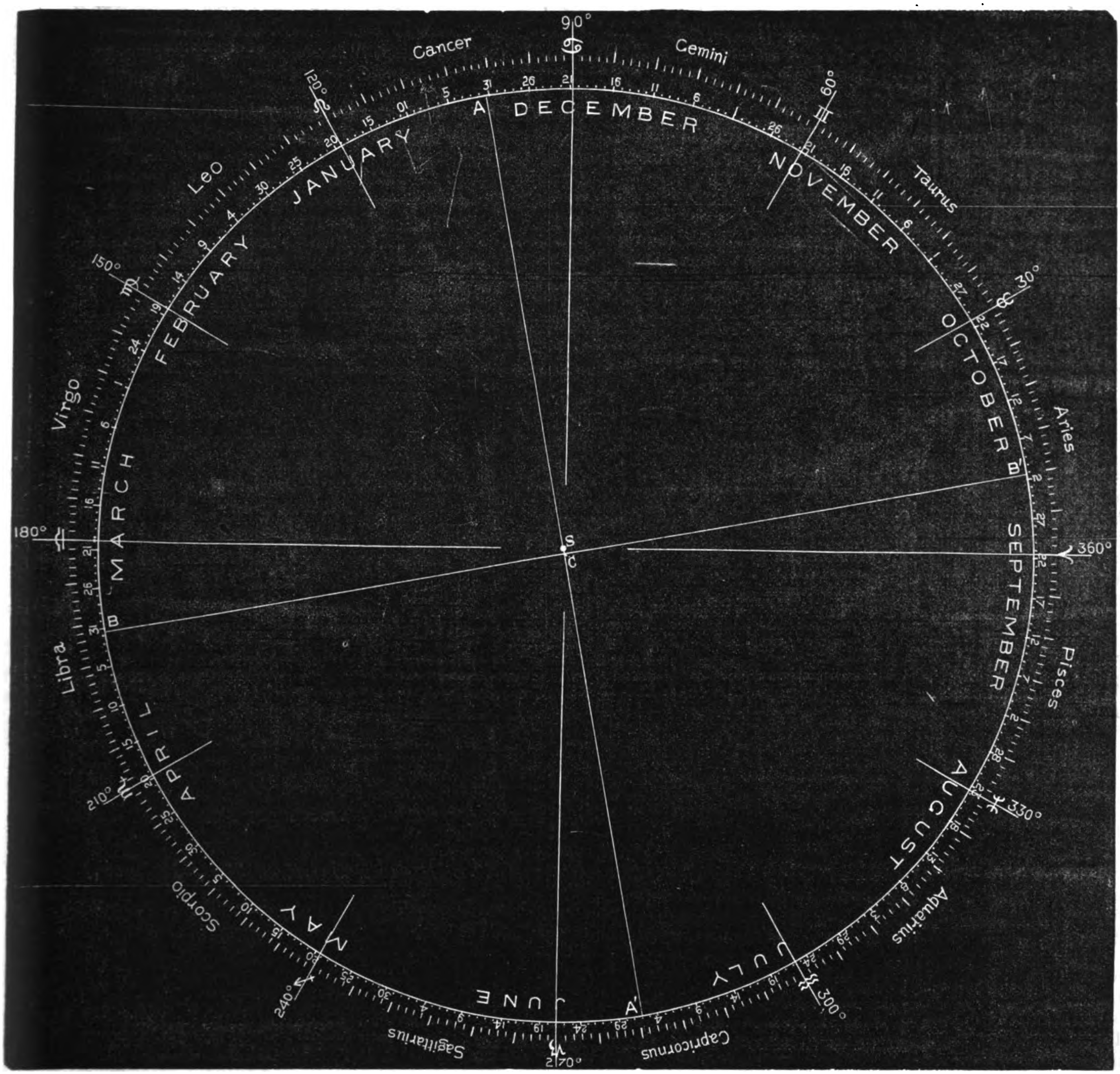
THIS week we give the Zodiacal Star Map for January. It needs no explanation. The path of Jupiter is given, and will appear again in the next map, for these zodiacal maps overlap some five or six degrees. Albeit the path of Jupiter is not shown with any idea of helping even the youngest student of astronomy to identify that resplendent planet. Rather will the observed position of Jupiter serve to guide the student to recognise the stars shown in these two zodiacal maps. (In the next, which will appear next week if room can possibly be found for it, the path of Mars also will be shown.)

I may note, however, that although the zodiacal map which appears this week is the first of the dark or black-ground series, yet as I am not satisfied with the effect, where the meridians and declination parallels appear in full, all the remaining maps of the series will show only the intersections of the meridians and parallels, so that the star groups may appear more clearly. This one will therefore be repeated in due course (that is as the last of the series), with that change made.

In the white zodiacal maps the meridians and parallels will be shown in full, and also the sun's course from day to day throughout the year in the order corresponding with the positions of the earth from day to day in her orbit as shown this week.

The movements of the planets in their orbits will be shown in pictures to appear shortly, in such sort that the exact positions of all the planets in any day during the next year or two can be seen at a glance. But the fact is that there is a difficulty in making room for all the astronomical illustrations which the movements of sun, moon, planets, &c., really require from month to month.

Britain's Future.—It savours of the sensational to assert that there are causes in operation which, if they continue to act unchecked, will in the course of time produce not merely the downfall but the absolute destruction and obliteration of Great Britain. Yet such an assertion expresses the literal truth. Observing in passing that "there is much virtue in your if," and that "the course of time" is a somewhat vague generalisation, we may state that not only has the above opinion been expressed by the most eminent scientific men, by such authorities as Sir Charles Lyell and Sir John Herschel, but it does not admit of doubt or uncertainty. Britannia, though doubtless "the pride of the ocean," is being quietly but remorselessly absorbed by the sea-waves. The process is one which—unless hastened by unforeseen but possible changes—must take many ages. It is also a process which may be, and probably has often been, interfered with in a summary manner. But no doubt whatever can exist that the process is now in steady operation, and that its effects are far from being insignificant.



THE EARTH'S PATH ROUND THE SUN.

WE give this week a picture, exactly drawn to scale, of the earth's motion from day to day in her orbit around the sun. We suppose her to start from 0° longitude at noon on March 20, and show her position at noon on successive days from that time till the next March 20, when she is still one quarter of a day's motion from the position whence she started. The study of this diagram will be found useful in considering the explanation of the Almanac running through KNOWLEDGE for this present year 1884; but it has also other uses. We shall have occasion to refer to the diagram many times during the next few months, and perhaps years.

We propose to give in a few weeks, for comparison with the path here shown, the more eccentric orbit of the earth 850,000 years ago, when the orbit had attained very nearly the greatest degree of eccentricity possible.

THE MIGRATIONS OF BIRDS.

AT the bottom of what we call our "lower gardens" we have a large, disused, double coach-house, in which wood is stored. The doors of this coach-house are always left standing open in both summer and winter, in order to give free access and ingress to "all sorts and conditions" of wild birds which may care to build or shelter there. A massive beam supports the ceiling of this coach-house in-

side; the beam is fully 1 ft. 2 in. square as to girth, and perhaps 20 ft. in length.

About twelve years ago a pair of swallows discovered that the inner side of this beam offered an "eligible building site," and they accordingly built a nest in the snug angle formed at one end by the beam, the wall, and the ceiling. We should never have discovered the nest, except from seeing the birds flying in and out of the doorway; for, being *behind* the beam, it was quite invisible to any one who did not go into the farther end of the coach-house, and then turn round to look up at the inner side of the beam.

When the next summer came, our swallows returned to the old nest, bringing with them another pair, who built at the other end of the same beam, also on the inner side. And now, every year, two or more pairs arrive with the regularity of winter tourists returning to their English homes for the summer months.

I venture to think that this fact may be accepted as positive evidence that the same birds, or their young, return each year to the same nests. The place is so secret, so hidden from sight, even from the doorway, and, in all respects, so out-of-the-way, that no mere stranger would be likely to discover it. I conclude that the first pair of swallows did discover it, by pure accident; but it was an accident not likely to happen again. It had certainly never happened before, during all the years that we have lived here. Once discovered, it has never been forgotten; and when each successive summer brings our busy and beautiful visitors we please ourselves with the thought that, if not the identical swallows of twelve years ago, they are undoubtedly the descendants of our first lodgers.

I beg to offer this observation by way of a pendant to Mr. Grant Allen's interesting paper on the "Migrations of Birds," in your number for Jan. 4.

AMELIA B. EDWARDS.

OUR PORTRAIT.—"We" remember hearing Emerson say that none of his portraits that he had yet seen did full justice to the combined grace and dignity of the original. It is so with ourself. When we—but this editorial "we" is a nuisance—when I was in America, as also when I was in Australasia, several portraits of me appeared which gave me entirely new ideas about my personal appearance. If they had agreed either among themselves, or with my photographs, or with what my friends saw in me, I might have inferred that I was simply seeing myself as others saw me. But they did not. In some I looked preternaturally truculent, in others as portentously amiable; while others showed all sorts of varieties between those limits. In this country, during the past twelve months four presentations, all intended however as caricatures, have appeared:—One in *Vanity Fair* last spring, with which the artist was not content, there I look rather dictatorial and more positive than I feel; another in *Figaro* (Dec. 7) where I look too sweet for any use; another in the Christmas number of *Truth* where I look peppery (I suppose at least it is I scowling over Lord R. Churchill); and lastly one in the current number of the capital new weekly *Our Boys*, where I find myself looking very happy and contented, but rather more leary than I supposed. The paragraph relating to me states that I am a very clever man, and know it. Am I? and Do I? I should like to know: but I am not likely to. I learn that I was a famous athlete in my youth, which surprises me almost as much as the statement in *Vanity Fair* that I was once a great equestrian. The account of my life in *Figaro* is as near the truth I think as it could be made.

THE BÜRGIN DYNAMO-ELECTRIC MACHINE.

(Concluded.)

THE field magnets, to which a brief reference was made in the previous article (KNOWLEDGE, No. 113), are of massive proportions, and cast-iron is used as in other machines, on the score of economy and because their polarity does not change in quality or kind. The resistance of the wire on the field magnets of the machine illustrated, which is constructed to feed the powerful arc lamps, is 1·2 ohms, the resistance of the armature being 1·6 ohms, making a total internal resistance of 2·8 ohms.

The wire on the field magnets is connected to separate terminals, and is, therefore, capable of being used in a circuit distinct from the armature, the object of this arrangement being to afford facilities for imparting larger currents to the illuminating or external circuit. When this is done a smaller *exciting* dynamo, with its armature current passing round its own field magnet, in the usual manner is used to supply the necessary current to the field-magnets of the larger machine. The armature being thus separated from the field or inducing magnets, a material reduction in the resistance of the external circuit is effected, and a proportionately larger current produced. It is evident that this feature is of enhanced importance in incandescent lighting, because of the extremely low resistance of the purely external or lamp portion of the circuit.

In comparing the Bürgin with the gramme we observed that there is a considerable deviation in the construction of the armature, which consists of a number of rings rigidly fixed to a revolving spindle (Fig. 3). In the Gramme ring the current generated in one sectional coil of wire is connected to the next succeeding coil; but M. Bürgin allows the current produced in the first section of the first ring to be transmitted through the second section of the second ring, thence through the third section of the third ring, and so on. In this way the current traverses what is practically a complete ring, and so compares with the Gramme.

A remarkable feature about the machine is the proportionately small quantity of copper-wire used, and the increase in the proportion of iron employed. By this means a more intense magnetic field is produced, which is truly traversed by a smaller quantity of wire, the reduction in the wire being compensated by the increased intensity of the field. Iron is furthermore considerably cheaper than copper, and much less liability to heating and to opposing induction is experienced. On the occasion of a visit we made to the "Arc Works," Chelmsford, where the machines are manufactured, we were much struck by the relative coolness of the machines, in the attainment of which the above feature is not the least important factor. They run at speeds ranging from 900 to 2,000 revolutions per minute; to do which the armature, being a heavy one, must be, as indeed it is, very carefully and accurately balanced. With a No. 4 machine, weighing 8½ cwt., running at 800 revolutions per minute, a current having an electro-motive force of 150 volts is produced, the current through a circuit of three Crompton arc lamps being sufficient to yield a luminosity of about 3,000 candles in each arc. The works referred to are probably the largest of the kind in the kingdom. Certain it is, however, that Messrs. Crompton & Co. have for some time been enjoying a very large share of what business has been transacted in the electric-light world. The adaptability of the Bürgin dynamo contributes to this in no small measure. When made in larger proportions it is capable of sustaining a great number of lamps, and for incandescent lighting a

machine is constructed to produce sufficient current to illuminate five hundred Swan lamps. The weight of such a machine is about 70 cwt., and its speed 900 revolutions per minute.

Mr. Crompton invariably uses Swan lamps for incandescent lighting, because he finds, as he and others anticipated, that a greater illuminating power per horse-power could be produced with Swan than with either of the other forms. He finds that he can actually obtain three hundred candle power per horse-power, or twelve Swan lamps at the full power of twenty-five candles each. This is largely in excess of the work accomplished by other systems.

The dynamo may be wound on the shunt principle, that is to say, instead of the whole of the current passing from the armature through the field magnets, only a portion is allowed so to travel, the remainder being sent through the external circuit. In this way the current, as it emerges from the armature, has two courses open to it, and will divide between them inversely as their resistances, so that if by any means the resistance of the external circuit is increased a larger proportion of the current passes through the inducing coils and produces a more intense magnetic field, and a stronger current is consequently produced in the armature. Conversely, should the resistance of the external circuit be diminished, a smaller proportion of the current will pass through the coils of the inducing magnets, and a less intense magnetic field is produced, with the result that feebler currents are produced in the armature. Thus, if an ordinarily wound dynamo were sustaining, say, four arc lamps, and another lamp were inserted, the resistance of the circuit, including the lamps, armature, and inducing coils, would be increased by the amount of resistance in the added lamp and its arc. The result would be that the current would be lessened, and consequently the inducing or field-magnets would have less effect on the armature, and a further reduction in the strength of the current ensues. Thus, there is not only insufficient current for the five lamps, but actually less current than when only four lamps were in the circuit. On the other hand, if one of the four lamps were cut out of the circuit, the total resistance would be diminished, and a stronger current would circulate through the inducing magnets, &c., resulting in a further increase in the current produced in the armature, but obviously less current suffices to light three lamps than is required for four lamps. In experiments made upon two similar machines, one worked on the shunt system and the other with the whole of the wire and the lamps in one circuit, the superiority of the shunt for producing a steady current and yielding automatically to variations in the condition of the external circuit was fully maintained.

The Crompton-Bürgin Compound Machine, designed for incandescent lighting, is an advance upon the shunt system. The current, on emerging from the armature, divides into two circuits—one composed of comparatively fine wire, and forming the coils for exciting the field-magnets; the other comprising, in addition to the lamps and leads, a few turns of thick wire, also wound on the field-magnets. The automatic controlling effect exerted by this arrangement is obvious from what has been said anent the simple shunt-system. It must be remembered that every lamp added to the circuit in the usual manner—that is, in multiple arc — decreases the external resistance, because an additional channel is offered for the transmission of the current. Consequently, a smaller proportion of the current will pass through the thin coils, and if left alone to influence the field-magnets, the intensity of the field would be diminished; but the increased current circulating in the thick coils compensates for the loss in the thin coils, and so maintains the luminosity of the lamps. So readily do the coils respond to any change in the circuit, that if nearly the whole of the lamps were cut out, those remaining would continue to yield a steady light, no change being perceptible.

The system introduced by Mr. Crompton has been in commercial use for about four years, and during that time it has been installed in a large number of public buildings, including the Royal Courts of Justice and the Mansion House, London; the Town Hall, Birmingham; the Post Office, Glasgow; and several of the larger railway stations, in addition to several large ocean-going passenger steamers. Taking all things into consideration, we may safely say that no man has done more to make the electric light practicable than Mr. Crompton.

A THEORY OF VISIONS.*

A CAMBRIDGE student had arranged, some years ago, with a fellow-student that they should meet together in Cambridge at a certain time for the purpose of reading. A short time before going up to keep his appointment my informant was in the South of England. Waking in the night he saw, as he imagined, his friend sitting at the foot of his bed. He was surprised by the sight, the more so as his friend was dripping with water; he spoke, but the apparition, for so it seems to have been, only shook its head and disappeared. This appearance of the absent friend occurred twice during the night. Information was soon received that, shortly before the time of the apparition being seen by the young student, his friend had been drowned while bathing.

This story has the typical features of a whole class. The essential characteristic is the recognition, after physical dissolution, of a deceased person, by one who has known him in his lifetime, in the form which distinguished him while a member of the living human family.

Now let me pass from the spiritual to the physical, and endeavour to expound some notions concerning real vision and supposed vision of objects, which may be useful in helping us to form something like a *rationale* of such apparitions as those of which I have been speaking.

When an object is placed before the eye, the light emanating from each point of the object falls upon the eye, and having passed through the several lenses and humours of which the eye is composed, is made to converge upon a point in the screen or retina which constitutes the hinder portion of the eye; and so a picture is formed upon the retina, much in the same way as in the photographer's camera-obscura. In fact, the eye may be described with some advantage, and without much error, as being a living camera-obscura. The retina is in reality the expanded extremity of the optic nerve, which communicates with the brain; our object, therefore, by means of the machinery of the eye, is placed in immediate communication with the brain; every wave of light from each point of the object produces a vibration on the retina, and so presumably on the brain. After this our physical investigation comes to an end—the vibrations of light from our visible object are lost in mystery. It is no exaggeration to say that we know nothing more than men knew centuries ago. A man says, "I see a ship;" and he tells the truth, but *how* he sees it neither he nor any one else can tell. You track the ship to its picture on the retina, but there you must leave

* From a paper by the Bishop of Carlisle (Dr. Harvey Goodwin) in the *Contemporary Review* for January.

it: even if you say that you can connect it with the brain, you have still an infinite gap between the impression on the brain, and the result expressed by the words "I see."

The fact is, that in vision we have a demonstrable transition from the physical to the spiritual; how the transition takes place it baffles our intellect and our imagination even to guess, but that there is such a transition no one can doubt.

This being so, is it not at least conceivable that, as the object moves the visual machinery of the eye, and this machinery moves the mind, so if the mind be directly moved (supposing for a moment that this is possible), the result may be the movement of the visual machinery, or at all events the production of the impression that it has been so moved?

To illustrate my meaning, take the case of the ringing of a bell. The pulling of the bell-rope causes the bell to give forth a sound; if you hear that sound, you conclude that the rope has been pulled; and if the bell should, in reality, have been rung by some one who had immediate access to it, you would still, in default of other knowledge, conclude, though erroneously, that the sound arose from the pulling of the rope.

Now let it be supposed, for argument's sake, that the mind can be acted upon otherwise than through the senses. The senses, as we all know, are the ordinary avenues to the mind, especially the two highest of the senses—namely, seeing and hearing; still it does not seem unreasonable to suppose that there may be other avenues. If man has a spiritual nature which is embodied in a fleshly tenement—which is at least a reasonable supposition, and corresponds almost to a human instinct—and if there be spiritual beings which are not so embodied, then it would seem not unreasonable to suppose that those spiritual beings should be able to hold converse with the spiritual part of men without the use of those avenues which the senses supply, and which are the only means whereby one material being can communicate with another.

Let us go a step further. Is it not conceivable that the spiritual part of man, "set free from the burden of the flesh," may (under conditions which we, of course, are not in a position to determine) have communication with the spiritual part of another man who still lives in the body? I do not at all say that we could anticipate by the power of reason that this would be so; but I can see nothing unreasonable in supposing it possible, and if phenomena should be in favour of the hypothesis, I think the hypothesis could not be set aside by any *à priori* considerations.

But if this be so, we arrive at a case similar to that of the bell being rung without any pull upon the rope. In other words, may it not be that a communication made directly by one spirit to another may *seem* to arise from that action of the senses to which mental impressions are usually due? I lose a friend, and that friend is able (I know not how or why) to communicate with me; his spirit makes itself known to my spirit; I become conscious of his presence by a direct, though inexplicable, spiritual action; what more probable than the supposition that this direct communication will *seem* to have been made through the senses? In fact, as being myself subject to the laws of sense, could I be conscious of my friend's presence in any other way than by imagining that I saw his form or that I heard his voice?

To take the case, the particulars of which I have already related. If we suppose that the student who was drowned was able to hold, at the moment after his decease by drowning, some kind of spiritual communication with his friend in Cambridge, is it not conceivable that the spiritual communication would transform itself into a brain action by the reverse of the process according to which brain action normally transforms itself into a spiritual communication, and that so the effect would be the production of a persuasion in the mind of the student in Cambridge that he actually saw with his eyes his absent friend?

The *rationale* of apparitions which has been suggested will, perhaps, receive confirmation from the consideration, that instances occur in which the full sense of vision is produced by the brain itself, without any suspicion of what may be called preternatural agency. The following story was related to me some years ago in the presence of one of the persons to whom the event described happened, and who vouched for its truth:—

A lady with a family of young children was occupying a house in Cheltenham, while the husband and father was absent on business in Scotland. Looking out of the windows of a back drawing-room upon a small garden, which communicated by a door with a back lane, several of the children saw the garden-door open and their father walk through and come towards the house. They were surprised, because they were not expecting their father's return; but uttering a shout of joy, several of the party ran downstairs there to find, to their disappointment and sorrow, that no father had arrived. So strong was the illusion that when the father did return, a week or more afterwards, he was reproached for having played some trick, of which he was perfectly innocent. I ought to add that the curious illusion which has been described had no consequences of any kind—good, bad, or indifferent; no one died, no one was taken ill, no family event of any sort took place; the whole thing was an illusion, and nothing more. It is, however, curious, as having been shared by several persons. The member of the family, whom I knew, and in whose presence I heard the story, assured me that she never saw anything in her life more distinct than her father seemed to her to be, and that her sisters had said the same.

AN EXPENSIVE RAILWAY.—Seven tunnels, of a total length of seven miles, will be built on the line of the Harrisburgh and Western Railway (U.S.A.), between Harrisburgh and the Youghiogeny River. This portion of line will cost about £40,000 per mile. It is to be completed in two years.

IMPORTANCE OF TRIFLES IN SCIENCE.—Few circumstances in the history of scientific progress are more remarkable than the way in which apparently insignificant facts suddenly acquire an unexpected importance. A physicist examines with interest a few dark lines which he detects in the solar spectrum, and by degrees the peculiarity attracts attention among scientific inquirers; yet the very labourers in the new field of research would have heard with amazement that the lines they were examining were one day to become the means of teaching us how to analyse the central luminary of our system. Again, inquisitive men examined the phenomena of electricity, and accumulated a number of seemingly unimportant facts; yet the days were to come when results which appeared to be mere matters of curiosity were to bear more largely on the interests of men and nations than any perhaps which have ever rewarded the inquiries of scientific men. The steam-engine itself, which alone vies with the electric telegraph in the importance of its influence on the well-being of mankind, resulted from inquiries suggested by the insignificant movements of a common saucepan-lid. In the history of astronomy we are presented with many illustrations of the kind here referred to.

Reviews.

AMONG THE INDIANS OF GUIANA.*

By EDWARD CLODD.

THIS is as solid and withal as attractive a book of travel as it has been our fortune to read for many a day. Its interest is not dependent upon the adventures which make up so many "travellers' tales," for of adventures, in any exciting sense, there are none. It is not the outcome of a rapid scamper across a wide continent, neither is it a hotch-potch of information gathered at second-hand and served up with trimmings from the "Encyclopædia Britannica." The author has spent in all four years among the fellow-subjects of ours whom he describes, and his sound and scholarly book has slowly built itself up from materials which he has personally gathered, and which in the end come to us in their now related, shapely, and charming force.

The earlier and less important chapters of the work are devoted to a rapid sketch of the physical features of the regions explored, and of the journeys from the coast into the interior—necessarily taken, in the absence of any roads thither, along the larger rivers. The country consists of four distinctly-marked tracts, lying one beyond the other, parallel to the coast-line. The sugar-tract, which is the only cultivated portion, lies nearest the seaboard. Behind this is the timber tract, which yields important and abundant supplies; then the forest tract, lying too remote for profitable carriage of its valuable wood; and last of all is the savannah, or wide-reaching grass-plain, which spreads over so large an area of South America.

Mr. im Thurn's sketch of the physical geography of British Guiana might with advantage have been supplemented by a sketch of its geology. The rich alluvial *detritus* of this shoulder of the South American continent is the gift of its noble rivers, and of more recent origin than the mountain ridges intersecting the savannahs, and probably marking the ancient coast-line of the Atlantic Ocean; whilst evidence points to the former existence of a large inland lake, from the mist of whose shrunken waters was born that legendary capital of the fabled land of El Dorado which kindled the ardour of Raleigh. Strolling to the brow of the hill on which Quatata stands, Mr. im Thurn describes how—

> "In the far distance the plain was bounded by the ridges of the Pacaraima mountains, which were at that moment much hidden by dense white clouds. Gradually the masses of these clouds rose, and only a long, rugged, and broken line of vague white mist remained, marking where the mountains rose from the plain. Presently the sun began to shine with power, and lighted up each jutting fantastic point of this low-lying mist, until the whole seemed a city of temples and towers, crowned with gilded spires and minarets."

In his description of the scenery the author's remarks are usefully supplemental to, although not always in agreement with, those made by Mr. Alfred Wallace, in his well-known essay on "Tropical Nature." Both are corrective of current misconceptions as to the brilliant blaze of colour in the flowers and birds which is thought to be a universal feature in the tropics. Speaking of plants, Mr. im Thurn says: "The spread of the colour-fallacy is due to the fact that it is the more gorgeous plants which, being selected from an infinitely greater number of less brilliant hue, are grouped together in our glass-houses." The general colour of the forest is due rather to the various shades of the leaves, than to any wide scattering of flowers, and at no season is the Guiana leafage as splendid as is an ordinary English wood, either in the early spring or in the autumn time. The difference of effect lies not so much in the colour of flowers in temperate and tropical zones as in their distribution; in the latter the blossoms are more evenly scattered throughout the year, while in the former they are more seasonal. But the sweet odour of the tropical trees, with their plumes, and globes, and flame-like spikes of dazzling flowers, would be a poor exchange for the flowers that carpet our meadows, for the rich undergrowth of our copses, and for the mosses that nestle round our forest trees.

Whilst Mr. im Thurn's love for "the larger and more free ways of Nature" makes him wax eloquent in attempt to dispel the prejudices of the dweller in temperate climes against tropical discomforts, he candidly piles up the agony in an appalling list of the ills to which man is subject in hot countries, chiefly from creatures whose power to irritate seems in inverse proportion to their size. Taking only a sample or two from the long catalogue, when one reads of bats, from whose painless bite such loss of blood may ensue as to prove fatal; of black "*kaboori* flies," more dreaded than mosquitoes; of jiggers, that bury themselves under the soft skin between the nails and the fingers or toes, and deposit their eggs to be thus hatched under one's skin, vermin, of which, despite every care, twenty-three squatted on, or rather in, Mr. im Thurn in one day, we may count over our list of British pests with complacence. In his visit to the Kaieteur Fall, formed by the rush of a tributary of the Essequibo over an abrupt cliff of 741 feet, Mr. im Thurn was fortunate enough to discover both a new plant and a new bird, the latter, *Agelæus imthurni*, being very properly named after him by Mr. Sclater; and, apart from this addition to the naturalist's knowledge, he will find the chapters on the fauna and flora of Guiana a storehouse of information.

But we must turn to that part of his book in which, as its sub-title indicates, the author (and with him the present reviewer) has the keenest interest, namely, that dealing with the four or five semi-barbarous tribes, for they have practically passed out of the stone-using stage, mostly of Carib stock, scattered over the forest tract and the savannahs. The rivers of the colony are, as already stated, the high roads to the interior, and the tributaries are the crossroads. It was up the largest of the four chief rivers, the Essequibo, that Mr. im Thurn and his companions made their way, taking steam to the confines of civilisation, when they embarked in canoes with the natives whom they had engaged. Wherever they stayed they found the Indians hospitable and civil, and free in their offers of casiri, a drink prepared from the cassava plant, and tasting "like something between sour porter and thin claret." But it is a delectable liquor compared to the horrible stuff called paiwari, of which "etiquette demands that the visitor should finish it to the last dregs." It is made from burnt fragments of cassava bread, which, after being mixed with water, are picked out and chewed by the women while moving about their usual household work, the fragments being replaced in the water, which is boiled and then allowed to ferment, the result being a liquor looking like coffee with plenty of milk in it. Some of the tribes prepare paiwari by boiling only; but, in the judgment of native connoisseurs, the flavour is very inferior to that secured by the "orthodox chewing process." "One day passes very like another to the traveller as he ascends the river in his canoe," Mr. im Thurn remarks. For hundreds of miles only three or four settlements were passed. Each day brought the same sounds from the same dense forests

* "Among the Indians of Guiana"; being sketches, chiefly Anthropologic, from the interior of British Guiana. By Everard im Thurn, M.A., Oxon. (London: Kegan Paul, Trench, & Co. 1883.

growing down to the river banks and clothing the mountain ranges, from the summit of which one may look down

"on the great and wide sea of tree-tops, ending only at the circle of the horizon, and unbroken except where here and there a long, narrow thread of white mist marks the winding course of some small stream."

The Indians were more alert by night than by day, telling endless tales as they lay smoking in their hammocks, over which lumps of meat hung close to their mouths for economy of time in wakeful moments. Their capacity for gorging was measured by Mr. im Thurn. In thirty-six hours ten men consumed "252 lb. of smoked fish, 62 lb. of fresh fish, a whole wild hog, and an indefinite quantity of cassava bread." But the monotony of the journey was varied by misfortunes, all which were referred by the Indians to the paiman or medicine-man, or to Mr. im Thurm's presumption in actually touching an erratic boulder, which no Indian looks at if he can help it!

At last the limits of the canoe voyage were reached, and the explorers glad to make their home for a time in one of the largest settlements, consisting of a few mud-walled and palm-thatched houses, in one of which they held their levées for barter with the Indians. The information as to their social customs and beliefs gathered by Mr. im Thurn is of sufficient importance to demand separate notice.

(*To be continued.*)

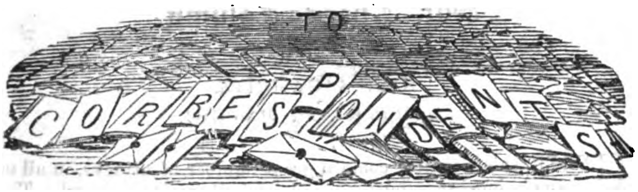
THE FACE OF THE SKY.

FROM JANUARY 18 TO FEBRUARY 1.

BY F.R.A.S.

AS fine groups of spots continue to appear on the solar disc, the sun should be examined with the telescope whenever the sky is clear. The night sky is pictured in Map I. of "The Stars in their Seasons." Mercury is still, for the observer's purpose, invisible. As Venus does not set for between two and three hours after the sun at the end of the month, she may be caught with the naked eye in the twilight over the western horizon. She continues perceptibly gibbous in the telescope. Mars comes into opposition to the sun at 11 a.m. on February 1st, so that he is now as favourably placed as he will be during his present opposition. His angular diameter slowly increases from 16″ to 16·6″ during the next fortnight, and the amount of detail now visible on his surface renders him a most interesting object for the telescopic observer. His green seas, red land, and white polar snow, albeit distributed in a widely different fashion on his surface to that in which their terrestrial analogues are on that of our own globe, seem to give him a kinship to the earth which is not apparent in any other of the planets. Jupiter comes into opposition to the sun at 3 a.m. on the 20th, and is now as well placed for the observer as he possibly can be. His equatorial diameter subtending an angle of some 43″, he is now a truly noble object, even in a small telescope; and every increase of optical power reveals fresh wonders and beauties in his complicated system of belts and markings, with their fine distinctions of colours, and in the varying phenomena exhibited by his satellites as they pursue their respective paths round their giant primary. Of such phenomena, visible before 1 a.m. during the next fourteen days, the following are the chief:—To-night (the 18th) the shadow of Satellite II. will enter into Jupiter's limb at 10h. 35m., followed by Satellite II. itself at 10h. 38m. Neither the satellite nor its shadow will quit the planet's face until between one and two a.m. to-morrow. Satellite III. will reappear from occultation at 8h. 1m. p.m. on the 19th. On the 20th, Satellite II. will be occulted by Jupiter at 5h. 41m. p.m., reappearing at his opposite limb at 8h. 35m. p.m. On the 22nd, Satellite IV. will enter on the face of the planet at 11h. 8m. p.m., followed by its shadow at 11h. 58m. The student should by all means observe this transit, if possible, for the reason given more than once in these notes, viz., that very curious appearances indeed have been witnessed by observers during the transits of Jupiter's outer satellites. At 12h. 23m. on the same night Satellite I. will begin its transit, as will its shadow at 12h. 28m. The egress of these satellites respectively and of their shadows will not occur until two or three hours after midnight. On the 23rd Satellite I. will be occulted at 9h. 30m., and, after passing behind the body of the planet and through its shadow, will reappear from eclipse at 11h. 53m. 2s. p.m. On the night of the 24th Satellite I. will begin its transit at 6h. 49m., as will its shadow at 6h. 56m. The satellite will leave Jupiter's face at 9h. 9m., and be followed by its shadow at 9h. 16m. The same Satellite I. will on the succeeding evening (that of the 25th) reappear from eclipse at 6h. 21m. 35s., and the transit of Satellite II. begin 52m. after midnight. On the 26th Satellite III. will be occulted at 7h. 41m. p.m., to reappear from eclipse at 11h. 53m. 6s. Satellite II. will be occulted by Jupiter at 7h. 55m., on the 27th, to reappear subsequently from eclipse on the opposite side of the planet at 11h. 10m. 33s. Satellite I. will be occulted at 11h. 14m. on the night of the 30th. On the 31st Satellite I. will enter on to Jupiter's disc at 8h. 33m. p.m., followed by its shadow at 8h. 51m. The satellite will leave the opposite limb of the planet at 10h. 53m., its shadow at 11h. 11m. p.m. Lastly, on the night of Feb. 1, Satellite I. will be occulted at 5h. 40m.; to reappear from eclipse at 8h. 16m. 4s. Saturn continues to present an object of the highest interest to the observer, and may be well seen up to midnight. His ring system continues sensibly unchanged; but, as he is receding from the earth, his angular diameter slowly diminishes. He continues to travel backwards through the sky, in that part of it to the west of ε Tauri ("The Stars in their Seasons," Map I.). Our remarks on p. 13, with reference to Uranus and Neptune are equally applicable during the ensuing fortnight. Pons' comet has now travelled down into the southern hemisphere, to be observed, let us trust, under clearer skies than have prevailed in these latitudes during its apparition. The moon is 20 days old at noon to-day (the 18th), and will, of course, be 29 days old at the same hour on the 27th. Then at noon, on the 23rd, her age will be 0·3 day; and, quite obviously, 4·3 days on Feb. 1. The only occultation which will occur before 1 a.m. during the next fourteen days is one of the sixth magnitude star, 21 Piscium, which will disappear at the moon's dark limb on the 31st, at 7h. 49m. p.m., at an angle of 162° from the moon's vertex. It will reappear at her bright limb at 8.45 p.m., at an angle from her vertex of 314°. The moon is in Virgo to-day, and will continue to travel through that constellation until 6 p.m. on Sunday, the 20th, when she will pass into Libra. She will not emerge from Libra until 8 p.m. on the 22nd, at which hour she will cross the boundary into Scorpio. She will take ten hours to traverse the narrow northern strip of this constellation, and at 6 a.m., on the 23rd, will enter the southern confines of Ophiuchus. It will be 2 a.m., on the 25th, before she will quit Ophiuchus for Sagittarius, and she will take until 1 p.m., on the 27th, to traverse the last-named constellation. At the hour just named she passes into the north-west corner of Capricornus, crossing it in nineteen hours; and at 8 a.m., on the 28th, enters Aquarius. At midnight, on the 30th, she quits Aquarius for Pisces, where she will still be found at the date of the termination of these notes.

THE TELESCOPE AND ASTRONOMY.—It is almost impossible to exaggerate the influence which the invention of the telescope has had upon modern astronomy. The mere enlargement of the various objects of interest visible on the celestial vault is the least of the services rendered by the telescope. It is as an aid to the progress of physical astronomy that the instrument derives its chief importance. Not even Newton's gigantic intellect could have established the theory of gravitation without the aid of the telescope; or rather, we should say that without its aid he would not even have attempted to establish the theory which renders his name illustrious. Poor as was the instrument with which Flamsteed pursued his observations at the Royal Observatory in comparison with the telescopes now in use, yet—we have it on Newton's own authority—without its aid Newton's researches into the lunar motions would have been valueless. On those researches, as is well known, the theory of gravitation was established. Since Newton's time telescopic observation has again and again brought forth new evidence of the truth of the great theory, and has again and again led mathematicians to devise new and more powerful modes of analysis, to push their calculations to a greater and greater degree of exactness, and, in fine, to bring their science to that wonderful degree of perfection which has been exemplified by the labours of Adams and Leverrier, Airy, Hansen, and Delaunay.



"Let Knowledge grow from more to more."—ALFRED TENNYSON.

Only a small proportion of Letters received can possibly be inserted. Correspondents must not be offended, therefore, should their letters not appear.

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PERPETUAL MOTION ON THE LARGE SCALE.

[1092]—In his remarks appended to my letter (No. 1081, p. 29), Mr. Proctor says that my note in the *Gentleman's Magazine* of December last on "Dismal Astronomy," was an attack especially directed against himself. This is a complete mistake, as a careful reading of the note itself will show. The hypothesis I attacked is there described as the one "now in fashion." Had I supposed that Mr. Proctor would have thus interpreted my remarks, I should have been more definite in stating the duration of the fashion, which is many years older than himself. I named no particular astronomer, because a very large majority of the astronomers of the present day follow this fashion. All who accept with the easy confidence—"now in fashion"—the nebular hypothesis of the origin of solar systems are "attacked." [Regret my mistake.—R. P.]

I may be imprudent in thus confronting such a host of high authorities, but am not likely to adopt the feminine device to which Mr. Proctor alludes as a weapon of attack upon anybody, especially on a friend so greatly respected as himself. If ever I should be moved to especially "pitch into" him, it will be on his own ground in KNOWLEDGE, with his own readers to form the ring; and whatever may be the amount of mutual bruising, I have no doubt that we shall shake hands, both before and after the fight like true Englishmen. [Gladly.—R. P.] W. MATTIEU WILLIAMS.

THE AFTERGLOW.

[1093]—In reply to the query of Captain Noble (No. 1078, p. 14) I quote the following from my diary: "Dec. 25, crimson fog; 26th, crimson fog morning and evening, and shown by gas-lights; 27th, crimson fog with drizzle, same as on 25th and 26th. Warm wind S.W. to W." "Shown by gas-lights," refers to the gas-light glow of London, which I see very distinctly across the fields that extend about three miles to S.E., between Stonebridge Park and Kilburn. Formerly this London luminosity was greyish, like the zodiacal light, but since the early part of last summer it has become pink and crimson, the redness increasing gradually and variably up to the present time. It is not only visible on looking towards London from a distance, but may be seen *in* London on misty evenings, by looking along a street that is well lighted, and by looking upwards. This was the case last night (Sunday, Jan. 13), and I have little doubt that it will be visible again many times during this winter.

Jan. 14, 1884. W. MATTIEU WILLIAMS.

THE AFTERGLOW IN CHESHIRE.

[1094]—I see that no one has yet called your attention to the brilliant and extraordinary sunsets in Cheshire, and as they differ somewhat from those already described, I thought you might like to hear about them. Last week, on December 19, 20, and 21, respectively, we had, about 4.30, a strange "light" noticed by everyone, and rather resembling the "magnesians." This, in a few moments, became golden, then of the loveliest mauve ever seen, followed by blue, and lastly blood-red, changing to the deepest crimson, whilst there were clouds in the south-west horizon of inky blackness. The above lasted upwards of an hour, and the sunrise has been the same. We have this phenomenon at present, but not in such intensity as that of last week. What is the cause of it? For all the nine years we have lived in Cheshire, we have never before seen it. I am sorry you are so unwell, but trust you are really going to "rest on your oars," although we shall miss you. With best wishes for many happy new years, F. ADELINE HARKER.

SKY-GLOW.

[1095]—In reply to query of Capt. Noble in last week's number, I spent Christmas Day in North Yorkshire. The day was cloudless, warm, and spring-like; and, in a perfectly clear sky, the depth of tone of afterglow was most marked. Although I have watched this phenomenon for a considerable time, I have never seen such a depth of vivid colour as on the evening in question. Venus was visible through the glow as a sparkling green gem (I presume from contrast).

I should like to ask Captain Noble, or any other reader in KNOWLEDGE, if he has noticed the glow in daylight? On Nov. 27 and Dec. 1. this was very decidedly visible here in Yorkshire.

The long continuance of dull, foggy weather has been a bar to regular observation.

On the evening of Dec. 3 the glow was very extended, and at 5.35 several stars in Ophiuchus were well visible to naked eye in the midst thereof.—I am, sir, yours truly, ALLAN EKERSHAW.

Ilkley, Yorks., Jan. 6, 1884.

SILENT LIGHTNING.—STRANGE SUNSETS IN AUSTRALIA.

[1096]—The following extract from a letter, received by me yesterday from Adelaide, South Australia, may perhaps be of interest with reference to recent correspondence. The communication is dated December 1 last.

"Last Sunday night we had a most extraordinary storm of lightning, with much rain but little thunder. There had been thunder-storms on the three preceding nights, and this formed the climax. I never saw anything like it. All round the sky was lit up with brilliant flashes, which continued at intervals of not more than a second for two hours. The city might have been lit up by the electric light, so tremendous was the lightning. Since then the weather has been beautifully cool.

To-night the sky has been, just after sunset, a most lovely red colour, tinged with purple, as if a gigantic bush fire were on. It has been so frequently for two months, and nobody seems to know the cause of it. CHAS. E. BELL.

THE MIGRATION OF BIRDS.

[1097]—Will you point out to Mr. G. Allen that his explanation of the migration of birds is very well as far as it goes, but can only be applied to large birds which fly high, but *not to small birds* who do not. The water birds and waders travel by night, and that they do not travel by sight is proved by the fact that they use a continual *call* to enable them to keep together.

Owls may see at night, but they do not fly high, nor do finches, flycatchers, and such like. NATURALIST.

CAGE NESTS.

[1098]—I purchased a parrot at the Crystal Palace about eight years ago. Last year, to my surprise, she made a rude nest at the bottom of the cage, and laid three eggs. I thought it might be information to the student of natural history, but perhaps it may be an ordinary occurrence. While I have the pen in my hand it is as well to mention that last spring a hen canary escaped from the breeding-cage, and made a neat nest between two flower-pots in the conservatory. She got her materials from a Berlin wool mat. But what makes it worth mentioning, she chose the brightest colours, and showed a decided predilection for red. A. J. W.

P.S.—I was in hopes of hearing in your articles on the rotundity of the earth something about a pendulum that demonstrated the movement of the earth. It is about 25 years ago we all went pendulum mad.

LETTERS RECEIVED (SUB-EDITOR.)

E. E. CORDNER. Thanks.—W. H. HUDEY.—H. J. WYKE.—T. H.—F. PODMORE. We do not keep addresses.—C. H. JONES.—P. B. F.—C. CARNS-WILSON.—A. G. WELD.—R. H. P.—J. GROUT.—MARS. G. FLETCHER. Thanks.—MARS. Probably the state of the atmosphere. If you use the telescope in a warm room the peculiarity is sufficiently accounted for. F.R.C.S.—NATURALIST.—JAS. GILLESPIE. Confident in your own mind you are right! and others wrong? Yet we think the earth really moves round the sun.—A. G. WELD. Cannot be electrical.—J. P. B. Many thanks; but no space.—W. H. SHRUBSOLE.—A. C. P. FIRMIN. In *both* cases.—in any position.

ERRATUM.—In middle of second column of p. 12, (No. for January 4) in the sentence beginning "For instance when a planet," &c., the word "centrifugal" should be replaced by the word "centripetal."

Our Whist Column.

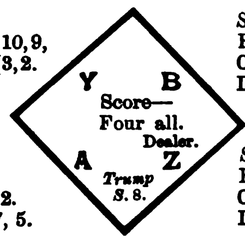
By "Five of Clubs."

THE following game is from an old number of the *Westminster Paper*, where, however, it was carelessly annotated :—

The Hands.

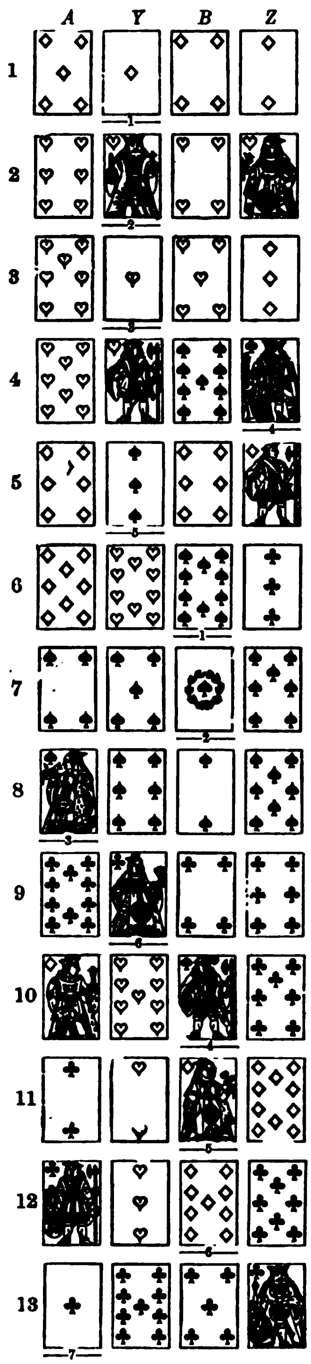
Y.
Spades—6, 5, 3.
Hearts—A, K, Kn, 10, 9, 3, 2.
Clubs—Q, 9.
Diamonds—A.

B.
Spades—A, Kn, 10, 9, 2.
Hearts—5, 4.
Clubs—5, 4.
Diamonds—Q, 9, 6, 4.



A.
Spades—Q, 4.
Hearts—8, 7, 6.
Clubs—A, Kn, 10, 2.
Diamonds—K, 8, 7, 5.

Z.
Spades—K, 8, 7.
Hearts—Q.
Clubs—K, 8, 7, 6, 3.
Diamonds—Kn, 10, 3, 2.



Notes and Inferences.

1. *A* should have led a Club. *Y* has no more Diamonds.

3. *Z* should have discarded a Club. His head sequence was worth guarding.

4. *Y* holds all the remaining Hearts—four.

5. *A* knows that the Q is with *B*; and he can infer almost certainly from *Z's* discard that *B* holds one more Diamond at least, in which case,

6, Diamond 8 should not have been discarded, but the King; for the King unguarded could only block *B's* Diamonds. But probably at this stage *A* did not feel sure that he might not want a re-entering card later.

9. *A* should have led his Diamond King before the Club 10. He knows that *Y* has two Clubs and the three long Hearts; that *B* has the long trump and the Diamond Queen. Now it matters not whether *B* has three Clubs, or two Clubs and another Diamond: *A B* must win on the line indicated, making one trick in Diamonds, two in Clubs, and one with the long trump.

10. Here, says the Editor of the *Westminster Papers*, "the game is evidently lost unless *A* discards the Diamond King; this is almost the only point in the game; and yet simple as it is, how many players will persist in keeping such a card; nay more, will scold their partner for throwing it away if it should happen to turn out of no avail." *A*, however, only simplified his partner's play by throwing away the Diamond King. Had he retained it, *B's* reasoning when about to lead at trick 11 would have run thus :—"We must make all three tricks; a trick in Diamonds is certain; but it is equally certain that unless *A* can make two tricks in Clubs we are lost: he certainly has not both Ace and King, or he would neither have led Diamonds originally nor Club 10 at trick 9; but anyway, if he has these cards, we win; if he has not, we can only win by my leading up to him in Clubs." Therefore *B* would have led a Club, and *A* would have made the three remaining tricks. *A* was right, however, in throwing the Diamond King.

Our Chess Column.

By Mephisto.

KING'S GAMBITS.

$$1. \frac{\text{P to K4.}}{\text{P to K4.}} \quad 2. \frac{\text{P to KB4.}}{}$$

UNDER this heading come a great number of attacks, in all of which, however, the King's Bishop's Pawn is sacrificed. The capture of the proffered Pawn has the immediate result of favouring White's development, by enabling P to Q4 to be played with advantage, should the Black Bishop venture to QB4, and attacking the advanced Pawn on KB5. Black has to devote his attention to defending this Pawn, which is a somewhat difficult task, as White has a variety of bold and promising continuations at his disposal. If Black succeeds in maintaining his advantage against White's attack—and most authorities agree that the advantage can be maintained—then, of course, Black will emerge from the opening with a better game.

Another course is likewise open to Black, that of declining the Gambit, by not capturing the KBP. We shall first deal with the accepted Gambit, and subsequently revert to the Gambit declined.

After 1. $\frac{\text{P to K4}}{\text{P to K4}}$ 2. $\frac{\text{P to KB4}}{\text{P takes P}}$ White has two moves at his disposal 3. Kt to KB3, which is the more general move offering an immense variety of interesting combinations, and called the King's Knight's Gambit; or, 3. B to B4, called the Bishop's Gambit. In reply to 3. Kt to KB3, Black's usual defence is 3. P to KKt4, but 3. P to Q4 or 3. Kt to KB3 are also played. We will treat of these moves under the heading of Gambit declined.

In continuation of the attack, White can now play either 4. B to B4 or 4. P to KR4. If 4. B to B4, Black may either choose the normal variation, by playing 4. B to Kt2, or the more vigorous move of 4. P to Kt5, in reply to which White can either play 5. Castles, called the Muzio attack, or 5. Kt to K5, the Salvio attack. Again, in reply to 4. P to KR4, Black's best move is 4. P to Kt5, upon which White has two modes of continuing the attack: by 5. Kt to Kt5, called the Allgaier, or 5. Kt to K5, the Kieseritzky Gambit. The Allgaier Gambit is the more attacking of the two, but entails the sacrifice of the Knight, which Black can force by 5. P to KR3, but the Kieseritzky is more likely to result in an even game. The last attempt in this opening was made by Steinitz against Zukertort, in their encounter at the Vienna tournament, when Zukertort won in twenty-seven moves. After 5. Kt to Kt5, Black need not at once play P to KR3, but may also play P to Q4 which likewise yields a fair game with less risk of attack, to be followed by P to KB3.

We may also mention another popular form of Gambit attack arising out of a variation of the Vienna Opening in 1. $\frac{\text{P to K4}}{\text{P to K4}}$ 2. $\frac{\text{Kt to QB3}}{\text{Kt to QB3}}$ 3. $\frac{\text{P to KB4}}{\text{P takes P}}$. The difference in the position is that the QKt is already in play. Here Steinitz plays the bold move 4. P to Q4, which is called the Steinitz Gambit, and until lately was considered sound play. If, however, White does not pin his faith to this Gambit he can continue with 4. Kt to B3, upon which Black may reply with 4. P to KKt4, or likewise with 4. B to K2, a move favoured by Bird.

These are the leading Gambit attacks, and although many volumes of analysis have been written regarding these openings, modern research has confined the attack within comparatively narrower limits. We shall explain the attack and defence of the leading variations, and believe that their mastery, combined with ordinary caution, will be sufficient protection to a player against any serious disaster in these difficult openings.

Contents of No. 115.

KNOWLEDGE
AN ILLUSTRATED
MAGAZINE OF SCIENCE
PLAINLY WORDED - EXACTLY DESCRIBED

LONDON: FRIDAY, JAN. 25, 1884.

CONTENTS OF No. 117.

THE CHEMISTRY OF COOKERY.

By W. MATTIEU WILLIAMS.

XXVII.

THE cookery of milk is very simple, but by no means unimportant. That there is an appreciable difference between raw and boiled milk may be proved by taking equal quantities of each (the boiled sample having been allowed to cool down), adding them to equal quantities of the same infusion of coffee, then critically tasting the mixtures. The difference is sufficient to have long since established the practice among all skilful cooks of scrupulously using boiled milk for making *café au lait.* I have tried a similar experiment on tea, and find that in this case the cold milk is preferable. Why this should be, why boiled milk should be better for coffee and raw milk for tea, I cannot tell. If any of my readers have not done so already, let them try similar experiments with condensed milk, and I have no doubt that the verdict of the majority will be that it is passable with coffee, but very objectionable in tea. This is milk that has been very much cooked.

The chief definable alteration effected by the boiling of milk is the coagulation of the small quantity of albumen which it contains. This rises as it becomes solidified, and forms a skin-like scum on the surface, which may be lifted with a spoon and eaten, as it is perfectly wholesome, and very nutritious.

If all the milk that is poured into London every morning were to flow down a single channel, it would form a respectable little rivulet. An interesting example of the self-adjusting operation of demand and supply is presented by the fact that, without any special legislation or any dictating official, the quantity required should thus flow with so little excess that, in spite of its perishable qualities, little or none is spoiled by souring, and yet at any moment anybody may buy a pennyworth within two or three hundred yards of any part of the great metropolis. There is no record of any single day on which the supply has failed, or even been sensibly deficient.

This is effected by drawing the supplies from a great number of independent sources, which are not likely to be simultaneously disturbed in the same direction. Coupled with this advantage is a serious danger. It has been unmistakably demonstrated that certain microbia (minute living abominations) which disseminate malignant diseases may live in milk, feed upon it, increase and multiply therein, and by it be transmitted to human beings with very serious and even fatal results.

I speak the more feelingly on this subject having very recently had painful experience of it. One of my sons went for a holiday to a farm-house in Shropshire, where many happy and health-giving holidays have been spent by all the members of my family. At the end of two or three weeks he was attacked by scarlet-fever, and suffered severely. He afterwards learned that the cow-boy had been ill, and further inquiry proved that his illness was scarlet fever, though not acknowledged to be such; that he had milked before the scaling of the skin that follows the eruption could have been completed, and it was therefore most probable that some of the scales from his hands fell into the milk. My son drank freely of uncooked milk, the other inmates of the farm drinking home-brewed beer, and only taking milk in tea or coffee hot enough to destroy the vitality of fever germs. He alone suffered. This infection was the more remarkable, inasmuch as a few months previously he had been assisting a medical man in a crowded part of London where scarlet fever was prevalent, and had come in frequent contact with patients in different stages of the disease.

Had the milk from this farm been sent to London in the usual manner in cans, and the contents of these particular cans mixed with those of the rest received by the vendor, the whole of his stock would have been infected. As some thousands of farms contribute to the supplying of London with milk, the risk of such contact with infected hands occurring occasionally in one or another of them is very great, and fully justifies me in urgently recommending the manager of every household to strictly enforce the boiling of every drop of milk that enters the house. At the temperature of 212° the vitality of all dangerous germs is destroyed, and the boiling point of milk is a little above 212°. The temperature of tea or coffee, as ordinarily used, may do it, but is not to be relied upon. I need only refer generally to the cases of wholesale infection that have recently been traced to the milk of particular dairies, as the particulars are familiar to all who read the newspapers.

It is an open question whether butter may or may not act as a dangerous carrier of such germs; whether they rise with the cream, survive the churning, and flourish among the fat. The subject is of vital importance, and yet, in spite of the research-fund of the Royal Society, the British Association, &c., we have no data upon which to base even an approximately sound conclusion.

We may theorise, of course; we may suppose that the bacteria, bacilli, &c., which we see under the microscope to be continually wriggling about or driving along are doing so in order to obtain fresh food from the surrounding liquid, and therefore that if imprisoned in butter they would languish and die. We may point to the analogies of ferment-germs which demand nitrogenous matter, and therefore suppose that the pestiferous wanderers cannot live upon a mere hydro-carbon like butter. On the other hand, we know that the germs of such things can remain dormant under conditions that are fatal to their parents, and develop forthwith when released and brought into new surroundings. These speculations are interesting enough, but in such a matter of life and death to ourselves and our children we require positive facts, direct microscopic evidence.

In the meantime the doubt is highly favourable to *bosch.* To illustrate this, let us suppose the case of a cow grazing

on a sewage-farm manured from a district on which enteric fever has existed. The cow lies down and its teats are soiled with liquid containing the germs which are so fearfully malignant when taken internally. In the course of milking a thousandth part of a grain of the infected matter containing a few hundred germs enters the milk, and these germs increase and multiply. The cream that rises carries some of them with it, and they are thus in the butter, either dead or alive, we know not which, but have to accept the risk.

Now, take the case of bosch. The cow is slaughtered. The waste fat, that before the days of palm oil and vaseline was sold for lubricating machinery, is skilfully prepared, made up into 2 lb. rolls, delicately wrapped in special muslin or prettily moulded and fitted into "Normandy" baskets. What is the risk in eating this?

None at all provided always the bosch is not adulterated with cream-butter. The special disease germs do not survive the chemistry of digestion, do not pass through the glandular tissues of the follicles that secrete the living fat, and therefore, even though the cow should have fed on sewage grass, moistened with infected sewage water, its fat would not be poisoned.

What we require in connection with this is commercial honesty, that the thousands of tons of bosch now annually made be sold as bosch, or if preferred as "oleomargarine," or "butterine," or any other name that shall tell the truth. In order to render such commercial honesty possible to shopkeepers, more intelligence is demanded among their customers. A dealer, on whom I can rely, told me lately that if he offered the bosch or butterine to his other customers as he was then offering it to me at 8½d. per lb. in 24 lb.-box, or 9d. retail, he could not possibly sell it, and his reputation would be injured by admitting that he kept it; but that the same people who would be disgusted with it at 9d. will buy it freely at double the price as prime Devonshire fresh butter; and, he added, significantly, "I cannot afford to lose my business and be ruined because my customers are fools." To pastrycooks and others in business, it is sold honestly enough for what it is, and used instead of butter.

Before leaving the subject of animal food I may say a few words on the latest, and perhaps the greatest, triumph of science in reference to food supply—*i.e.*, the successful solution of the great problem of preserving fresh meat for an almost indefinite length of time. It has long been known that meat which is frozen remains fresh. The Aberdeen whalers were in the habit of feasting their friends on returning home on joints that were taken out fresh from Aberdeen and kept frozen during a long Arctic voyage. In Norway game is shot at the end of autumn, and kept in a frozen state for consumption during the whole winter and far into the spring.

The early attempts to apply the freezing process for the carriage of fresh meat from South America and Australia by using ice, or freezing mixtures of ice and salt, failed, but now all the difficulties are overcome by a simple application of the great principle of the conservation of energy, whereby the burning of coal may be made to produce a degree of cold proportionate to the amount of heat it gives out in burning.

Carcasses of sheep are thereby frozen to stony hardness immediately they are slaughtered in New Zealand and Australia, and then packed in close refrigerated cars, carried to the ship, and there stowed in chambers refrigerated by the same means, and thus brought to England in the same state of stony hardness as that originally produced. I dined to-day on one of the legs of a sheep that I bought a week ago, and which was grazing at the Antipodes three months before. I prefer it to any English mutton ordinarily obtainable.

The grounds of this preference will be understood when I explain that English farmers who manufacture mutton as a primary product, kill their sheep as soon as they are full grown, when a year old or less. They cannot afford to feed a sheep for two years longer merely to improve its flavour without adding to its weight. Country gentlemen who do not care for expense, occasionally regale their friends on a haunch or saddle of three-year-old mutton, as a rare and costly luxury.

The antipodean graziers are wool growers. Until lately, mutton was merely used as manure, and even now it is but a secondary product. The wool crop improves year by year until the sheep is three or four years old; therefore, it is not slaughtered until this age is attained, and thus the sheep sent to England are similar to those of the country squire, and such as the English farmer could not send to market under eighteenpence per pound.

There is, however, one drawback; but I have tested it thoroughly, having supplied my own table during the last six months with no other mutton than that from New Zealand, and find it so trifling as to be imperceptible unless critically looked for. It is simply that in thawing, a small quantity of the juice of the meat oozes out. This is more than compensated by the superior richness and fulness of flavour of the meat itself, which is much darker in colour than young mutton.

GAMBLING SUPERSTITIONS.

BY RICHARD A. PROCTOR.

(*Continued from page* 20.)

IT may appear paradoxical to say that chance results right themselves—nay, that there is an absolute certainty that in the long run they will occur as often (in proportion) as their respective chances warrant, and at the same time to assert that it is utterly useless for any gambler to trust to this circumstance. Yet not only is each statement true, but it is of first-rate importance in the study of our subject that the truth of each should be clearly recognised.

That the first statement is true, will perhaps not be questioned. The reasoning on which it is based would only suit the Mathematical Column; but it has been experimentally verified over and over again. Thus, if a coin be tossed many thousands of times, and the numbers of resulting "heads" and "tails" be noted, it is found, not necessarily that these numbers differ from each other by a very small quantity, but that their difference is small compared with either. In mathematical phrase, the two numbers are nearly in a ratio of equality. Again, if a die be tossed, say six million times, then, although there will not probably have been exactly a million throws of each face, yet the number of throws of each face will differ from a million by a quantity very small indeed compared with the total number of throws. So certain is this law, that it has been made the means of determining the real chances of an event, or of ascertaining facts which had been before unknown. Thus, De Morgan relates the following story in illustration of this law. He received it "from a distinguished naval officer, who was once employed to bring home a cargo of dollars." "At the end of the voyage," he says, "it was discovered that one of the boxes which contained them had been forced; and on making further search a large bag of

dollars was discovered in the possession of some one on board. The coins in the different boxes were a mixture of all manner of dates and sovereigns; and it occurred to the commander, that if the contents of the boxes were sorted, a comparison of the proportions of the different sorts in the bag, with those in the box which had been opened, would afford strong presumptive evidence one way or the other. This comparison was accordingly made, and the agreement between the distribution of the several coins in the bag and those in the box was such as to leave no doubt as to the former having formed a part of the latter." If the bag of stolen dollars had been a small one the inference would have been unsafe, but the great number of the dollars corresponded to a great number of chance trials; and as in such a large series of trials the several results would be sure to occur in numbers corresponding to their individual chances, it followed that the number of coins of the different kinds in the stolen lot would be proportional, or very nearly so, to the number of those respective coins in the forced box. Thus, in this case the thief increased the strength of the evidence against him by every dollar he added to his ill-gotten store.

We may mention, in passing, an even more curious application of this law, to no less a question than that much-talked-of but little understood problem, the squaring of the circle. It can be shown by mathematical reasoning, that, if a straight rod be so tossed at random into the air as to fall on a grating of equidistant parallel bars, the chance of the rod falling through depends on the length and thickness of the rod, the distance between the parallel bars, *and* the proportion in which the circumference of a circle exceeds the diameter. So that when the rod and grating have been carefully measured, it is only necessary to know the proportion just mentioned in order to calculate the chance of the rod falling through. But also, if we can learn in some other way the chance of the rod falling through, we can infer the proportion referred to. Now the law we are considering teaches us that if we only toss the rod often enough, the chance of its falling through will be indicated by the number of times it actually does fall through, compared with the total number of trials. Hence we can estimate the proportion in which the circumference of a circle exceeds the diameter by merely tossing a rod over a grating several thousand times, and counting how often it falls through. The experiment has been tried, and Professor De Morgan tells us that a very excellent evaluation of the celebrated proportion (the determination of which is equivalent in reality to squaring the circle) was the result.

And let it be noticed, in passing, that this inexorable law—for in its effect it is the most inflexible of all the laws of probability—shows how fatal it must be to contend long at any game of pure chance, where the odds are in favour of our opponent. For instance, let us assume for a moment that the assertion of the foreign gaming bankers is true, and that the chances are but from $1\frac{1}{4}$ to $2\frac{1}{2}$ per cent. in their favour. Yet in the long run, this percentage must manifest its effects. Where a few hundreds have been wagered the bank may not win $1\frac{1}{4}$ or $2\frac{1}{2}$ on each, or may lose considerably; but where thousands of hundreds are wagered, the bank will certainly win about their percentage, and the players will therefore lose to a corresponding extent. This is inevitable, so only that the play continue long enough. Now, it is sometimes forgotten that to ensure such gain to the bank, it is by no means necessary that the players should come prepared to stake so many hundreds of pounds. Those who sit down to play may not have a tithe of the sum necessary—if only wagered once—to ensure the success of the bank. But every florin the players bring with them may be, and commonly is, wagered over and over again. There is repeated gain and loss, and loss and gain; insomuch that the player who finally loses a hundred pounds, may have wagered in the course of the sitting a thousand or even many thousand pounds. Those fortunate beings who "break the bank" from time to time, may even have accomplished the feat of wagering millions during the process which ends in the final loss of the few thousands they may have begun with.

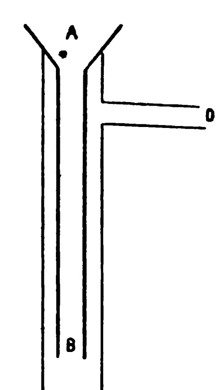
(*To be continued*).

PLEASANT HOURS WITH THE MICROSCOPE.

By HENRY J. SLACK, F.G.S., F.R.M.S.

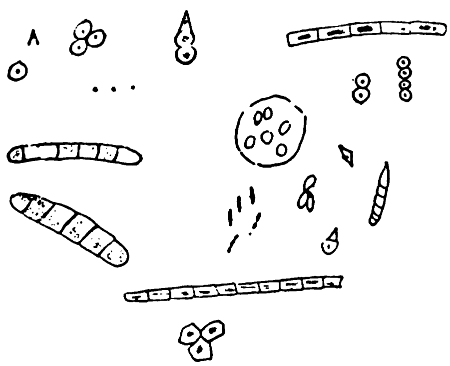
MUCH interest has been excited lately in the character of atmospheric dust, in the belief that we have been passing through a meteor stream of very minute bodies, which may have caused the splendid after-glows following sunsets, and the similar appearances before sunrise. Besides the investigations of Captain Noble and Mr. Mattieu Williams in this country, similar examinations of atmospheric dust on the Continent have led to like results; and it was stated in the French Academy that there was reason to believe an unusual quantity of fine meteoric matter fell in the second half of November and the beginning of December. Collecting snow as soon as it falls in the open country, letting it thaw in a perfectly clean vessel, and viewing the matter deposited under the microscope, seems the most successful way of catching these particles, free from much admixture; but at any time, in ordinary localities, the air is found to contain a multitude of small objects. In and near towns the quantity is much greater than away from them in open country places, or at considerable heights, far from habitations, where the air is often pure. Snow, however, in such places, would be likely to yield meteoric dust.

There are many ways of obtaining atmospheric dust. It may be swept from roofs, ledges in church-steeples, gutters, &c. Placing a sheet of glass, moistened with glycerine, so as to catch the wind, or putting the glass on the ground so as to receive whatever may fall perpendicularly, answers very well. Another plan is to use an aspirator, the principle of which is shown in the annexed diagram. It is evident that if a stream of water runs from A to B, and falls through C, it will carry with it air from C, which will be replaced by a fresh supply entering at D. If the tube D is stopped loosely with a tuft of pyroxilin, which is similar to gun-cotton, but less explosive, it filters the air and stops all solid particles. This substance readily dissolves in a mixture of strong alcohol and ether, and all particles not dissolved by those agents may be obtained by giving them time to deposit, or by filtering.

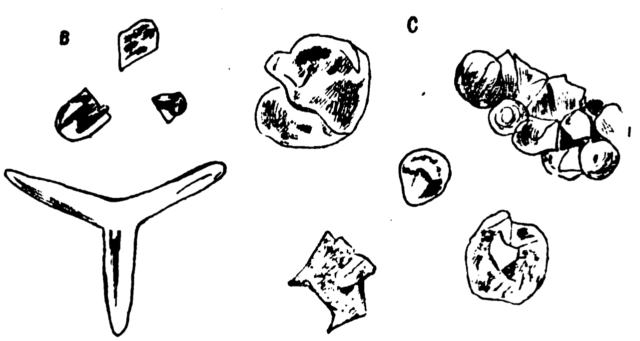


The writer was from home at the period of the snowfall which yielded meteoric matter to Captain Noble and Mr. Williams; but some water in a rain-gauge, the product of snow melting, did show some minute bodies like a finely-powdered specimen of a meteorite, but too small, and not numerous enough for positive identification by chemical means.

After some days of fog, the dust on windows facing prevailing winds was removed with a clean sponge and a little rain-water. Some particles were quickly deposited at the bottom of a tumbler in which the sponge had been drained. Many more did not fall in less than two days, and the water was still turbid. The heavier particles were found to be Ashdown sand from the adjacent open forest. Besides these were bits of mineral matter not identified—some, perhaps, meteoric. The majority of the objects were of vegetable nature, fresh-water algæ, spores of fungi, and an immense quantity too small for positive identification, but certainly organic. There were very few of the fragments of clothing common in the air of towns, few smuts, and few starch grains, which are sometimes plentiful



in less open places. The principal things discoverable with powers of half-inch, one-sixth, and one-eighth, magnifying respectively about 100, 320, and 450 linear, are shown in the figures. The objects in group A are vegetable × 100; B mineral, except the three-cornered ones, which are plant



hairs; C also mineral. In this damp season spores of fungi are deposited abundantly in all directions. If some of the green stuff forming thin layers on moist stones, or the green on fruit-trees, is scraped off, fungi spores will be found, as well as protococcus and other simple algæ. The minute algæ has a glaucous or a bright green tint, while most of the fungi spores are brownish.

If the remarkable glow aspects continue, or occur again, the search for meteoric dust will be of importance. M. Estrelle, writing in *Ciel et Terre*, observes that the zone of coloured light which all have admired is high up, "twenty kilomètres and more above the earth"—thirteen miles and more. The matter which causes it "is not water, because water at that height would be congealed by the cold that prevails in such regions. It is not ice, because ice-particles would make halos, and instead thereof a uniform tint has been seen without any symmetrical arrangement."

It is convenient to use a magnet in examining dust. If it attracts particles, they may or not be meteoric, and, on the other hand, they may be meteoric when not attracted. For example, I apply a magnet to a little powder from a large Australian meteorite, and no particle is attracted. Sometimes iron particles are carried a long way from smelting-works and engineering factories. Dust containing flattened iron particles, arising from the friction of locomotion on rails, has been found in railway carriages, together with others of a different shape, supposed to come from other sources.

The more free the air is from solid particles, the better for health. Many things found in air in and near towns, and especially the great manufacturing cities, are very mischievous as mechanical or chemical irritants; and such things as vegetable spores, even when not of a poisonous character, must interfere with the action of the delicate lung-cells. The enormous quantity of spores of various sorts that may be inhaled was shown by Dr. Angus Smith, who calculated from data obtained by examination of Manchester air, that, besides rubbish of all kinds, a man, in the course of ten hours, might breathe into his lungs 37½ millions of spores visible with a magnification of 1,600 diameters. The vitality of the lung-cells must be injured by such treatment, and rendered susceptible of disease, if any of the tubercle bacteria happen to enter them. Preparations of portions of human lung from workers in coal-mines look like miniature coal-cellars, and one wonders how life could be prolonged to even the low average that is attained under such conditions.

Starch cells in atmospheric dust are easily coloured blue with iodine. Sulphuric acid dissolves them, but does not readily destroy fungoid spores. Testing for iron is easy. A drop of dilute nitric acid will dissolve some of the metal, and ferrocyanate of potash readily gives the characteristic Prussian blue. Detecting a minute proportion of nickel in a very small quantity of meteoric material requires the practised skill of an experienced analyst. In summer time pollen grains are commonly found in the air. Hay fever is supposed to be caused by the pollen of grasses. Helmholz found a solution of quinine injected into the nose acted as a specific, but whether the pollen or some more minute organism causes the disease is still doubtful.

WILD BEES.

By S. A. Butler, B.A., B.Sc.

To most people, in all probability, the expression "*wild* bees" is nothing more than a name. We are all familiar enough with the honey-collecting and comb-forming propensities of the industrious communities which have for so many ages been domesticated, so to speak, by man; and as these particular habits of this particular species form its sole commercial recommendation, the word "bee" usually suggests, in this utilitarian age, only ideas of that "little

busy bee," which, according to the poet, if not according to the naturalist, delights to—

"Gather honey all the day
From every opening flower."

At the outset, therefore, let me say that I shall make no reference to the species, *Apis mellifica*, the Honey, or Hive Bee, which is the inhabitant of the hives of our thrifty villagers. These are not really *wild* bees; they can scarcely merit the epithet while their convenience and wellbeing are so carefully studied as in the modern apiarium. But there are to be found in the woods and fields of our country upwards of 200 different kinds of insects which structurally bear a close resemblance to our friends of the hive, but lead an existence entirely independent of the cherishing care of man, and are therefore truly "wild bees." Many of them are somewhat obscure-looking insects, and, when on the wing, might readily be mistaken for hive bees, or even for flies. The minute details of structure, which are often of the greatest significance with respect to the habits of the insects, and which, on an attentive and close examination, sufficiently separate them from the hive bee, cannot, of course, be discerned by the hasty glance which is all that is possible as the insect buzzes past on its industrious flight. To gain an intimate knowledge of these insects, as of all others, we must catch them, minutely examine them, even with the aid of a microscope, pull them to pieces, compare them with one another part by part; and when we have done this, we shall be struck with the extraordinary diversity that exists under the external aspect of similarity. It will be well to notice some structural points that all bees have in common, that we may form a clear conception of what a bee is really like, and may be able to recognise such insects when we meet with them in our country rambles—not so easy a task, perhaps, as it may seem at first sight.

In the first place, a bee is a hymenopterous insect, *i.e.*, it has four membranous wings, the fore pair being much larger than the others. Now, this alone will serve to distinguish bees from certain two-winged flies that are very generally mistaken for bees. It should be mentioned, however, that when the wings are expanded, the hind pair are so closely hooked on to the fore pair that the two may easily be mistaken for a single wing. But it will not do to rely wholly upon the number, relative size, and texture of the wings; there are hundreds of other insects that have wings of the same kind; there is one point about these organs, however, that should be attended to, as it will often help to show what insects are *not* bees, even if it does not determine what are: I refer to the neuration of the wings. The rays which form the strength and support of the wings are very constant in their arrangement in the same species, and generally in the same genus, while differing considerably in different genera. They are not, as many might suppose, set in the wing at random; but there is always some definite arrangement of them—so definite, indeed, as to render possible the giving of names both to the nervures and to the cells they form by their intercrossing. Towards the tip of the fore-wing (Fig. 1), on the front margin, there are two such cells, the one nearer the body opaque, and called the stigma; and the other transparent, and called the marginal cell. Below these there are always two or three other cells placed one after another in a row; these are called submarginal cells, and no bee has less than two or more than three of these; if, therefore, you find an insect that, in other respects, looks something like a bee, and yet has only one submarginal cell, you will know that this is the key to other important differences which a careful inspection will reveal, and which remove the creature—not, indeed, from the Hymenoptera, but from that section of the order which contains the bees. All the most familiar bees, such as the hive bee, the bumble-bee, the Andrenæ, Halicti, and Anthophoræ, have three submarginal cells; it does not follow, however, that if a hymenopterous insect has two or three submarginal cells, it is therefore a bee.

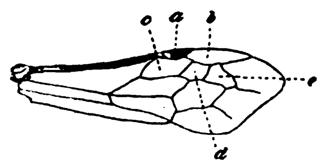


Fig. 1.—Fore-wing of Andrena pilipes.
a, stigma. *b*, marginal cell.
c, *d*, *e*, submarginal cells.

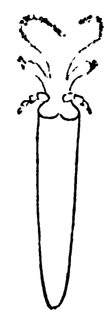


Fig. 2.—Tongue of Colletes Daviesana, a Plastering Bee.

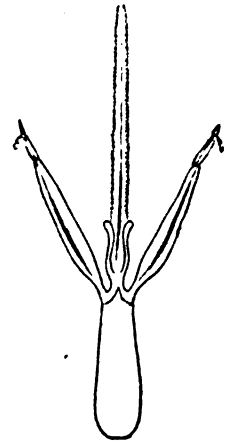


Fig. 3.—Tongue of Bombus terrestris, a yellow-tailed Humble-Bee.

Bees have a pair of powerful mandibles, accompanied by a licking apparatus of somewhat complicated structure, and often of considerable length, and called popularly the tongue, technically the labium (Figs 2 and 3). This structure, especially in its longer forms, is very characteristic of the group. The antennæ are stout, and generally of moderate length, but longer in the males than in the females. The sting is a curiously-modified ovipositor, and therefore exists only in the females. The males, which may generally be recognised by the superior length of their antennæ, may be handled with impunity, for they are quite unable to inflict any injury. All bees are vegetarians, their food consisting principally of pollen. The three so-called sexes—viz, males, females, and neuters—are to be found only amongst those few bees that are social—*i.e.*, form large communities living in the same nest. You will look in vain in the nests of other bees for neuters; only males and females are to be found. The greater number of bees have some part or other of the body clothed with hairs, longer or shorter, called collectively "pubescence." One very important use of this is to assist in the collection or conveyance of pollen. Such are the principal characteristics of bees, and attention to them can scarcely fail to lead the observer to the correct separation of bees from the rest of the Hymenoptera.

But the creatures are not always thus. During the course of their life they pass through a series of marvellous changes. There is first the egg, then the grub or larva, then a motionless little doll or pupa, and then, finally, the fully-developed bee. Before the little creature commences its existence as an egg, however, many preparations have to be made by its mother for its reception into the world and its nourishment therein; in fact, with the deposition of the egg the maternal duties cease. First, a suitable situation has to be selected for the home of the expected offspring. The nature of the spot fixed upon will

vary with the species; some choose a sandy bank, others a dead bramble stick, others again the chinks in a wall, and yet others an old snail-shell; in fact, there are very few things that offer cavities of any kind which may not be selected by some species or other. If there is no cavity ready-formed, in which to place the egg, the mother must set to work to make one; this she does by means of her powerful mandibles, and a substance must be hard indeed to resist the incessant attacks of these powerful weapons. By dint of continuous snippings, she bites out fragment after fragment of the rock or wood, until a hole large enough to admit her body is made. Using this as a starting-point, she tunnels out to the depth of several inches—sometimes a single burrow, sometimes a number of ramifying channels—carefully smoothing off all asperities as she proceeds. The burrows are frequently lined, either with a kind of silk secreted by the insect, or with fragments of leaves, all cut most carefully to pattern, and laid so as to overlap.

(To be continued.)

HOW TO CHOOSE A TRICYCLE.

By John Browning,

Chairman of the London Tricycle Club.

Take care that any machine you purchase is provided with good foot-rests, for the purpose of putting the feet on when running down hill. These should be well raised, so as to bring the feet as high as possible, as a change from the vertical position they should take when pedalling, and they should be covered with stout indiarubber tube, to prevent jarring the feet as much as possible. In this respect they will be improved if, instead of being attached directly to the front portion of the frame which carries the fork of the steering-wheel, they are mounted on a strong flat spring. These remarks apply only to machines of the Salvo type. It is a good plan to have the pedals run on a clutch. This is a contrivance by which the pedals drive the lower chain wheel continually when you are pedalling forward; but on descending an incline, as soon as the machine is running as fast as you are pedalling, the pedals stand still, and become available as foot-rests. The clutch is also extremely useful in another way; it will save you from ever becoming fixed when riding up a hill or working over rough ground, by getting your machine on dead centres—that is one pedal at the top and the other at the bottom—in which position you would probably have to dismount from inability to move the machine. A slight turn backwards of one of the cranks when you are using a clutch brings it into a position of full power. Of course, it is impossible to back the machine when it has a clutch as just described unless it is provided with some contrivance for throwing the clutch out of action; but this is of little or no consequence unless the machine has to be used constantly in the traffic of crowded thoroughfares.

I reckon a good clutch worth about one mile in ten, probably; and certainly worth one mile in fifteen, at least, when riding over constantly undulating roads.

I wish it to be distinctly understood that throughout my articles I exclude both the "Humber" and the "Coventry" when I speak generally of front or rear-steering machines. Each of these machines may best be considered as constituting a class in itself, though I would respectfully submit to those who dissent from the views I have expressed as to the efficiency of machines with small wheels, that even the "Humber" tricycle is not exempt from the laws which govern Mechanics.

I find from a comparison of a number of notes that I have made, that for every decrease of 2 lb. in the weight of a tricycle the machine will travel roughly about one mile further in a day without extra exertion, so that if a rider can ride fifty miles in a day on a machine weighing 90 lb., he will be able to ride sixty miles on a machine weighing 70 lb.

It is a commonly accepted notion that little or no weight must be thrown on the steering-wheel. This is a complete mistake. A certain amount of weight may be thrown upon the steering-wheel with advantage. But to get the best results the steering-wheel should be larger and the driving-wheel should be smaller than they are now generally made. We should have machines with 20-in. steering-wheels and 40-in. driving-wheels, instead of 14-in. steering-wheels and 48-in. driving-wheels as at present. A reduction in the size of the driving-wheels of 2 in. saves pounds in weight, while a reduction in the steering-wheel of 2 in. only saves a few ounces.

Have the rubber tyre of the steering-wheel at least one-eighth of an inch larger than the tyres of the driving-wheels, as it will lessen vibration greatly. Manufacturers commonly make the rubber tyre on the steering-wheel smaller than those on the driving-wheels. Do not have such a machine.

In my "Cobweb" sociable I have had the steering-wheel made 18 in. and the driving-wheels 36 in. The tyre of the steering-wheel is seven-eighths, and those on the driving-wheels are three-quarters. The weight of the riders and machine is 332 lb., and the weight on the front wheel is 85 lb. This machine drives much more easily than any other sociable I have ridden.

Choose a machine which is enamelled plain black with Harrington's enamel. It has a very superior look to paint, it seldom gets rubbed off, and if it does get injured you can easily make good the injury yourself with a little of the best black varnish enamel, sold by all tricycle agents.

Do not have a lot of nickelised work about the machine. It is a deal of trouble to keep it clean, and it is scarcely any protection to the steel work against rusting.

If you should think of testing a tricycle before you buy one, which I think is very probable, mind you begin first by practising on a nearly level road. Do not attempt to ride down a steep hill, *nor yet up one.* Either is likely to prove dangerous until you can pedal strongly and evenly, steer well, and use the brake. I can imagine some reader asking, What danger can there be in trying to ride up a steep hill? Why, it is that if you come to a standstill from finding the work too severe for you, and the machine begins to run backwards, and you attempt to stop it by putting the brake on, you will find either that it will not stop the machine instantly, or that, if it does, the machine will turn over backwards upon you.

In my next article I will relate a few of the results of my experiments in testing tricycles, so that others may profit by my experience.

VARIATION IN ANIMALS.

A multitude of variations have arisen, for instance in colour and in the character of the hair, feathers, horns, &c., which are quite independent of habit and of use in previous generations. It seems far from wonderful, considering the complex conditions to which the whole organisation is exposed during the successive stages of its development from the germ, that every part should be liable to occasional modifications: the wonder indeed is that any

two individuals of the same species are at all closely alike. If this be admitted, why should not the brain, as well as all other parts of the body, sometimes vary in a slight degree, independently of useful experience and habit? Those physiologists, and there are many, who believe that a new mental characteristic cannot be transmitted to the child except through some modification of that material substratum which proceeds from the parents, and from which the brain of the child is ultimately developed, will not doubt that any cause which affects its development may, and often will, modify the transmitted mental characters. With species in a state of nature such modifications or variations would commonly lead to the partial or complete loss of an instinct, or to its perversion; and the individual would suffer. But if under the then existing conditions any such mental variation was serviceable, it would be preserved and fixed, and would ultimately become common to all the members of the species.—CHARLES DARWIN.

TUMBLER PIGEONS.

THE tumbling of the pigeon is a habit which, if seen in a wild bird, would certainly have been called instinctive; more especially if, as has been asserted, it aids these birds in escaping from hawks. There must have been some physical cause which induced the first tumbler to spend its activity in a manner unlike that of any other bird in the world. The behaviour of the ground-tumbler or Lotan of India, renders it highly probable that in this sub-breed the tumbling is due to some affection of the brain, which has been transmitted from before the year 1600 to the present day. It is only necessary gently to shake these birds, or, in the case of the Kalmi Lotan, to touch them on the neck with a wand, in order to make them begin rolling over backwards on the ground. This they continue to do with extraordinary rapidity, until they are utterly exhausted, or even, as some say, until they die, unless they are taken up, held in the hands, and soothed; and then they recover. It is well known that certain lesions of the brain, or internal parasites, cause animals to turn incessantly round and round, either to the right or left, sometimes accompanied by a backward movement: and Mr. W. J. Moore (*Indian Medical Gazette*, Jan. and Feb., 1873) gives an account of the somewhat analogous result which followed from pricking the base of the brain of a pigeon with a needle. Birds thus treated roll over backwards in convulsions, in exactly the samé manner as do the ground-tumblers; and the same effect is produced by giving them hydrocyanic acid with strychnine. One pigeon which had its brain thus pricked recovered perfectly, but continued ever afterwards to perform summersaults like a tumbler, though not belonging to any tumbling breed. The movement appears to be of the nature of a recurrent spasm or convulsion, which throws the bird backwards, as in tetanus; it then recovers its balance, and is again thrown backwards. Whether this tendency originated from some accidental injury, or, as seems more probable, from some morbid affection of the brain, cannot be told; but at the present time the affection can hardly be called morbid in the case of common tumblers, as these birds are perfectly healthy and seem to enjoy performing their feats, or, as an old writer expresses it, "showing like footballs in the air." The habit apparently can be controlled to a certain extent by the will. But what more particularly concerns us is that it is strictly inherited. Young birds reared in an aviary which have never seen a pigeon tumble, take to it when first let free. The habit also varies much in degree in different individuals and in different sub-breeds; and it can be greatly augmented by continued selection, as seen in the house-tumblers, which can hardly rise more than a foot or two above the ground without going head over heels in the air. —CHARLES DARWIN.

THE CANADIAN PORCUPINE.

FROM time immemorial the belief has existed that the porcupine can project its quills through the air like arrows at an enemy, and beyond this the popular mind is yet more in error as regards the structure and habits of this aberrant and curious mammal. Let us therefore consider some of the more prominent points in its life history.

The female porcupine during the last of April or the first of May builds a rough nest in some hollow tree or rock fissure, and there brings forth usually two, sometimes three, young ones. The mother is exceedingly shy until the young are weaned, and but few observations have been made upon them during the period of suckling: probably like all rodents they mature very rapidly and are soon able to shift for themselves. This species is one of the slowest and most clumsy of quadrupeds; safe in its protective armour, it seldom makes much effort to escape when surprised on the ground, but placing its muzzle between its fore legs, erecting its spines, and whisking rapidly its short tail, waits on the defensive—and even the panther and formidable grizzly bear are obliged to retreat from this fine array of bayonets.

The spines vary much in size and shape, varying from the coarse brown hair with which they are mingled to strong three-inch spikes, one-eighth of an inch in diameter. Their bases are white, and the points dark brown, the latter portion well provided with sharp, recurved barbs. Being but loosely rooted in the skin, when roughly touched the points penetrate, the barb holds fast, and the quill comes off attached to the offending body; doubtless from this arose the fable that the animal can *shoot* its quills. When the sharp spines once penetrate the skin of an animal, owing to the peculiar set of the barbs, the muscular movements of the wounded part cause them to work their way inward, and a very serious wound is finally the result. Panthers, wolves, and wild-cats have frequently been found dead with hundreds of quills embedded in their fore feet and mouths, thus proving fatal. Dogs are also frequently killed and injured, and, in consequence, the porcupine is hated and always mercilessly killed by hunters whenever found. The food of the hedgehog—as the porcupine is almost universally called by woodsmen—consists of the inner bark, and at times the leaves, of trees. When pressed by hunger it will devour the bark of almost any species, but the hemlock and spruce seem to constitute its favourite food. The young and succulent trees are usually the ones selected, and the animal seldom leaves one until it has been entirely stripped of its bark. But the porcupine seems to be almost omnivorous, for in captivity it will eat almost any vegetable substance. In the Adirondack wilderness—where this species abounds—they are frequent visitors at deserted camps, trying their powerful incisor teeth on all that comes in their way. It is exceedingly unsafe to leave one of the light cedar canoes there used anywhere in the woods unguarded for a day or two, for the hedgehog seems to have a decided liking for oil paint and varnish, and will cut down the entire side of a boat in a very few days. I have seen many boats so rendered useless. As may be imagined, they are not much beloved by the guides, among whom "the d—d hedgehogs" is a favourite topic on which to let off steam when a boat leaks.

As already suggested, the porcupine is a capital tree-climber, its strong, hand-like fore feet and long claws being perfectly adapted for the purpose. It uses these paws to hold food when eating, sitting on its haunches in the manner of a squirrel.

It does not hibernate, but remains active during the winter, clearing the snow away from the tree branches, and living entirely on their bark. At times it forms a den in a hollow tree near its feeding grounds in which to pass the night.

The porcupine quill-work of the Indians—the quills being stained various colours—is too well known to need description. Its flesh is also eaten by both whites and Indians, and is said to resemble pork.

This species becomes very tame and gentle in confinement, readily learning to take food from the hand, and never elevating its quills when stroked or taken in the arms of those who are kind to it.

The Canada Porcupine.

The Canada porcupine (the *Erethizon dorsatus* of zoologists) scarce needs any description; a short, heavily-built animal, 38 in. in entire length, with a short tail, huge yellow incisor rodent teeth, two above and two below, the skin provided with a thick mat of erectile spines, are sufficiently prominent characters to identify the animal at a single glance.

The genus *Erethizon* presents many interesting details of structure. As is the case in the beaver, the molar teeth resemble in structure those of the horse, being formed of complex infoldings of dentine bounded with enamel, and the valleys between filled with cementum—the best arrangement imaginable for grinding thoroughly its coarse and fibrous food. It is exceedingly interesting that the beaver, feeding on the same substances, should present the same tooth structure.

The infraorbital foramen—in most mammals of very small size and transmitting only the infraorbital nerve and blood-vessels—is of enormous size, and through it passes the great muscle which closes the lower law—the masseter; by this arrangement great leverage and strength is given to the inferior jaw.

A porcupine found in Java shows the curious anomaly of a tongue provided with tough, horny plates, but this is not the case with the American representative of the genus. The right lung of the *Erethizon* is stated to be twice the size of the left, but my own dissections would not put the limit at more than one-third larger.

The whole muscular system is exceedingly well developed, and the skin is well supplied with powerful special muscles to erect the spines.

The Canada porcupine is essentially a northern animal, seldom being found as far south as Virginia. A western variety is said to be found as far south as Mexico, but only, I believe, on high plateaus of temperate climate. It has been found as far north as latitude 67°. In the North Woods of New York State, as already stated, I have found it abundant; a few yet remain in the wildest portions of Pennsylvania; but this is one of the many animals doomed to rapid extinction, and every year it becomes rarer.—*Scientific American.*

THE UNIVERSE OF SUNS.

By RICHARD A. PROCTOR.

(*Continued from p.* 35.)

IN passing to the work of Sir W. Herschel as a student of the constitution of the universe, I cannot but express some degree of surprise that so little has been done to bring the records of his labours properly before the students of astronomy. His papers merely collected into a volume would form a most important accession to astronomical literature. But if suitably edited and illustrated by the work of his son and of others who have succeeded him in his own field of labour, the volumes would do more to advance the study of sidereal astronomy than any work which has been published during the last century. What has hitherto been done in making Herschel's words and work public (in England at least) has been rather an injustice than otherwise to his memory.* It seems to have been supposed that Herschel's own account might be treated as we should treat such a work as his son's "Outlines of Astronomy," that extracts might be made from any part of any paper without reference to the position which that paper chanced to occupy in the complete series. It does not seem to have been noticed that not only was there a progression in the ideas (as well as in the work) of the great astronomer, but that there was a complete change in his opinions during the long course of his labours. Hence views expressed by him in his earlier papers are not uncommonly in strong contrast with those which he advocated in later years; opinions which he regarded as almost certainly just at one time were rejected as most probably incorrect after a few years of fresh labour; and whereas in 1785 he enunciated with some definiteness a theory respecting the constitution of the universe, he not only abandoned this theory (implicitly) in 1811, not only gave up the very principles on which it had been based, but he did not consider himself in a position during any subsequent part of his career to

* I except of course the account given by Sir J. Herschel of his father's work, as well as the account in Grant's "History of Physical Astronomy." Neither of these accounts is however complete, though so far as they extend they are accurate.

state with the same degree of definiteness any theory whatever respecting the constitution of the heavens.

It is to be premised, however, that even the theory of 1785 is not properly described in most of our textbooks of astronomy, and that some of the descriptions are the merest travesties of the noble conceptions of Sir W. Herschel. This will appear from his own words, which I shall quote as freely as space will permit.

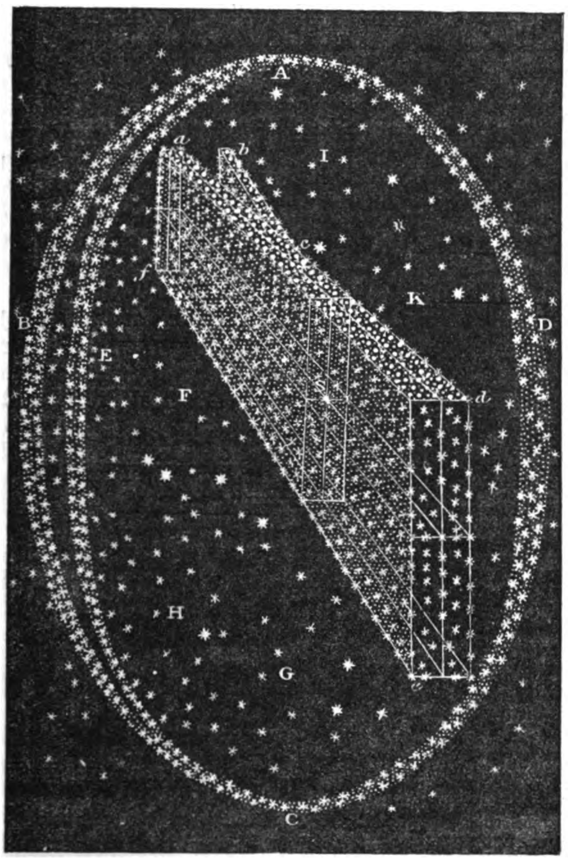


Fig. 4. Illustrating Sir W. Herschel's views in 1784.

In the year 1784 Sir W. Herschel advanced, but only in a preliminary way, the hypothesis that the Milky Way is to be regarded as the projection of our stellar system on the celestial sphere. "We gather this," he said, "from the appearance of the galaxy, which seems to encompass the whole heavens, as it certainly must do if the sun is within the same; for suppose a number of stars arranged between two parallel planes indefinitely extended every way, but at a given considerable distance from one another, and calling this a sidereal stratum, an eye placed somewhere within it will see all the stars in the direction of the planes of the stratum projected into a great circle, which will appear lucid on account of the accumulation of the stars, while the rest of the heavens at the side will only seem to be scattered over with constellations, more or less crowded, according to the distance of the planes or number of stars contained in the thickness or sides of the stratum."

"Thus, if the solar system be supposed at S, in the middle of the nebula *a b c d e f*, with two branches, *a c*, *b c* (Fig. 4), the nebula will be projected into a circle A B C D, the arches A B C, A E C, being the projection of the branches *a c*, *b c*. while the stars near the sides of the stratum will be seen scattered over the remaining part of the heavens among the spaces F, I, H, K, G. If the eye were placed somewhere without the stratum, at no very great distance, the appearance of the stars within it would assume the form of one of the lesser circles of the sphere, which would be more or less contracted according to the distance of the eye; and if this distance were exceedingly increased, the whole stratum might at last be drawn together into a lucid spot of any shape, according to the position, length, and height of the stratum."

Such was the idea broached in the paper of 1784, which has been absurdly combined with the paper of 1785, as if the two presented the same views. There can be no question that Herschel himself regarded the earlier paper as merely preliminary. The observed results described in the paper of 1785, as well as most of the inferences, are by no means in strict agreement with the paper of the preceding year. But Herschel has not thought it necessary to dwell on this point, not supposing it would seem that the paper of 1784 would one day be amalgamated with that of 1785, as though the two were on the same footing.*

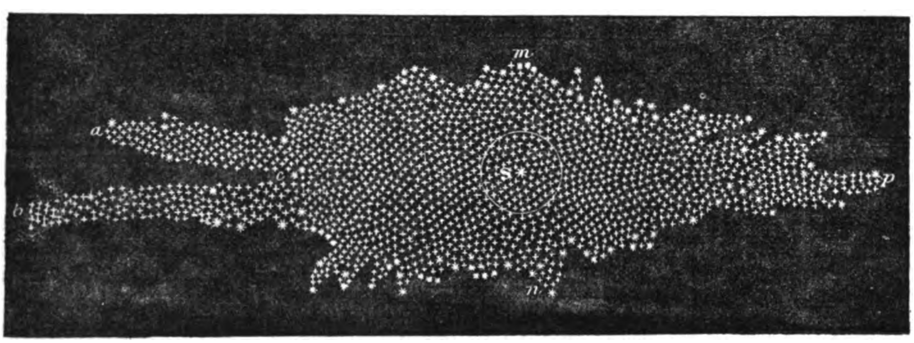


Fig. 5. Illustrating Sir W. Herschel's observations up to 1785.

* Even Arago in his Analysis of Herschel's Life and Labours, has fallen into this strange mistake; and he has been followed by all the French writers of popular treatises on astronomy. At p. 456, he says that our galactic system is a hundred times more extended in one direction than in another; and then he refers to a figure of three dimensions, constructed by Herschel on the basis of his observations. Struve justly remarks that there is a misapprehension here, since the proportion of the greatest extension to the least, in the only section Herschel based on observation, is not 100 to 1, but barely 5½ to 1. "There is also another mistake, it appears to me," adds Struve, "in Arago's Analysis. Herschel only gives a section of the Milky Way in the Memoir of 1785; in that of 1784 there is indeed a figure of three dimensions; but this figure is only given to explain the theory, though it does in some sense correspond with the ideas Herschel had formed respecting the Milky Way." For comparison with the fanciful figure here referred to, I give here, a little before its time, a picture (Fig. 5) illustrating the *results* which Sir W. Herschel was able to announce in 1785.

This may be inferred from his remarks in the paper of 1785 respecting the relative position of observation and theory—remarks which are in other respects well worth quoting. "First let me mention," he says, "that if we would hope to make any progress in an investigation of this delicate nature, we ought to avoid two opposite extremes, of which it would be hard to say which is the more dangerous. If we indulge in fanciful imagination, and build worlds of our own, we must not wonder at our going wide from the path of truth and nature; but these will vanish like the Cartesian vortices, that soon gave way when better theories were offered. On the other hand, if we add observation on observation without attempting to draw not only certain conclusions, but also conjectural views from them, we offend against the very end for which only observations ought to be made. I will endeavour to keep a proper medium; but if I should deviate from that I could wish not to fall into the latter error."

(To be continued.)

Curious Phenomenon.—At 5.25 p.m. on Friday, the 11th inst., a ball of light, shaped like a pear, with the broad end downwards, was seen from Fort William as if suspended midway between Ben Nevis and the Caledonian Valley. It descended till near the surface of the earth, and then it burst, lighting the whole valley. In colour it resembled the electric light.—*The Electrician.*

Professor Loisette's System of Memory-Training.—Since the appearance of a paragraph in Knowledge mentioning (with my sanction) that an Assistant of "ours" had tested Professor Loisette's method of training the memory, with most satisfactory results, I have received several hundreds of letters asking for my own opinion of the system. To these I have been unable to reply, first because I did not know what Professor Loisette's method might be, and secondly because the letters reached me in too great abundance. But wishing to respond to these correspondents I have taken advantage of the period of rest from travel recently enjoined me, to examine thoroughly the Loisettian system. I have not been content to try it in my own case only, but knowing how greatly memories differ in quality, I have made the inquiry in company with two relatives, whose powers of memory are very different from mine—in some points superior, in others I think not quite so good. The result in every case has been to confirm fully what my friend Dr. Andrew Wilson has said respecting the system. Whether regarded as a device for memorising, or in its more important aspect as a system of memory-training, Prof. Loisette's method appears to me admirable. I have tested it in my own case on those matters in which my memory is least trustworthy, perhaps because least exercised; and I have been surprised to find how easily and pleasantly I can fix such matters in my mind, almost without an effort, yet in such a way that I am satisfied they are there for good. My two companions in the inquiry have had in one sense the same experience, in another sense an experience entirely different; they have tested the system on entirely different subjects, but with the same satisfactory results. I have no hesitation in thoroughly recommending the system to all who are in earnest in wishing to train their memories effectively, and are therefore willing to take reasonable pains to obtain so useful a result. —R. A. Proctor.

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Reviews.

RED DEER.*

A CHARMING work by the author of "The Gamekeeper at Home." It is full of interest, and as instructive as interesting. Mr. Jefferies "can well of vénerie." The chapter on the Ways of Deer is especially delightful. Here is a story which would be thought rather startling in a novel, but being true is naturally more surprising than an invented tale:—"It happened once that a 'forester' was discovered in a certain district" (a "forester," urban reader, is the old name for a stag), "and a party was quickly formed to go out and shoot the stag. Among those who went was a man well known as a successful deer shot, upon whose aim they chiefly relied. They took with them a gallon of spirits. After some time spent in searching for the stag, and just as they were beginning to weary of the attempt, up the 'forester' jumped close to the party. A volley was fired—the muzzles almost touching the stag—but the game went off at full speed. The old gunner declared that he had hit the mark, he was sure he had aimed straight. In a minute or two as they watched the stag bounding up the hill a mile away, suddenly he dropped and lay still, evidently dead. It was found that the ball from the old gunner's weapon had grazed the stag's heart, and yet with that wound he had run upwards of a mile. No other bullet had struck him"—though the shooters were so close around him that they narrowly escaped wounding each other!—"Now" follows the dim horror of the tale": the opening is rather absurd than otherwise—"The party were so tired of walking after the stag that they did not go at once to ascertain if he was really dead or to cut his throat. They sat down in the heather to refresh themselves with the spirits, and so well did they do this that by and by the old gunner fell firm" (*sic*) "asleep. Neither blows nor shouts could arouse him, so in *order to wake him up* they set fire to the heather. Dry as tinder, the heather blazed up in such a fury of flame that they fled aside to get out of the way, leaving the sleeper to his fate. The flame passed over him as he lay, and when the wind had driven it along they found him in his burning clothes. They could not put the burning clothes out, and so carried him to the river and dipped him in. He was terribly scorched and half drowned" (we presume he was by this time awake) "and was long ill, but ultimately recovered."

WORLD LIFE.†

In this work Prof. Winchell combines together a number of known facts bearing directly or indirectly, or not at all, on the past development of the universe with a number of calculations based on various theories more or less (generally less) reconcilable with known physical laws but quite irreconcilable *inter se*, and to his satisfaction reconciles everything with his own ideas about the past of the planetary system. Hinrichs and Ennis, Alexander and Slaughter, Trowbridge, Twisden and a host of others who have put more or less grain-like chaff into fine grinding-mills and have gotten out what they take for pure flour, are questioned, quoted, and, where their results cannot be reconciled with his own, corrected by the unerring author. The nebular hypothesis which Laplace threw out only as a

* "Red Deer." By Richard Jefferies. (Longmans, Green, & Co., London.)

† "World Life; or Comparative Geology." By Prof. Alex. Winchell, LL.D. (London: Trübner & Co.)

speculation, which has been disproved along its whole length by modern physical inquiries, is taken as the foundation for a superstructure which even such a theory as Newton's gravitation would not bear. The time has not nearly come yet—perhaps it will never come—when science will be able to speak so definitely and clearly about the past of the universe as Prof. Winchell here claims to do. Yet the matter here collected would be valuable if it had not been so wildly scattered as it is.

The work is full of errors of all sorts and sizes, from the initial error of taking sand for the foundation of what is meant to be a magnificent structure down to such trivial though significant mistakes as mis-spelling names (Mitchel for Michell, Rutherford for Rutherfurd, &c.) speaking of Neison's treatise "Der Mond" whose "Moon" is a well known English work, and in other ways showing how little original inquiry our author has made into the matters of which he speaks with such cool confidence.

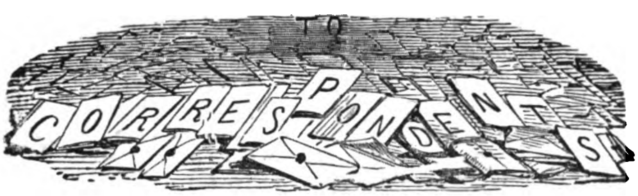
We much prefer Professor Winchell's views on Geology to his fancies about matters astronomical.

NOTES ON BOOKS.

THE TRANSIT INSTRUMENT; a manual of the Transit Instrument, as used for obtaining correct time, by *Latimer Clark*, M.I.C.E. (Messrs. E. & F. N. Spon, London.) A useful and trustworthy little work. — WHERE SHALL I EDUCATE MY SON, a manual for parents of moderate means, by *Charles Eyre Pascoe* (Houlston & Sons, London). A very useful treatise, and so far as we have been able to test the statements, reliable: the study of the work will sadden those who know something of educational opportunities abroad, and especially across the Atlantic. — TRUE AND FALSE ISSUES BETWEEN CHRISTIANITY AND SCIENCE. By the *Rev. T. Blackburn*, B.A. (W. Skeffington & Son, London). A well meant and moderately toned work of a kind liked by many, but outside our scope.—THE EARTH AND THE SOLAR SYSTEM (Moffatt & Paige, London). We learn from this work that longitude is determined by observing the exact moment when the sun attains his greatest altitude; that the earth is kept in her track by two antagonistic forces one centripetal the other centrifugal; that the earth turns on her axis in twenty-four hours; that the Pole-star remains fixed in the sky; that the distance from the Sun to Mars (see p. 51) is about the same as from Mars to the orbit of Uranus; with several other remarkable matters which surprise us a little. The anonymous author of this little work would have done well to obtain the excellent American edition of Mr. Lockyer's "Elementary Lessons in Astronomy," instead of trusting as he appears to have done, to the original version.—GREEK AND ROMAN COINS, Handbook of, by *B. V. Head*, M.R.A.S. (W. Swan Sonnenschein & Co., London). An excellent little guide (illustrated) for young collectors. Price one penny only.—PHOTOGRAPHY, Amateur's first Handbook. By *J. H. T. Ellerbeck* (D. H. Cussons & Co., Liverpool). A very useful work containing many valuable hints suggested and confirmed by experience. Sufficiently illustrated. — DR. CORPUS'S CLASS (Wyman & Sons, London). A series of nine more or less laughable lectures on the human body from what the author regards as a comic point of view. The work merits fairly what Wendell Holmes calls the "laugh eleemosynary."—WIESEN as a Health-Resort in early Phthisis, by *Dr. A. T. Tucker.* (Baillière Tindall & Cox, London). With the increasing popularity of the high altitude system of treatment of early phthisis books of this class have become not only useful but essential. This work is obviously the result of careful study. The chapter on personal health-treatment is especially valuable.—CLEOPATRA'S NEEDLE, by the *Rev James King*, M.A. (The Religious Tract Society, London) contains an interesting account of Hieroglyphics, and gives the full interpretation (with careful drawing) of each face of Cleopatra's Needle. Londoners expecting visits of country cousins, should master these interpretations. The remarks on Egyptian religious views seem narrow, when we consider how much that is now valued was derived from the land of the Pharaohs—KASHGARIA (Eastern or Chinese Turkistan) translated from the Russian of *Colonel A. N. Kuropatkin*, by *Major Walter E. Gowan.* (W. Thacker & Co., London). Although the number of persons who take special interest in Kashgaria may be somewhat limited, as much care has been taken by Col. Kuropatkin with this historical and geographical sketch of the country as if he had a wider audience. The work has been well translated by Major Gowan, and though its subject is rather special it is a valuable treatise.

Dingler's Journal recently described the explosion of an open kettle. At a factory in Neusalz a large cast-iron wash-kettle was used to hold water, into which melted iron was allowed to flow, in a moderate stream, for making iron shot. On October 23, 1882, one of the workmen by mistake allowed the iron to flow too rapidly. There was a sudden development of steam, which threw out a part of the water, frightening the labourer and causing him to drop his ladle, so that about 20 kilogrammes—44 lb.—of melted iron fell at once into the water. There was an immediate rapid outburst of steam and a loud explosion, which shattered the kettle into fragments, tore up the woodwork, threw the workman nearly 8 ft. backwards, and broke his right leg. Only very small pieces of the kettle were found where it stood; some of them were thrown to a distance of about 50 ft.

THE project of a canal across Florida, connecting the Gulf of Mexico with the Atlantic Ocean, is being warmly taken up in America. According to the report of the chief engineer, the total length of the canal will be 139½ English miles. It is proposed to make it wide enough to admit of two steamers passing through it abreast. The cost of the work is estimated at £9,000,000 sterling. When the canal is finished it will diminish the distance between New Orleans and Liverpool or New York by 412 miles. General Stone and his assistants have already completed the preliminary survey. The question the company has to determine is whether the traffic in prospect promises to be sufficiently remunerative to justify the large necessary outlay in realising the project. The chief engineer, in support of his view that the scheme will be a financial success, points out that the shortening of the distance between the Mexican Gulf ports and Europe and the North Atlantic seaboard of America will effect an economy of from three to seven days in time. This means in large vessels a saving of from £60 to £100 a day in food and wages, in addition to a saving of £100 a day in coal. The Straits of Florida are exceedingly dangerous, and shipwrecks there are very frequent. A further saving will accordingly be made in insurance on vessels, which is estimated at from 1 to 1½ per cent. A large increase in the foreign and ocean trade to Texas and the States bordering on the Mississippi will be almost certain to follow the completion of the Canal. The most elevated spot through which the Florida Canal could be cut is considerably lower than the highest point on the route through which the Suez Canal was carried. There are, in fact, no great engineering difficulties to surmount, and the realisation of the scheme is practically only a question of time.—*Engineer.*



"Let Knowledge grow from more to more."—Alfred Tennyson.

Only a small proportion of Letters received can possibly be inserted. Correspondents must not be offended, therefore, should their letters not appear.

All Editorial communications should be addressed to the Editor of Knowledge; *all Business communications to the* Publishers, *at the Office, 74, Great Queen-street, W.C.* If this is not attended to delays arise for which the Editor is not responsible.

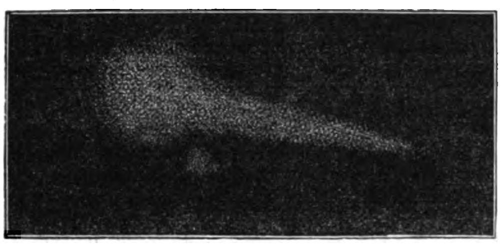
All Remittances, Cheques, and Post Office Orders should be made payable to Messrs. Wyman & Sons.

The Editor is not responsible for the opinions of correspondents.

No communications are answered by post, even though stamped and directed envelope be enclosed.

THE COMET.

[1099]—When looking at the comet a few days ago I observed a separate nebulosity quite distinct from, but near to the tail, and about half way down on the left hand (telescopic). It was circular, somewhat fainter than the tail, and about the same breadth.



Doubtless others will have seen this, but it may be well to draw the attention of any who have not seen it. Senex.

THE AFTERGLOW.

[1100]—I have to thank Mr. Mattieu Williams (letter 1093, p. 4) and Mr. Ekershaw (letter 1095, same page), for their interesting replies to my question as to the condition of the sky where it was clear on Christmas evening.

In reply to Mr. Ekershaw, I have not noticed the glow in actual daylight, but its appearance has occurred very early in the evening, and persisted to an abnormally late hour. Observations of it in actual sunlight are both interesting and important.

With reference to Mr. Ekershaw's idea that the greenness of Venus was a mere retinal effect of contrast, I would refer him to my record of an observation by Mr. Pratt, of Brighton (letter 1040), on p. 365 of your last volume. William Noble.

Forest Lodge, Maresfield, Uckfield,
Jan. 21, 1884.

STRANGE PHENOMENON.

[1101]—Had things been as "A. McD." seems to infer with regard to the "Strange Phenomenon" (1068) which I witnessed in the Persian Gulf, I would certainly not have taken up your space by asking enlightenment on a subject I could easily explain myself.

Though my letter was in no way scientific, still it was none the less true. I don't suppose "A. McD." means any harm, but I do think it's rather unjust to say a man is drunk because he sees something out of the common. If there is one thing that I pride myself in, it is being able to say that never in my life have I indulged in anything stronger than water. What I described in letter 1068 is quite true. I don't pretend to have given the exact dimensions of the wheels, nor the exact time of revolution, but the general appearance was just as I said.

The appearance could not possibly have been caused by lights from cabin windows, as I had seen all lights out at 10 p.m., except, of course, the mast-head, binnacle, and side-lights.

I should like an explanation a little more scientific, but not quite so personal. However, "No offence taken where, I suppose, none is meant." J. W. Robertson.

[We owe our correspondent an apology,—a misunderstood instruction led to the appearance of a letter which, though we are sure well-meant, was not well-advised. The opening criticism, as to the apparent size and velocity was sound enough. But the observation ostensibly recorded only impressions not measurements.—R. P.]

TRICYCLE WHEELS.

[1102]—The advice given by Mr. Browning on the size of tricycle wheels induces me to remark that I think it will be found misleading under ordinary conditions of roads, though in some instances it is correct. I am publishing a treatise especially to inform tricyclists on the various points of construction in a tricycle, and I would ask your readers to understand for themselves what their requirements are before following a personal opinion. So many advantages follow on the use of the "correct" size, that I think when the reasons that should regulate the size of the driving-wheel are known it will be found that Mr. Browning's ideas, though erring on the right side, are rather too "low."

F. Warner Jones.

CRIBBAGE PROBLEM.

[1103]—To find the highest number that can be scored in one deal at six-card cribbage by the dealer, he having power to select all the cards, and to determine the order in which they shall be played. [Solution deferred.] H. H. H.

LETTERS RECEIVED (SUB-EDITORIAL.)

D.M.—Edward Bennis.—An Old Subscriber. Not published. Publishers afraid few readers. Thanks.—Electrician.—Hallyards.—J. D. R.—G. D. E.—R. A. Wilde.—An Admirer of Knowledge on the Whole. "Cakes and Ale," by a figure, oh sad one: not meaning necessarily either one or the other but what Shakespeare figured by them. What is it to you, however, if others like what you dislike, or to those others if you dislike what they like? But to understand that line to recommend the foolish jollity of wine-bibbing days, you must have been scarcely awake.—E. Cuthbert. 1. Sun is moving towards Hercules; rate estimated by M. Otto Struve at about three or four miles per second: probably much greater. Nature of orbit unknown. 2. Other suns moving, on various paths, as yet not connected by science. 3. Doubtless our sun will in time lose his light and heat.—Φύσις. The project seems feasible, but we have not space for its discussion here.—Juan. Clogs and clog-dances outside our knowledge.—More Light. Thanks. Spectrum of phosphorescent bodies not definite like the spectrum of a glowing gas.

Our Mathematical Column.*

ACHILLES AND THE TORTOISE.

By Richard A. Proctor.

Among the problems with which it pleased the ancients to perplex themselves was one which bears in an instructive manner on the doctrine of limits. It may be thus stated:—

The swift-footed Achilles started in pursuit of a tortoise which was 10,000 yards from him, Achilles running 100 times faster than the tortoise. Now, when Achilles had traversed the 10,000 yards, the tortoise had travelled 100 yards; when Achilles had traversed these 100 yards the tortoise had travelled one yard; when Achilles had traversed this yard the tortoise was still one hundreth part of a yard in advance; when Achilles had traversed this hundredth part of a yard the tortoise was the 10,000th part of a yard in advance; and so on *for ever*—the tortoise being at each stage in advance of Achilles by one hundredth part of the distance Achilles had traversed in the preceding stage. The tortoise then remains always in advance of Achilles by some distance however minute; and therefore Achilles can never overtake the tortoise. But we know that Achilles travelling faster than the tortoise *will* overtake it. Therefore Achilles will and will not overtake the tortoise; which is absurd.*

* The ancients were strangely fond of problems of the sort. Thus there was the famous problem about the ass between two exactly equal bundles of hay, at exactly equal distances. "This ass," says the sophist, "will attempt to eat neither bundle; for,

It will be noticed that this problem with its *solvitur ambulando* contains implicitly the very difficulty concerning the method of limits which has caused many to look with doubt on results obtained by this method—imagining that we cannot learn the exact truth by a method which causes us to approach it as near as we please. It is as true on the one hand that we may take any number of the stages considered in the problem without bringing Achilles on a strict level with the tortoise, as that if such a race were actually run the man would at the end of a definite time overtake the animal. And in like manner it is as true that in problems depending on the method of limits we do not obtain exact relations *while* applying the method, as that the result arrived at *by* the method is strictly exact. And that difficulty which in the case of the Achilles and Tortoise Problem some of the ancients chose to regard as insuperable, is the very one that troubles many modern students of mathematics, when they are told of those seemingly contradictory relations which appertain to the theory and method of limits.

I do not know that I can better introduce the doctrine of limits than by taking the above problem as an illustrative case, and showing how the method of limits applied to that problem leads to precisely the same result as the simpler method applicable in this case (and in many others which admit of being solved by the method of limits).

Suppose we take the problem, first, as one to be solved by simple algebraical considerations. We must first assign a definite velocity to Achilles and the tortoise. Suppose that the Swift-footed runs at the rate of a mile in 4 m. 24 s., or 400 yards per minute (our best professional runners covering a mile in 4 m. 20 s., and Achilles having much more than a mile to run). Then the problem would be thus dealt with by the algebraist:—

Let x = time in minutes occupied by Achilles in overtaking the tortoise. Then the space covered by Achilles in yards will be $400\,x$; and the space traversed by the tortoise will be $4\,x$. And since Achilles has in this time gained 10,000 yards on the tortoise we have as our equation for determining x

$$400\,x = 10000 + 4\,x$$
$$\text{or } 396\,x = 10000$$
$$x = \frac{10000}{396} = 25\tfrac{25}{99}$$

in which number of minutes Achilles will overtake the tortoise.

Now the method of limits would be applied somewhat on this wise. Achilles traverses the 10,000 yards in 25 minutes, and is then 100 yards behind the tortoise. He traverses the 100 yards in one 4th part of a minute, and is then 1 yard behind the tortoise. He traverses the yard in one 400th part of a minute, and is then one 100th part of a yard behind. He traverses the one 100th part of a yard in one 40,000th part of a minute, and is then the 10,000th part of a yard behind and so on continually, each stage occupying him one 100th part of the time occupied by the preceding. Hence he will overtake the tortoise in

$$\left[25 + \frac{1}{4} + \frac{1}{400} + \frac{1}{40000} + \frac{1}{4000000} + \&c. + \&c.\right] \text{ minutes} \qquad (A).$$

Now setting aside the $25\tfrac{1}{4}$ minutes for the present, let us see how the other fractions sum up, when we take two, three, four, and so on. We get

$$\frac{1}{400} + \frac{1}{40000} = \frac{101}{40000}$$
$$\frac{1}{400} + \frac{1}{40000} + \frac{1}{4000000} = \frac{10101}{4000000}$$
$$\frac{1}{400} + \frac{1}{40000} + \frac{1}{4000000} + \frac{1}{400000000} = \frac{1010101}{400000000}$$

And it will be noticed that these fractions approach more and more nearly to the value $\frac{1}{396}$. This is easily seen by taking the defect of each from $\frac{1}{396}$. Thus—

$$\frac{1}{396} - \frac{1}{400} = \frac{1}{39600}$$
$$\frac{1}{396} - \frac{101}{40000} = \frac{1}{3960000}$$
$$\frac{1}{396} - \frac{10101}{4000000} = \frac{1}{396000000}$$
$$\frac{1}{396} - \frac{1010101}{400000000} = \frac{1}{39600000000}$$

The difference it will be seen grows continually smaller and smaller, and may be made as small as we please by proceeding far enough. Hence the *infinitely* extended series (A) is equivalent to

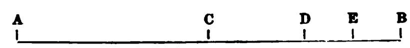
$$\left(25 + \frac{1}{4} + \frac{1}{396}\right) \text{ minutes}$$

that is to $25\tfrac{25}{99}$ minutes

the same result which we obtained by the direct method.

Now the point to be noticed here is that although the method of limits does not here actually bring us to the exact value *while we are still applying the method*, it shows us the way to that value and *not to an approximate value*.

In order the more clearly to recognise the nature of the approximation which is actually involved in the method of limits we may take a much simpler case. Suppose I have a line of two inches before me as A B in Fig. 1. Then I may suppose this line directly measured and its length to be thus ascertained. But



may also conceive that one-half of it, A C, is cut off, and then the half C D of the remainder, the half D E of what then remains, and so on *ad infinitum*. In this way I should never get to the end B, but I should get as near as I pleased to this point. The point B would obviously be the end to which this process of section would tend; and if I added together the length of all these pieces I should see that the length to which the sum continually approached was as by direct measurement a length of two inches. This *method* of determining the length would be in itself approximate; but the deduced length of two inches would be exact.

Now in both these instances we have gone out of our way (though not without a purpose) to apply the method of limits to matters which can be much better solved by a simpler method. But when the student learns that there are numerous problems—or rather an infinity of problems—which can only be solved by the method of limits, he can see the importance of establishing the *exactness* of the results obtained by the method. The method is *approximative*, but the results obtained by it are exact.

by whatever line of reasoning it could be shown that he would turn first to one bundle, by a line of reasoning precisely similar it may be shown that he would turn first to the other. But he cannot turn first to both. Therefore, he will turn to neither." Another of these problems was thus worded:—"Epimenides the Cretan says that the Cretans are liars. Now Epimenides is himself a Cretan; therefore Epimenides is a liar. Therefore the Cretans are not liars. Therefore Epimenides is not a liar. Therefore the Cretans are liars. Therefore Epimenides is a liar. Therefore, &c., *ad infinitum*." Others stated the problem in a more simple form, thus:—"When a man says *I lie*, does he lie or does he not lie? If he lies he speaks the truth, if he speaks the truth he lies." We are told that one philosopher, after vainly endeavouring to clear up this important question, flung himself, in despair, into the sea. Philosophy sustained no great loss, it may be conceived.

Our Whist Column.

By "Five of Clubs."

LATE SIGNALS.

It is well remarked by both Cavendish and Clay that if a player fails to signal at the first opportunity, his partner need not regard a signal given later as having the same authoritative character which an original signal possesses. An original signal means more than a trump lead. It means, or should mean (only some players are too ready to signal), that the signaller is not only very strong in trumps, but has such strength in other suits that (1) he can answer for the absolute safety of a trump lead, and (2) can give good promise of a great game. A signal after the first chance for signalling has passed, means much the same as a trump lead; and, whatever rule to the contrary may be set up, a trump lead does not involve the return of trumps by partner as necessary or even always proper. Very often a trump lead is tentative, and in not a few cases where it is so, the return of trumps would be bad play. So, a late signal means little more, usually, than that a lead of trumps seems likely to be advantageous.

But it occasionally happens that a late signal points to the one sole way of making the game, and should be answered at once. In fact, after the middle of a hand, a signal—if possible, which is not often—acquires a very pointed meaning. Take a case such as occurred to the editor a few evenings ago. We will call the editor *B*, his partner *A*, and players to right and left of *B*, *Y* and *Z*, as in our games. Seven rounds remained to be played, and one round of trumps (diamonds) had been already taken out, in such sort as to leave the best, 3rd, and 5th best trumps with *B*, the 2nd and

4th with Y and three trumps between Z and A, their positions unknown but one certainly with A. One trump had been forced from Y, the original trump leader. The best and third best hearts lay with Y and three small Hearts were with A, command in Clubs being with Z. B, who has not had a lead, holds, besides his three trumps, Queen, Knave, Three, and Two of Spades. As it chanced, every trick was wanted to make the game. At this juncture A led Spade King, Spades having been as yet unplayed, but (from the play) being Z's suit. Here B's course to a won game (with A's concurrence) is plain and obvious, while it is equally clear that any other course must lead to the loss of one trick at least by A, B. The one sole way of making the game is by signalling. Therefore, B dropped the Three and Two of Spades in that order to the King and Ace; A responded to the signal by a trump lead; and every trick went to A, B. On the contrary, if B had not signalled, or A had failed to respond, A would have led a small Heart which Y would have covered with the third best, and B would have been forced to ruff; for if he passed the trick, Y would have simply repeated the force. Then B could have done nothing with his command in Spades but force the enemy, uncertain whether he were forcing Z or Y; if Z, then the lead of a winning Club would again force B, and three tricks in all would be made by Y, Z; if luckily Y, then but one trick would be made by Y Z, but still their game would be saved.

Some players seem to think that if they look out for the signal in the first suit led they have done all that is required of them; but the above case and others which might easily be cited show that even towards the end of a hand the signal may be played with effect; and that therefore it should be looked for to the last. In other words, it is always worth while to attend, to the very last, to the play of the small cards.

PROBLEMS.

The two following problems were contributed several years ago by Mr. Lewis to the *Westminster Papers*:—

Simple Ending No. 1.

Y's cards. C K; H Kn; S Kn, 2; D 9.	B's cards. C A, 5; S 4; D 4, 2.
A's cards. H A, 10; S 10; D Kn, 7.	Z's cards. H K, 5; S 6; D 10, 5.

Clubs trumps; A to lead; A B to make all five tricks.

Simple Ending No. 2.

Y's cards. C 10, 7; H A, Kn; S Q, 10, 2.	B's cards. H K, 7, 4; S K, 9; D A, Q.
A's cards. C A, Kn; H 6; S 4; D 10, 9, 8.	Z's cards. C 9, 6; H 9, 8; S 8, 7, 6.

Clubs trumps; A to lead; A B to make all seven tricks.

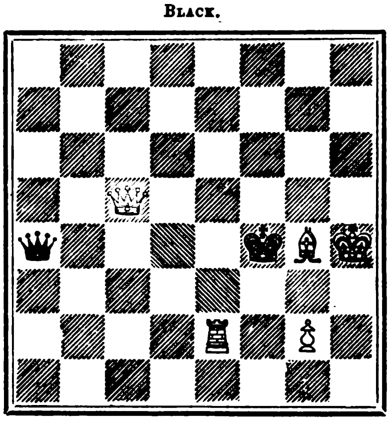
Our Chess Column.

By Mephisto.

SELECTED PROBLEMS.

By W. A. Shinkman.

Black.



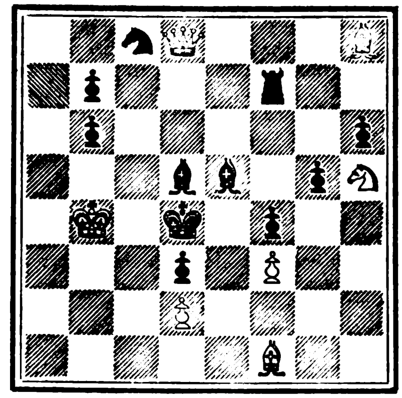
White.

White to play and self-mate in four moves.

The Index to Vol. IV., Knowledge, is now ready, price 2d.; post-free, 2½d. The Volume also is just published comprising numbers from July to December, 1883, price 7s. 6d. Office: 74 to 76, Great Queen-street, London, W.C.

By E. N. Frankenstein.

Black.

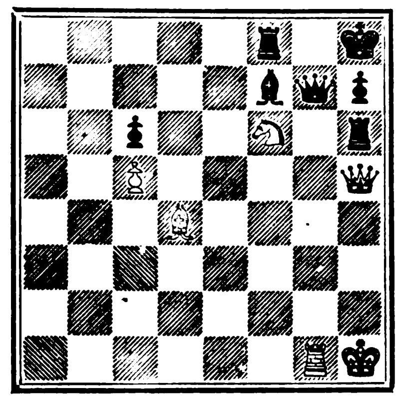


White.

White to play and mate in four moves.

By the Rev. H. Bolton.

Black.



White.

White to play and mate in seven moves.

SPECIAL NOTICES.

Volume IV., comprising the numbers published from July to December, 1883, is now ready, price 7s. 6d.; including parcels postage, 8s.
The Title-Page and Index to Vol. IV. also ready, price 2d.; post-free, 2½d.
Binding Cases for all the Volumes published are to be had, price 2s. each; including parcel postage, 2s. 3d.
Subscribers' numbers bound (including title, index, and case) for 3s. each Volume; including return journey per parcels post, 3s. 6d.
Remittances should in every case accompany parcels for binding.
Part XXVII. (January, 1884), just ready, price 10d., post-free, 1s.

MR. R. A. PROCTOR'S COURSE OF LECTURES.

1. LIFE OF WORLDS.
2. THE SUN.
3. THE MOON.
4. THE PLANETS.
5. COMETS.
6. THE STAR DEPTHS.

See Advt. Pages for full Syllabus.

The following arrangements are complete: the numbers in brackets referring to above list.

BRISTOL (Colston Hall), Feb. 19, 22, 26, 29; March 4, 7 (the full course).

CHELTENHAM (Assembly Rooms), Feb. 5, 8, 12, 15 (1, 2, 4, 6). At 3 o'clock, Feb. 5 and 12 (3, 5).

BATH (Assembly Rooms). Four Morning Lectures at 3 o'clock, Feb. 6, 9, 13, 16 (1, 3, 4, 6); two Evening, Feb. 6, 13 (2, 5).

BIRKENHEAD, March 10.

ALTRINCHAM, March 11 (5).

CHESTER, March 12, 13 (1, 2).

Note.—*All communications respecting Lectures should be addressed to Mr. John Stuart, Royal Concert Hall, St. Leonards.*

KNOWLEDGE
AN ILLUSTRATED
MAGAZINE OF SCIENCE
PLAINLY WORDED - EXACTLY DESCRIBED

LONDON: FRIDAY, FEB. 1, 1884.

CONTENTS OF No. 118.

THE *EDINBURGH REVIEW* AND THE SPENCERIAN PHILOSOPHY.

By RICHARD A. PROCTOR.

I HAVE just read the *Edinburgh Review* on Mr. Spencer's System of Philosophy, or rather on the first volume (First Principles) of the series relating to the system to which Mr. Spencer has given the appropriate name Synthetic Philosophy. I must confess to being saddened by the reading. The author of the review is one of those whose name is "writ large" in every line he pens. No one who has ever read a page from his pen can fail to recognise thereafter whatsoever he may write. A masterly advocate in a good cause, which is well though not always best in science, he here shows his power in special pleading for what I cannot but regard as a bad cause. I do not mean the cause of that philosophy for which he seems to contend, but his obvious wish to cast absurdities on (not to show absurdities in) the philosophy of one whose aims he detests. From the first lines of the review, in which he jeers at the letterpress and binding—"First Principles" inside, the "System of Philosophy" outside, and the "Synthetic Philosophy" on the back (where however "First Principles" appears in letter five times as large)—the review is unfair. The kind of ridicule cast on Mr. Spencer is such as a lover of the old Aristotelian philosophy might as effectively, nay much more effectively, have cast on Bacon's *Novum Organum*,—more effectively because in many matters Bacon was not abreast of the science of his day, and many of his suggestions were open to ridicule even in his own time, faults which cannot be urged against the teachings of the greater Bacon of our era.

To begin with, the author of the review touches on what would scarcely affect our estimate of a system of philosophy,—the language and even the grammar in which it is presented. He permits himself to say, "In quoting from Mr. Spencer, we must occasionally alter the grammar of a quotation to make it fit our own writing without abrupt changes." This, by the way, might not seem very severe criticism if the reviewer's writing were judged by the sentence immediately following, which begins thus, "No writer that we know of requires his definitions so carefully *attending* to, and the subsequent use of his defined terms so carefully *watching*." Only a few sentences before, the hypercritical reviewer (for I do not think any writer requires his writing so carefully attended to as for the moment I attend to his) speaks of something "which we should be sure to be told that we have misunderstood, or overlooked qualifying statements somewhere else,"—and I should very much like to hear his parsing of the words "which we should be sure to be told that we have overlooked qualifying statements," &c.,—[which it seems very bad grammar to me, or my last eight words good grammar, which they are not, nor which are the last four or these nine.]

If I smile at such trifles for a moment it is because the reviewer tries to give them prominence in pretending to deal with the Spencerian philosophy. He is perhaps wise in his generation. It is easier in this case to criticise style than matter; for such a subject as Mr. Spencer deals with in his "First Principles" is one in which the use of abstract language easily cavilled at is almost unavoidable.

When the *Edinburgh* reviewer attacks or rather tackles the subject matter of Mr. Spencer's volume, he still keeps clear of essentials to lay chief stress on misinterpreted words or manufactured absurdities. Among the latter the most remarkable perhaps is that, because Mr. Spencer speaks of the Unknowable First Cause as only known to us through the known effects of persistent force, therefore Mr. Spencer practically presents the Unknowable First Cause as identical with Persistent Force: whereas if there is a point on which Mr. Spencer lays special stress in dealing with THAT of which he speaks as the "First Cause, in every sense perfect, complete, total, including within itself all power and transcending all law," it is that It may be, nay must be, utterly unlike that by which we know of Its existence.

After this it is a trifle that the reviewer misrepresents Mr. Spencer in detail,—as for instance at p. 47, where he quotes professedly "a specimen of Mr. Spencer's most careful and precise style unreduced," yet omits several important words, and actually gives a concrete illustration of what he says he understands Mr. Spencer to mean, an illustration corresponding only with the garbled extract and naturally leading to an absurd abstract proposition.*

The reviewer takes Mr. Spencer to task for regarding the laws of Newton as results of experience, and points out that Newton himself established them by the "sufficient cause" argument. This is altogether new to me. In the "Principia" Newton presents these axioms as the results of experience and describes some of the experiments which establish them. In fact Newton uses the word "axiom" in its proper sense as meaning a fact or law established by experience and known to be worthy (ἄξιος) of acceptance.

From an attack such as this Mr. Spencer's work needs no defence. The reviewer says that Carlyle called something or other "clotted nonsense" and implies that the epithet may be properly applied to Mr. Spencer's philosophy. The true "clotted nonsense" story is very much at the reviewer's service, if he will condescend to accept an unadorned version. A choleric reviewer called Carlyle's

* The concrete illustration really corresponding to Mr. Spencer's abstract general statement would have been this,—If the indifferent mental idea conveyed to the mind by the aspect of a tiger leads always to the idea of physical injury and thus to the movements necessary for escape from the tiger's ferocity, it is a matter of no moment whether one idea or the other is identical with the reality or utterly unlike it. Of this it may be remarked, indeed, that it is obvious; but the very circumstance that a proposition can be laid down in abstract terms implies that any concrete example must be obvious. All the examples of the action of gravity are obvious,—but it required a Newton to deduce from them the Law of Gravitation.

"Sartor Resartus" "clotted nonsense"; and so it was no doubt to that angry writer: the *Edinburgh* reviewer, who seems to attach weight to what Carlyle wrote, would probably consider that criticism hasty and ill-judged, we may even say false and spiteful. The criticism has so far become a thing of the past that now, less than forty-five years after it was uttered, an admirer of Carlyle forgets that it was said *of* that writer and not *by* him. Perhaps in less time still our reviewer's angry abuse of Mr. Herbert Spencer may similarly have passed into oblivion.

THE EVOLUTION OF FLOWERS.

By Grant Allen.

I.—THE STARTING-POINT.

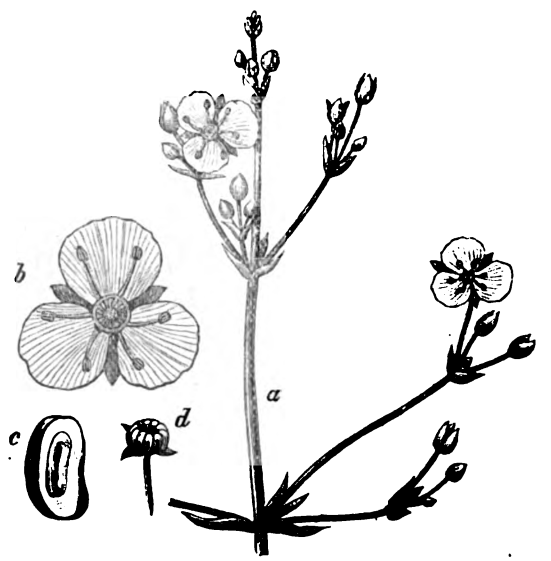
I PROPOSE in this set of papers that we should follow out together, so far as is possible, the various steps in the evolution of a single great group of plants, illustrated for the most part either by native English wild flowers, or by such common garden favourites as are within the easy reach and familiar knowledge of almost everybody. Starting from the simplest known form, which we may conceive to represent very nearly the peculiarities of the primitive ancestor, we must trace the gradual changes by which the various successively higher forms have been developed; and at each stage we must try to discover what was the advantage gained by the plant through the different new arrangements, and in consequence of what special agency these arrangements became finally stereotyped in the persons of its descendants. In this way, we shall obtain a more clear and connected view of the methods of evolution in the vegetable world than we could ever obtain by the study of mere casual isolated instances, and we shall be able more fully to understand the underlying meaning and reasons for the classifications long since half blindly (though very wisely) adopted by the earlier pre-Darwinian botanists. We shall see that the classes they mapped out are really genealogical divisions, and that all the members of each family or genus are really bound together by genuine ties of blood in their common descent from a single central and typical ancestor.

The great group of plants to which I propose to apply our present scrutiny is one that may be roughly described for unbotanical readers as that of the Lilies. Botanists will know more clearly what is meant if I say that our subject is to be the Monocotyledons, especially those with conspicuous petals or perianths, comprising the main central body of the class, from the Alismas up to the Orchids. This group may fairly enough be described throughout for popular purposes under the general name of Lilies, both because most of the flowers are moderately lily-like in form and texture, and because the true lilies occupy a central place in the class as a whole, presenting the peculiarities of the entire body in a comparatively simple and recognisable form.

What, then, is the simplest and most primitive existing type of lily, or, to speak more correctly, of Monocotyledon?

I believe, if we take relative simplicity in the arrangement of parts as our guide, we shall come to the conclusion that no lily-like plant is more primitive or antiquated in type than our own common English water-plantains. Let us begin, therefore, by looking briefly at the nature and structure of this familiar and pretty little British pond-haunter; and then let us inquire what are the marks which it still bears on its very face of its own archaic and ancient characteristics.

Everybody must often have seen and noticed the water-plantain, with its tall sparse whitish flowers rising in large, loose masses high above the stagnant surface of still pools or flooded ditches. It is a pretty, glossy-leaved plant, with long-stalked bright green blades, and a spreading panicle of starry little blossoms, which look white in the mass as you see them growing, but turn out to be delicately pink or rose-colour when you gather them for close inspection. In fact, if ever you have seen a lush and succulent water-weed, with a perfect pyramid of straggling white bloom clustered in its centre, overtopping the calm levels of a shallow English pond, you may be pretty sure that that was a water-plantain. Its botanical name (which I shall always add here for the benefit of those readers who already take an interest in structural botany) is *Alisma plantago.*



Alisma plantago.

Now, what are the reasons which induce us to begin our review of the lily tribe with this little inconspicuous English wild-flower? Well, let us premise first of all that evolution runs habitually from the simpler to the more complex; from the like to the unlike; from the less consolidated to the more consolidated. Suppose we find two flowers, one of which has five distinct petals, all alike, and the other of which, resembling it in every other way, has two of those petals specially modified into a peculiar form, we rightly conclude that the former is the more primitive and original of the two. Not necessarily that the second is directly derived from the first; about that we can only judge by means of very minute and circumstantial evidence; but that, at least, the first stands nearer in type than the second to the common ancestor from which both are presumably descended. Again, if we find one flower with five separate petals, and another just like it, only with the five petals united into a single tubular corolla, we once more rightly conclude that the former is more primitive and original than the latter. Distinctness of parts is almost always a mark of the early unconsolidated stage; coalescence of parts is almost always a mark of the later consolidated stage. For example, most simpler crustaceans have the body divided into several nearly equal and similar joints or segments; but in the crabs and lobsters, the princes among crustaceans, seven such segments have become united together to form the large head-piece with its single solid shell or carapace. In such a case, everybody can see at once that the union of

parts is an obvious sign of higher and more complete development.

If we look at the flower of the water-plantain, we shall similarly see that it presents many such symptoms of an early, uncompounded, simple type. In the technical language of botany, there is very little cohesion or adhesion among its parts: it shows us in the easiest and most separate form the ground-plan upon which all the lilies, high or low, are ultimately constructed. Only, while in the higher lilies we have to pick out the various component elements of the flower with some difficulty from their entangled and combined condition, in the water-plantain we get them all distinct and individualised, so that there need be no hesitation at all in recognising their nature and meaning.

This, in brief, is the original ground-plan of the blossom in the common ancestor from which the great lily group has ultimately descended. Its parts were all arranged in whorls of three members each. It must have had (as we know by comparison of all existing forms) first of all a protective calyx whorl of three outer green sepals, enclosing and shielding the unopened bud from all attacks of cold weather or greedy insects. Inside this must have come a second or corolla whorl of three brightly-coloured and delicate petals, intended for the attraction of its insect fertilizers. Within the petals, again, were the pollen-bearing stamens, arranged in alternate rows of three each; and of these rows there may have been one, two, three, or more; though the fact that most existing monocotyledons have six stamens apiece, or else exhibit traces of having originally had six, would seem to show that two rows were most probably the contingent possessed by the prime ancestor. Last of all, in the very centre, came the carpels or young seed-vessels, of which there were also three, six, nine, or more, according to circumstances.

To such a primitive ground-plan our existing English water-plantain very closely adheres. The little pale pink flowers that grow in loose flat bunches at the end of its branched stem are each divisible into very nearly the same divisions as the fancy flower we have here sketched out. Each of them has three small green calyx-pieces, quite separate from one another, and quite unlike the petals that adjoin them. Next it has three petals, larger and broader than the sepals, very delicate, and coloured white with a faintly roseate tinge. There are six stamens, arranged in two alternating rows of three outer and three inner, the former opposite the sepals, and the latter opposite the petals. Finally, in the centre there are a great many small, one-seeded, distinct carpels, from eighteen to thirty in number, arranged in a ring round a broad, flat receptacle, which forms the boss or axis of the whole flower.

It is to these carpels that we must most especially direct our attention at the outset, because they are, so to speak, the very patents of nobility of the Alisma family, the grand evidence that the water-plantain and its congeners do really form the most primitive existing members of the great lily group. In the first place, all the other lilies without exception (save only the Alisma family and a few closely related small orders) have the carpels more or less combined into a single compound ovary, the walls of the different carpels having coalesced, for a reason which we shall have hereafter to consider. In the second place, the number of carpels in the water-plantains is exceptionally large; and we know by the analogy of the buttercups, which are the simplest members of the other great group of flowering plants (the Dicotyledons), that primitive flowers always have a great many distinct carpels, and that with the advance towards higher types, the carpels tend to become reduced in number as well as to cohere with one another. In the third place, the water-plantains have only one seed in each carpel; and we also know by analogies elsewhere that primitive flowers always have only one seed in each carpel, but that more advanced types, while lessening the number of carpels, increase the number of seeds in each.

I know this first exposition has necessarily been a little dull, because we have here to dwell chiefly on fundamental points of structure, which are always dry, and to say very little about points of function and the practical use of parts, which are always comparatively interesting; but that could hardly be helped in an introductory sketch, where it is needful, above all things, that we should have a clear conception of the raw form from which we take our first departure. In future papers, I trust we shall be able to make the final development of the various lily-like plants from this simple original a little more graphic and a little less dull. Meanwhile, I hope my readers will try to master the first principles laid down in this opening part; as a firm grasp of the architectural plan of the water-plantain will greatly assist in following out the subsequent course of evolution on which we are about to embark.

One word more, as the preachers say, and I have done. It is a very significant fact that the water plantain and its congeners are all, without exception, aquatic plants of the marshes, ponds, and ditches. Now, it frequently happens that fresh-water animals and plants preserve for us very antique and otherwise extinct types—creatures of a sort which have become extinct in the fiercer competition of the great continents and the great oceans, but which have lingered on in the less-occupied reaches of inland rivers, lakes, or pools. It has been ingeniously noted that meres or ponds may be regarded in this respect as the aquatic analogues of oceanic islands, where so many very archaic forms have been preserved for us, far from the wild struggle for life which rages so incessantly in the wider stretches of land or water. Indeed, it may be said, roughly speaking, that almost all very early or primæval types of plants or animals yet existing belong to one or other of three peculiar habitats—islands, freshwater lakes or streams, and caves. And the one point these three habitats have in common is just this—freedom from competition save by the members of a very small and local fauna or flora.

GHOSTS AND GOBLINS.

BY R. A. PROCTOR.

THERE are few subjects more perplexing, on a close examination, than the ideas of men about the supernatural (as distinguished from the religious). Whether we analyse particular superstitions and endeavour to understand what is actually believed respecting them, or whether, taking a wider view, we consider the origin of the widespread belief in supernatural agencies, we find ourselves beset with difficulties; and these are only preliminary to the great difficulty of all—that of determining how far it is reasonable or likely that any of the common ideas about the supernatural have any basis of fact whatever.

But the first difficulty to be encountered resides in oneself. I who write have my superstitions. If I simply had them and believed in them, there would be little difficulty. But I do not believe in them. I know that they exist, because on certain occasions I have felt them in operation. Every reader of these lines must have had similar experiences—vague terrors coming we know not whence, and refusing to be exorcised by reason; the feeling —not momentary though transient—that a sight or sound

is not of this world; and other sensations conveying to us a sense of the supernatural which we can neither analyse nor understand, and in which the reason has no real belief.

Perhaps the consideration of this very difficulty may throw some light on our subject, for it often happens that the key to an enigma is indicated by the more perplexing circumstances of the problem. If we dismiss for the moment all those superstitions which may fairly be regarded as derived from early impressions, or as resulting from mere ignorance, and consider the case of well-educated, carefully-trained, and not weak-minded persons, who nevertheless at times experience superstitious tremors, we may perhaps find some circumstances pointing to the very origin of the superstitions now so widely entertained.

One well-marked feature of these emotions is their occurrence in the hours of darkness. I am not speaking here of the feeling of discomfort and fear which many experience when in the dark. This feeling is itself worth inquiring into. But I now speak of the circumstance that even those who have no unpleasant sensations when in darkness, are nevertheless only exposed to certain emotions of superstitious terror at such times. Who, for instance, thoroughly enjoys a ghost story if it is told in a well-lighted room? I use the word "enjoy," because, as a matter of fact, the sensation I am now considering is not by any means a painful one, except in extreme cases, or with persons of weak nerves. It is a mysterious, indefinable thrill, with about the same proportion of pain and pleasure as in the feeling of melancholy experienced on certain still, bright days in spring; and it is as difficult to understand why darkness and stillness should be essential to one feeling as why brightness and stillness should be essential to the other.

There is a commonplace explanation which ascribes both these feelings to the unconscious recalling of the emotions of childhood. To the child, darkness conveys the idea of discomfort. All that is enjoyable to him after darkness has come on, is in the light and warmth of the room where he sits or plays. Cold and gloom are without—in the long passages, in the unused rooms, and, in a yet greater degree, outside the house. The childish mind finds, indeed, a strange significance in the words "the outer darkness." Now, one can understand that any circumstances recalling those feelings of childhood would bring with them a thrill, relieved from pain because reason tells us no real danger is present, and conveying something of pleasure much as the idea of warmth and comfort is suggested by the roar of distant winds, or the sound of rain, when we are sitting in a cozy room. And in like manner one can understand how a bright, still day in spring may bring back "in sweet and bitter fancy" the feelings of childhood.

Yet there is more in either sensation than the mere unconscious remembrance of childhood. Something much farther back in our natures, if I may so speak, is touched, when the soul thrills with unintelligible fears. The proof of this is found in the fact that the feeling exists in childhood—nay, is more marked among children than with grown persons. "This kind of fear," says Charles Lamb, who knew better than most men what it is, "predominates in the period of sinless infancy." And I think that in the same essay he touches the real solution of the mystery, or rather he presents that higher mystery from which this one takes its origin, when he says, "these terrors are of older standing—they date beyond body."

There is a curious story in Darwin's latest work, which he uses as an illustration of a theory yet more singular. "My daughter," he says, "poured some water into a glass close to the head of a kitten, and it immediately shook its feet." "It is well known," he had before said, "that cats dislike wetting their feet, owing, it is probable, to their having aboriginally inhabited the dry country of Egypt." This explanation may not be the true one; but even if not, the real explanation we may be sure is quite as singular. Now the fact to be explained is analogous to the circumstance we are dealing with. We see in young creatures, like kittens, habits which cannot have been acquired from observation. These habits depend (almost certainly) on inherited peculiarities of the brain's conformation. May it not be that it is so with the superstitious tremors we have been considering? Those fears which affect children too young to know what fear is, those fears which in after life are but partially under the control of reason, may indicate a condition of the brain inherited not from parents or grandparents, but through long lines of descent—even, perhaps, from the ages when to our savage progenitors every unexplained sight or sound might indicate the presence of a lurking enemy. During long ages of savage life the conformation of the brain must have become permanently affected by the mental action resulting from the necessity for continual watchfulness against brute and human enemies. In the dark, particularly, such watchfulness was at once more requisite and more difficult; and it seems by no means unlikely that the anxious feelings which many experience constantly in the dark, as well as those peculiar tremors which are occasionally experienced in the hours of darkness, depend on mental peculiarities inherited from our gloom-fearing savage ancestors.

As respects the ordinary feeling of dread in darkness, although there can be no doubt that it is sometimes engendered by the talk of foolish nurses to young children (and, by the way, what an unhappy thing it is that so many must pass through the mischievous ordeal of training by foolish and ignorant persons), yet it is a mistake to suppose that this is the sole or even the main cause. Some children fear to be in darkness who have never heard of ghost or goblin. "It is not book or picture," says Lamb very justly, "or the stories of foolish servants, which create these terrors in children. They can at most but give them a direction. Dear little T. H., who of all children has been brought up with the most scrupulous exclusion of every taint of superstition—who was never allowed to hear of goblin or apparition, or scarcely to be told of bad men, or to read or hear of any distressing story—finds all this world of fear, from which he has been so rigidly excluded, *ab extrâ*, in his own 'thick-coming fancies;' and from his little midnight pillow, this nurse-child of optimism will start at shapes unborrowed of tradition, in sweats to which the reveries of the cell-damned murderer are tranquility. Gorgons and Hydras and Chimæras dire—stories of Celæno and the Harpies—may may reproduce themselves in the brain of superstition; but they were there before. They are transcripts, types—the archetypes are in us, and eternal."

Another remarkable circumstance in the superstitious impressions which affect those who have no real belief in ghosts and goblins, is the singular intensity of such impressions when aroused (in whatever way) immediately on waking. Especially after dreaming, when the dream has been of an impressive nature, the mind seems exposed to ideas of the supernatural. One often finds it impossible to understand, on waking again in full daylight, how the mind can possibly have entertained the feelings which had made night hideous or distressing. In remembrance, the matter seems like an experience of another person.

In passing it may be noticed that we perhaps owe to dreams many of the common ideas about spiritual agencies. Mr. Herbert Spencer accounts for the earliest belief in the supernatural "by man being led through dreams, shadows,

and other causes, to look at himself as a double essence, corporeal and spiritual." And "the spiritual being is supposed to exist after death, and to be powerful." Mr. Tylor also has shown how dreams may have given rise to the notion of spirits; "for savages," says Darwin (stating Tylor's views), "do not readily distinguish between subjective and objective impressions. When a savage dreams, the figures which appear before him are believed to have come from a distance, and to stand over him, or, 'the soul of the dreamer goes out on its travels, and comes home with a remembrance of what it has seen.'" "Nevertheless," says Darwin presently, "I cannot but suspect that there is a still earlier and ruder stage, when anything which manifests power or movement is thought to be endowed with some form of life, and with mental faculties analogous to our own."

(*To be continued.*)

THE EMOTIONS IN INFANTS.

THAT the infant of a few weeks old should experience emotion must doubtless seem strange to all those ignorant deriders of babyhood, who imagine that an infant and a cabbage are about on a par as regards emotional susceptibility. The apparently causeless bursts of crying so frequently indulged in by infants, would by these sceptics be put down to cold, hunger, the convenient pin, or, indeed, to anything rather than to what they would probably term a mental cause.

And yet, a very little observation shows us that infants are as much a prey to some of the emotions as are adults. To fear, in particular, do they fall easy victims, anything new in the way of a marked sensation being apt to have for its first result a certain startling effect. Darwin relates that when his child was sixty-six days old he happened to sneeze in its hearing; whereupon it started violently, frowned, looked frightened, and cried rather badly, remaining for an hour afterwards in a state which in older persons would have been called nervous. Again, when the same child was four and a-half months old, Darwin made a loud snoring noise, which at once caused it to look grave and burst out crying; and when through forgetfulness he repeated this sound a few days later, the same result as before followed.

The young mother, brought up to believe that the state of dread needs for its development some previous experience of evil, is often at a loss to understand her child's manifestation of fear, at an age when its experience of evil is necessarily almost nil. The apparent mystery, however, is easily explained by reference to the facts of heredity. The fear shown by infants is, in a great measure, the inherited effect of the manifold dangers through which the race has passed in its primitive struggle for existence. The same dangers no longer menace us, but the traces of these past experiences still survive under the altered conditions of civilised life, and manifest themselves in such transmitted tendencies as the infant's instinctive terror of darkness, its nervous shrinking from angry voices, and its dread of carnivorous beasts. Analogous phenomena prevail among the brute creation, Mr. Grant Allen assuring us that unworried calves have been known to turn and make common headway against a town-bred, half-grown puppy, which had been turned into the same enclosure with them.

The mother's non-recognition of these hereditary fears leads often to such injudicious action on her part, that a child which might grow up comparatively fearless becomes a confirmed coward. To cite one instance out of many, let us merely take the common practice of putting children to sleep by themselves in dark rooms. Courage can be established by habit, it is true; but this does not mean submitting children to conditions which must inevitably engender fear. If, as is the fact, children instinctively dread darkness, and it is wished to habituate them to it, pains should be taken that their exposure to darkness should always have an accompaniment of reassuring circumstances, such as the mother's voice or contact. The fear which darkness tends to inspire being thus instantaneously counteracted, the child's nervous system is spared those shocks which render it so liable to fresh accesses of fear, till, as a mere matter of growth, the vague terrors incidental to darkness cease to affect it.

It may be urged that it is not always possible to let the child have some one with it when it is put to bed for the night. In that case, then, let no attempt be made to inure it to darkness, but place a night-light in the room. The efficacy of this expedient is best proved by the fact that, whereas a child who wakes up alone, and in total darkness, will scream violently and be some time before it can be got off to sleep again, the child in whose room a night-light burns will sometimes scarcely cry at all, and can be hushed off to sleep directly.

After fear, perhaps jealousy and anger are the most marked emotions to which the young infant is subject. Jealousy shows itself at the age of three months, sometimes a month earlier, particularly should the mother attempt to nurse any other child than her own. Darwin's son showed jealousy when his father fondled a large doll or dandled his infant sister; while one very bright, intelligent little girl of about nine months old could not bear her mother to speak caressingly to a canary which hung in the nursery.

With regard to anger, it is not easy to say at how early an age anger is felt, for the signs of anger and distress are so alike that one may think the child is in pain when actually it is in a passion. Darwin declares that when nearly four months old there could be no doubt, from the manner in which the blood gushed to his child's face and scalp, that he easily got into a violent passion. "A small cause sufficed; thus, when a little over seven months old he screamed with rage because a lemon slipped away, and he could not seize it with his hands. When eleven months old, if a wrong plaything was given him he would push it away and beat it;" and Darwin suggests that the beating was an instinctive sign of anger, like the snapping of the jaws by a young crocodile.

Many children get into a passion over being washed and dressed, and many apparently for no reason at all that can be assigned. Their treatment, then, calls for very judicious firmness in those who have the charge of them. Often, if the parent or nurse herself is of an irascible nature, the piercing screams of the child will exasperate her to the point of slapping or shaking it with every demonstration of temper. This procedure is as mistaken as the contrary one of trying to soothe or coax it into good humour. At the same time there is wisdom in addressing an angry child in tones of grave disapproval, since, even when it is too young to understand the meaning of reproof, it is not too young to form an association between passionate outbursts and reproving intonations; and this is a first step towards the ultimate control of passion.

Among the early felt emotions we must not forget to include wonder and curiosity. "In the ordinary expression of the child," says Bain, "the distension of feature marking astonishment is very common," every little detail of the outer world seeming to produce upon it the effect, which in the adult is only occasioned by the extraordinary.

Curiosity shows itself the minute a child begins to take interest in other things besides its food; and when, though

it still carries everything to its mouth, it does so merely because the tongue is the finest, as well as the most exercised organ of touch. At this stage the child handles things, looks at them closely, pulls them to pieces, and so in playing instructs himself.

The emotion of affection is declared to rise early in life, though often it may not reveal itself in very palpable guise till the eleventh or twelfth month. Professor Ferri states that his little girl, when four months old, was rendered quite restless by her mother's absence, and that happening to catch sight of a gown her mother usually wore, she made every effort to get near it, and when it was brought within her reach, clutched at it and squeezed it with vehemence. Darwin's child at five months old plainly showed his wishes to go to his nurse. The likings of the child, in fact, are enlisted by those persons and things that procure it pleasurable sensations. Thus we find that it likes animals better than inanimate objects, because their movements, cries, colouring, and contact afford varied and intense satisfaction to its eye, ear, and sense of touch.

The consideration of the æsthetic emotions, and the means to be taken for cultivating these, must be reserved for a future article. A. M. H. B.

THE UNIVERSE OF SUNS.

By Richard A. Proctor.

(*Continued from page* 58.)

In accordance with the sound principles he had enunciated (see p. 57) Sir W. Herschel proceeded, in 1785, to develop his ideas of the universe, noting that for this purpose "it will be best to take the subject from a point of view at a considerable distance both of space and of time." I would invite the reader's special attention to this preliminary matter—absolutely omitted, so far as I am aware, from every published account of Herschel's views, and yet absolutely essential to their adequate interpretation.

"Let us suppose, then," he says, "numberless stars of various sizes, scattered over an indefinite portion of space in such a manner as to be almost equally distributed throughout the whole. The laws of attraction, which no doubt extend to the remotest regions of the fixed stars, will operate in such a manner as most probably to produce the following remarkable effects."

"*Form* I.—In the first place, since we have supposed the stars to be of various sizes, it will frequently happen that a star being considerably larger than its neighbouring ones, will attract them more than they will be attracted by others that are immediately round them; by which means they will be in turn, as it were, condensed about a centre; or, in other words, form themselves into a cluster of stars of almost a globular figure, more or less regularly so, according to the size and original distance of the surrounding stars.

"*Form* II.—The next case, which will also happen almost as frequently as the former, is where a few stars, though not superior in size to the rest, may chance to be rather nearer each other than the surrounding ones; for here also will be formed a prevailing attraction in the combined centre of gravity of them all, which will occasion the neighbouring stars to draw together, not indeed so as to form a regular or globular figure, but however in such a manner as to be condensed towards the common centre of gravity of the whole irregular cluster. And this construction admits of the utmost variety of shapes, according to the number and situation of the stars which first gave rise to the condensation of the rest.

"*Form* III.—From the composition and repeated conjunction of both the foregoing forms, a third may be derived, when many large stars, or combined small ones, are situated in long-extended, regular, or crooked rows, hooks, or branches; for they will also draw the surrounding ones, so as to produce figures of condensed stars; coarsely similar to the former which gave rise to these condensations.

"*Form* IV.—We may likewise admit of still more extensive combinations; when, at the same time that a cluster of stars is forming in one part of space, there may be another collecting in a different, but perhaps not far distant quarter, which may occasion a mutual approach towards their common centre of gravity.

"V.—In the last place, as a natural consequence of the former cases, there will be formed great cavities or vacancies by the retreat of the stars towards the various centres which attract them.

"So that upon the whole there is evidently a field of the greatest variety for the mutual and combined attractions of the heavenly bodies to exert themselves in."

Herschel then considers the objection that during the progress of the condensations he had described, there would be repeated catastrophes, caused by the shock of star against star and cluster against cluster. He shows that as regards the several clusters, "the indefinite extent of the sidereal heavens must produce a balance which will effectually secure all the great parts of the whole from approaching to each other." And as respects the several clusters, the particular stars may be saved from collision by the action of original projectile forces, which Herschel "had not intended to exclude," when, "by way of rendering the case more simple he considered the stars as originally at rest." "The admission of such forces will prove such a barrier," he remarks, "against the seeming destructive power of attraction as to secure from it all the stars belonging to a cluster, —if not for ever, at least for millions of ages. Besides," he adds, "we ought perhaps to look upon such clusters, and the destruction of now and then a star in some thousands of ages, as perhaps the very means by which the whole is preserved and renewed. These clusters may be the *laboratories* of the universe, if I may so express myself, wherein the most salutary remedies for the decay of the whole are prepared."

Herschel then considers the position of the terrestrial observer in his "own retired station, in one of the planets attending a star." He shows that to such an observer, placed in a much extended stratum or branching cluster of millions of stars, such as may fall under *Form III.* of nebulæ considered above, the following appearances will be presented:—To the naked eye "the heavens will not only be richly scattered over with brilliant constellations, but a shining zone or Milky Way will be perceived to surround the whole sphere of the heavens, owing to the combined light of those stars which are too small—that is, too remote—to be seen. Our observer's sight will be so confined, that he will imagine this single collection of stars, of which he does not even perceive the thousandth part, to be the whole contents of the heavens. Allowing him now the use of a common telescope he begins to suspect that all the milkiness of the bright path which surrounds the sphere may be owing to stars. He perceives a few clusters of them in various parts of the heavens, and finds also that there are a kind of nebulous patches; but still his views are not extended so far as to reach to the end of the stratum in which he is situated, so that he looks upon these patches as belonging to that system which to him seems to comprehend every celestial

object. He now increases his power of vision, and applying himself to a close observation, finds that the Milky Way is, indeed, no other than a collection of very small stars, He perceives that those objects which have been called nebulæ are evidently nothing but clusters of stars. He finds their number increase upon him, and when he resolves one nebula into stars he discovers ten new ones which he cannot resolve. He then forms the idea of immense strata of fixed stars, of clusters of stars, and of nebulæ*; till, going on with such interesting observations, he now perceives that all these appearances must naturally arise from the confined situation in which we are now placed. *Confined* it may justly be called, though in no less a space than what before appeared to be the whole region of the fixed stars; but which now has assumed the shape of a crookedly-branching nebula; not, indeed, one of the least, but perhaps very far from being the most considerable of those numberless clusters that enter into the construction of the heavens."

Noticing that our star-system is here described as of the *form* III., and that systems of *form* III. are made up of clusters and nebulæ of *form* I. and II., it will be inferred that by the clusters and nebulæ of the preceding paragraph Herschel signifies component parts of our stellar system. This is further shown by the sentence following almost immediately after the paragraph above cited. "It will appear," says Herschel, "that many hundreds of nebulæ *of the first and second forms*" (the italics are mine) are actually to be seen in the heavens, and their places will hereafter be pointed out. Many *of the third form* will be described,"—he actually describes ten such nebulæ, speaking of them as external Milky Ways,—"and instances of the fourth related. A few of the cavities mentioned in the fifth will be particularised, though many more have already been observed; so that upon the whole, I believe, it will be found that the foregoing theoretical view, with all its consequential appearances as seen by an eye enclosed in one of the nebulæ, is no more than a drawing from nature, wherein the features of the original have been closely copied; and I hope the resemblance will not be called a bad one, when it shall be considered how very limited must be the pencil of an inhabitant of so small and retired a portion of an indefinite system, in attempting the picture of so unbounded an extent."

* It is an important point to determine whether Herschel here means (i) the idea of immense strata,—composed of fixed stars, clusters, and nebulæ, or (ii) the idea of immense stellar strata, star-clusters, and nebulæ. The sentence will unfortunately bear either interpretation. Commentators seem usually to have regarded the second as Herschel's meaning, supposing the clusters and nebulæ to be something external to the Milky Way. Even Sir J. Herschel has adopted this interpretation. The context, as well as sequent remarks of Sir W. Herschel's show, however, that the first reading expresses his real meaning.

THE MORALITY OF HAPPINESS.

BY THOMAS FOSTER.

SELF *VERSUS* OTHERS.

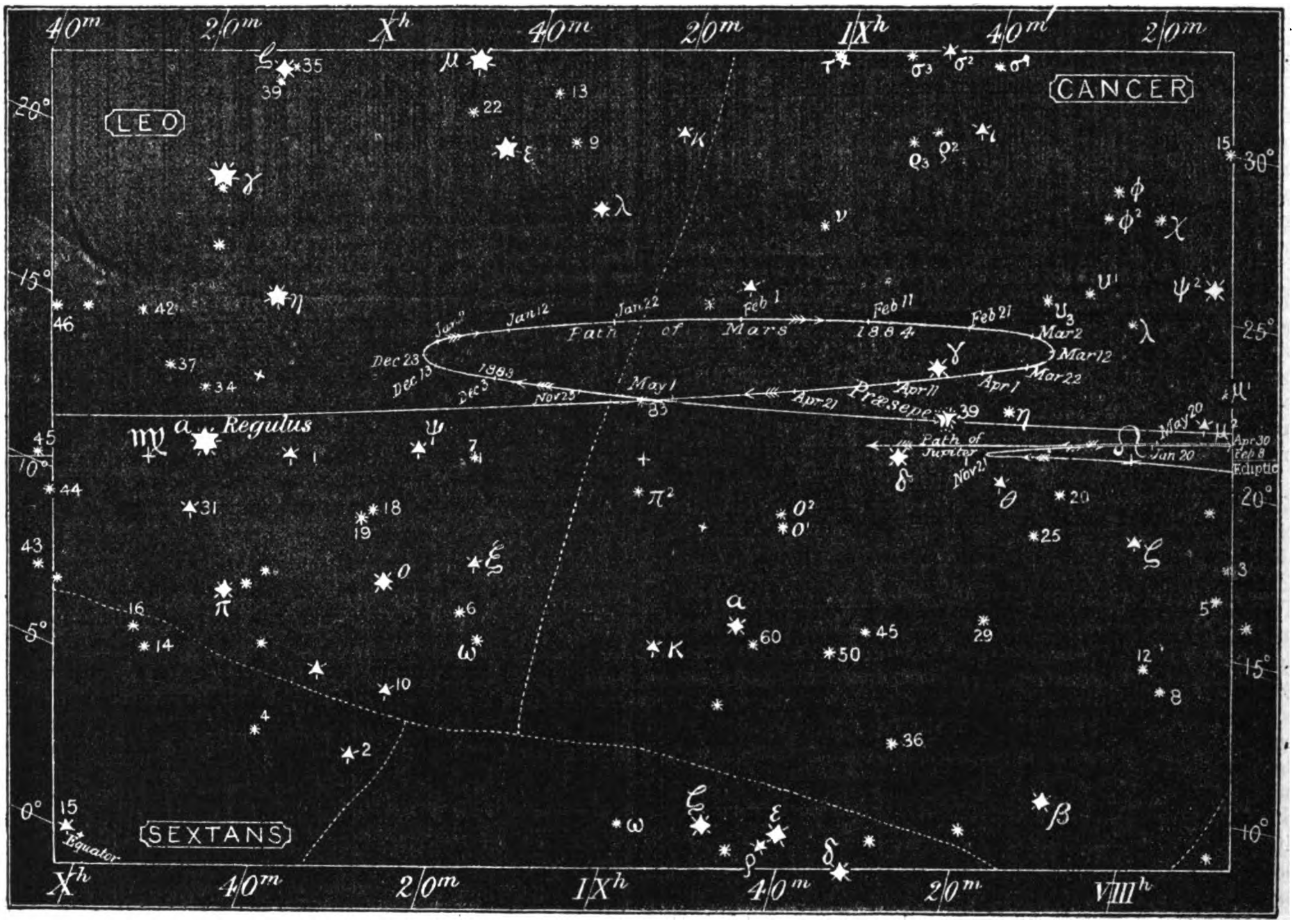
(*Continued from page 37.*)

A MAN'S power of increasing happiness depends both directly and indirectly on his fitness for the occupations of his life. Directly, because if unfit, whether through ill-health or inaptitude, he works with pain instead of pleasure, and because he gives less satisfaction or causes actual annoyance to those for whom his occupations, whatsoever they may be, are pursued. Indirectly, because as a result of work pursued under such conditions he suffers in temper and quality as a member of the body social. Hence all such care of self as is shown by attention to bodily health, by the careful culture of personal good qualities, by just apportionment of time to personal requirements, and so forth, may be regarded as of the nature of duty. In such degree as pleasure, recreation, change of scene, quiet, and the like, are necessary for the maintenance or improvement of the health, the care to secure these, so far from being held to be a concession to self, should be esteemed a most important point in "the whole duty of man."

A narrow view of duty to others may direct attention to what lies near at hand. Just as the savage consumes, to satisfy the hunger of a day, seed which should have been devoted to provide for many days in the future which lies beyond his ken, so the man who has no thought but of what lies near at hand, is apt to sacrifice health, strength, and fitness for work from which great and long-lasting benefits might have been reaped, to obtain painfully and uncomfortably much smaller results. By overwork and self-sacrifice—self-devotion if you will—a man may in a few years effect much material good to those around him,—perhaps more than in the time he could have effected by a wiser apportionment of his work and strength. But at the end of a much shorter period of work than he could have accomplished with ease and pleasantness, ere a tithe perhaps of the good he was really competent to do has been effected, his health breaks down, his strength fails him, he can no longer do the good he wanted so much to do. Nay worse, life not only becomes a burden to him but he becomes a burden to others. A wise and thoughtful care of self would have avoided this. Such care of self then, even if regarded from the point of view which should be taken by the rest, is simply far-sighted regard for others.

Perhaps the simplest way of testing the matter is by considering what would happen if all or many of the members of a community followed a course which is commonly spoken of as if it were meritorious. It is manifest that a community chiefly composed of persons who neglecting self broke down their health and strength in exhausting efforts to advance the well-being of others would be a community constantly burdened by fresh accessions of worn out and used up members,—including eventually most of those who had been most anxious to serve their fellows.

But the question becomes still more serious when the known facts of heredity are taken into account. The evil effects of self-neglect, whether in the form of overwork, or asceticism, or avoidance of all such pleasurable emotions as lighten the toils and worries of life, or in other ways, affect posterity as well as the individual life. Ill-health and weakness are transmitted to children and to children's children through many generations. It is not going too far to say that on the average more misery is wrought and to a much greater number by neglect of self than can be matched by any amount of benefit conferred during life, still less by such benefit as directly arises from self-sacrifice. A man shall work day after day beyond his strength for ten years, and by such excess of activity shall perhaps accumulate at the expense of a ruined constitution what may confer a certain amount of happiness on several persons, or keep discomfort from them. Probably with better advised efforts during that time more real good might have been conferred on those same persons, for man does not live by bread alone; and certainly in the long run even of a single ordinary life much more good may be done by combining zeal for others with due regard for the welfare of self. But when we consider the multiplied misery inherited by the offspring of weak, sickly, and



The Zodiacal Sign for February, with the paths of Mars and Jupiter in 1884.

gloomy parents, we see that even though on the whole there had been during life a balance in favour of happiness conferred, this—more than outweighed even in the first generation—would be many hundred times outweighed in the long run.

(*To be continued.*)

*** We feel each week more and more the pressure on our space, and are at length tempted to yield to those who have begged us to increase our size to 32 pages, or our matter by more than one half, and our price to threepence. Numbers of subjects and illustrations wait for want of room—and much time is occupied each week in cutting and contriving for space; while yet each week we run the risk of offending valued contributors and correspondents by keeping out or keeping back matter of great interest and value. We should be glad to be able to form an idea as to the views of readers on this point. It is idle, of course, to suggest (as many who take no interest in special subjects have done) that this or that subject should be omitted: on this point we have gauged the *average* opinion of our readers, which must guide us in the matter.

The Index to Vol. IV., Knowledge, is now ready, price 2d.; post-free, 2½d. The Volume also is just published comprising numbers from July to December, 1883, price 7s. 6d. Office: 74 to 76, Great Queen-street, London, W.C.

Fourpence each will be paid for copies of No. 33, Knowledge. —Apply or address, Publishers, 75, Great Queen-street, London, W.C.

THE NIGHT SIGN FOR FEBRUARY.

We give this week that sign of the zodiac, to wit Leo, which is now dominant in the midst of night, and the paths therein of Mars, (from November last to Midsummer) and Jupiter. The constellations now occupying this sign where once Leo reigned supreme are Cancer and the fore-part of the Lion. Mars just now is not far from the position referred to by Tennyson (we mean the Poet Laureate not the new law-maker) so poetically in Maud. We would recommend the use of a tint of red over the track of Mars, and of blue over the track of Jupiter, so that they will interfere less with the star-groupings. Names, letters, &c., can be similarly tinted with advantage. Next week we shall give the Day Sign, namely Aquarius, (the constellation Capricornus) and show the sun's path from day to day along that sign.

MARS IN OPPOSITION.

By Richard A. Proctor.

The Night-sky Zodiacal Map given above shows the loop traced out by Mars during the opposition of 1884, that is during that part of his motion around his orbit when he and the sun are on opposite sides of the earth.

Fig. 1 illustrates exactly to scale the motion of the earth and Mars along the parts of their respective paths

which they traverse from February 1st to May 11th. The positions of the two planets are shown at intervals of ten days, from February 1st the day of opposition (erroneously

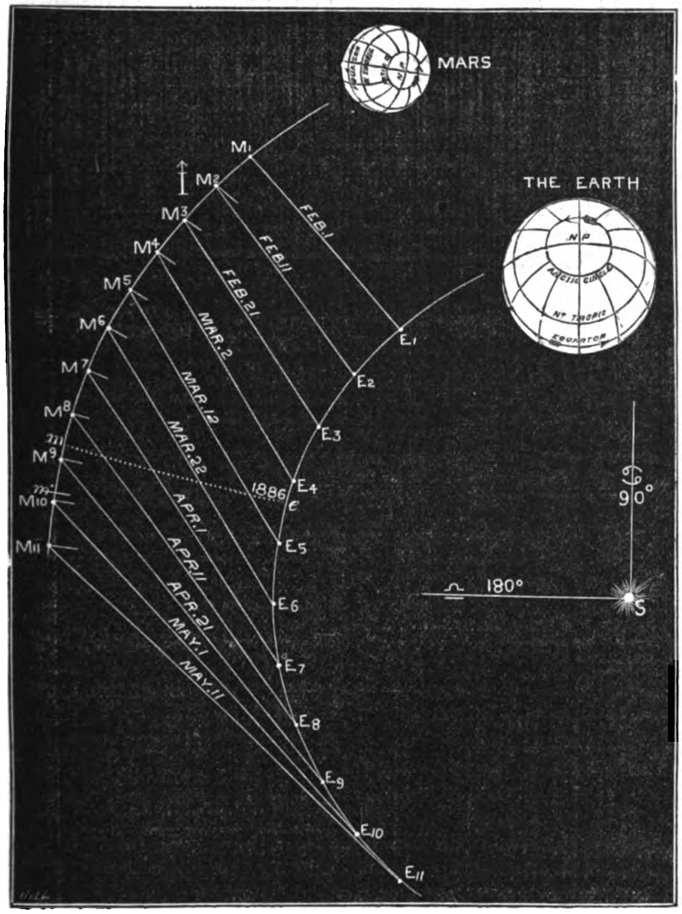


Fig. 1.

given January 31 in some almanacs*), to May 11. The planet as viewed from the earth appears stationary on March 13 (7 a.m.) or more correctly ceases then to retrograde, and begins anon to advance in the direction of the Zodiacal signs. The near parallelism of the lines connecting the planet on March 2, March 12, and March 22 shows why the planet seems no longer to retrograde. On April 27th the planet is seen again in nearly the same direction as on February 1st. Compare chart (Fig. 1), and also the looped paths of Mars (with corresponding dates) pictured at p. 8. The loop in the Zodiacal chart is in fact a perspective view of the loop shown on p. 8.

The projections of Mars and the earth in Fig. 1 are on a somewhat larger scale than the orbits,—in fact *exactly five thousand times* larger. The orbits are on a scale of fifty millions of miles to the inch; but the projections of the globes of Mars and the earth are on a scale of only 10,000 miles to the inch. On the same scale the sun would be nearly five yards in diameter; but I thought it would be inconvenient to show him on that scale in these columns.

Where the little arrow-headed line is shown Mars attains his greatest distance north of the ecliptic; at *m* he is in aphelion, at *m'* occurs the midsummer of his northern hemisphere.

The general reader but still more the student of Mars with the telescope may be interested by the following simple method for ascertaining the exact position of the polar axis and equator of Mars (as observed from the earth) when the right ascension and declination are known (these are given at convenient intervals in Whitaker's Almanac).*

* In Astronomical time Jan. 31, 23 h., which in civil time means Feb. 1, 11 a.m.

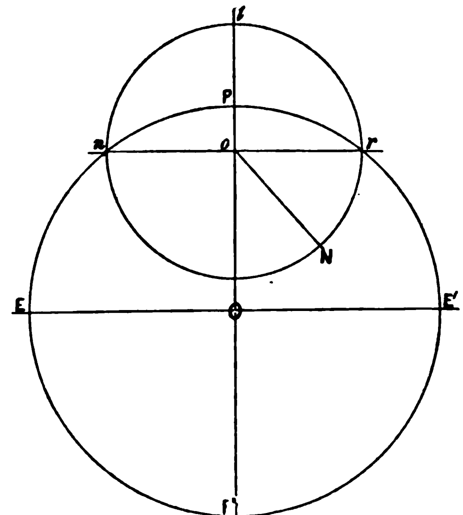


Fig. 2.

First make the construction in Fig. 2,—good for every case during the next half century or so. It explains itself:—P *r*, P *n*, each = 39²° (=I of the formulæ); *r* N = 47½° (=N of the formulæ).

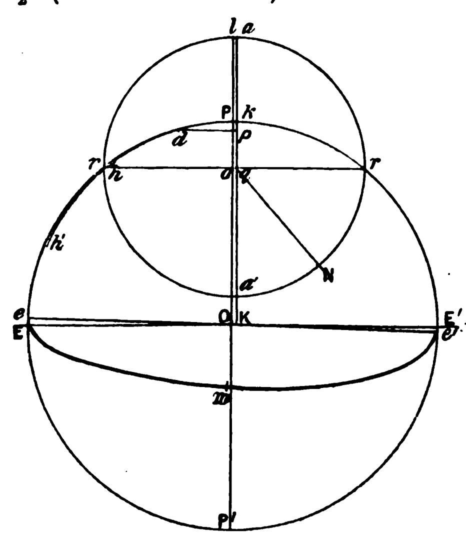


Fig. 3.

Suppose you want the axial aspect of Mars for Feb. 1.

Take (with a protractor) arc N *a* (Fig. 3) = Mars's right ascension 136° (9h. 4m.), and draw *a q* K, sq. to *n q r*, O E O'; describe arc *k h* about centre K to meet *n r* in *h*. Take with a protractor arc *h d* = Mars's north declination 21° 32'; and draw *d p*, sq. to *k* K.

* The demonstration of the method of construction above given will be found somewhere in the *Monthly Notices of the Astronomical Society*, to which I sent it in the long past time when I was weak enough to waste time that way. I remember Herr Marth who still yields to a similar foible, saying to me after I had read my paper, "My dear Mr. Proctor, I tell them what is the position of the axis of Mars from day to day and *they won't use that;* do you suppose they will be at the pains to make a construction to find out for themselves what they don't care for when it is given them?" I wonder whether he was right.

Then p is the position of the north pole of Mars on his face P E P′ E′ (where P O P′ represents the meridian), and $d\,p$=O m, the apparent distance of the middle point of Mars's equator from the centre of his disc.

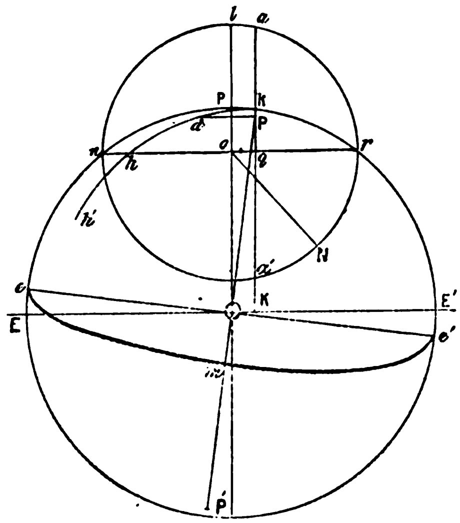


Fig. 4.

Fig. 4 gives the corresponding construction for March 13 when Mars reaches his stationary point. From Fig. 1 it will be inferred that the equator is at this time a little less open. About the end of April the planet's pose is nearly the same as on Feb. 1. A few days later, or more exactly on May 10, the planet's polar axis coincides with the meridian, and after that the visible or north pole passes to the left or east of the meridian.

In Fig. 5, the position of Mars as shown in an inverting telescope on February 1 is presented. To make such a construction after the position of the visible pole and the opening-out of the equator have been determined is easy enough. But presently in carrying out my plan for a series of mapping papers I shall give simple instructions for making a projection of this kind. Fig. 5 will serve for any time during the present opposition; for, as will be seen by

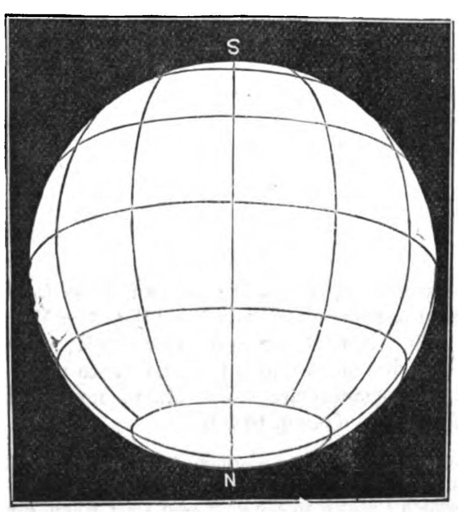


Fig. 5.

comparing Figs. 3 and 4, the opening of the Martian equator varies little during the next few months. The slant to the meridian attains it maximum, as shown in Fig. 4, about March 13.

I may remark that the above construction, including the constant part shown in Fig. 2, applies at all times; but it is to be noted that if the point a falls on n N r, the north pole lies on the invisible half of the planet. For instance, if a came as at a' in Figs. 3 and 4, the north pole would lie somewhere on $k\,g$, but on the concavity instead of the convexity of the sphere, P E P′ E′. Thus if the construction gave the point p, then p' such that O p'=O p would be the position of the visible pole.

Declinations must be measured from h towards k in every case, whether northerly or southerly.

In the next few weeks, I propose, if I can possibly get space, to give some pictures and charts of Mars, illustrating points of interest in this miniature of our earth.

TRYING TRICYCLES.

By John Browning,

Chairman of the London Tricycle Club.

THOUGH I wrote my title for this article without a thought of jesting, I fear my readers will think me guilty of attempting a pun before they have read many lines further. During the last three years I have tried a great many rear-steering tricycles, because I considered that they were being unduly neglected in favour of front-steerers—an opinion which the success of the "Rover" has confirmed.

On one of the most original of these rear-steerers, I started on a fine evening in June, running down a steep hill from Upper Norwood towards Anerley. Before long my progress was checked by traffic; and, as the machine was provided with ratchets, so that I could not back-pedal, I was compelled to apply the break, which was of the spoon variety, and acted on the tyres of both wheels. The instant I did so the machine turned round at right angles and ran across the road full tilt, face forward, into a brick wall. The wall was only very little injured, and the machine did not suffer much by this deviation of ours from the straight way, but I could not forget the circumstance for a long while myself, though I wished to. I had several reasons for recollecting it.

Now the cause of this accident was that one of the spoon-brakes skidded the wheel to which it was applied much stronger than the other; that wheel was checked, but the other ran on, and so turned the machine half-round, with the result I have described.

After having the spoons altered so that they bit the wheels equally, I went out on one of our club runs with the machine.

During the outward journey, in daylight, it behaved tolerably well, only betraying occasionally a fondness for kerbstones, ditches (dry or wet), posts, and cottage door-steps, which was trying, and kept the rider in a state of pleasurable excitement as to where he should find himself the next minute.

This uncertain behaviour was caused by the back-bone of the machine being too short and the hind steering-wheel too small, so that it did not answer the helm readily.

But if on the outward journey the machine evinced some desire to run away with its rider, when homeward bound it made handsome compensation by jibbing, going occasionally by fits and starts, and sometimes refusing to travel at all. I became the rear-guard of the run, and as the riders of the London Club are far too gentlemanly to run away from a member in difficulties, the return journey was trying to them as well as to myself.

The cause of this misbehaviour was that the nuts which

clasp the ball-bearings were too loose, and being screwed on from the under side, instead of the upper, as they should have been, permitted the opening of the journals, and this escaped observation in the dusk. The balls ran up on to the sides of the holes in the cages and jammed themselves, and then fell back with each hard jerk—or, as it is called, "kick"—on the pedal, and so allowed the machine to progress spasmodically.

The next day I sent the machine back to the maker, and he shortly returned me another.

Finding the faults in the former machine had been amended, I started out on the new mount with some confidence for a ride through Sydenham and Beckenham and on to Keston and Downe, till lately the dwelling-place of Darwin.

The machine seemed to run very well until, on descending West Hill, Sydenham, finding my progress impeded by the traffic, I attempted to apply the brake, which was certainly right when I started. To my consternation, I found it had no action on the machine. I was running at the rate of ten miles an hour, and I had a steep descent of full a quarter of a mile in front of me, with many vehicles in the road, and I was without any power of back-pedalling. There was no time for hesitation. I clapped the palms of my hands on to the rubber tires of the wheels, and, steering and checking the machine by these means, I brought myself up safely in a few minutes. As the skin was off my hands before I could stop, and my hands were bleeding, those few moments *were* trying.

Now, the cause of the machine running away in this instance was very simple. The break lever was secured to the rod to which the two spoons were attached by a quick tapered pin. This pin, with a thoughtlessness almost criminal, had been put into its place with the large end downwards, the jarring of the machine had loosened the pin, and it had fallen out on the road.

Had the pin been fitted with the largest end upwards this mishap would not have been possible.

Brake accidents should be provided against most carefully, so I will relate two others which befell me.

I was trying a new small front-steerer for the first time, and had more than the usual amount of difficulty in adjusting the seat to the right height. Having got the seat right at last, I pedalled away merrily, and presently turned over the brow of the short, steep hill known as the Robin Hood Hill. Seeing a furniture-van drawn right across the hill, I attempted to apply my brake, but found it had no effect.

I had the power of back-pedalling, but at the pace I was running on such a steep descent it would not have been possible for me to have stopped before I reached the furniture-van. In an instant I recollected that being on a very small-wheeled machine I could make my heels foul the axle, *if I wished to,* so I slipped my feet back until the points of my toes fell into the pedals, and my heels jammed against the axle at each revolution. In three or four revolutions I brought the machine up so that I was able to turn it round on the hill-side, and I ran it against the stone kerb, and stopped it just before it reached the van. I was naturally anxious not to damage the van if I could possibly avoid it.

I had to rest a little while before I proceeded on my way, for the time my toes were pinched in the pedals and my heels bruised by the axle had been trying.

How came this trouble to happen? Well, in this way:—Every time the height of the seat was altered the brake required re-adjusting. For the first time or two I recollected this, but on the last occasion I must have forgotten it, and started with my band-brake loose.

I have never forgotten to adjust it since.

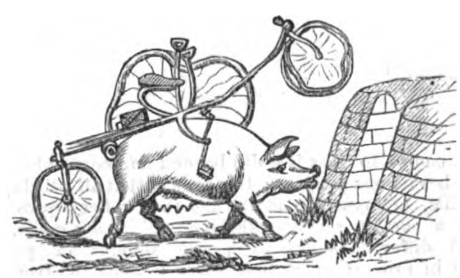
The last brake accident I have to recount befell me in this wise:—

I had got a new, small, extra light front-steering machine, and on the first attempt to bring it up by means of the break, when running down a slight incline I thought it inefficient. Intent only on testing the action of the break I got up speed a bit on the level, and then by back-pedalling reduced it to certainly less than six miles an hour. Having done this, I applied the break sharply, and in an instant I was shot over the handles. That break was perfect—no doubt about it!

I managed to remount and stop the machine twice, but I was more battered than I thought for.

My lip and tongue were cut through, and both my kneecaps nearly cracked. I had to live on slops for one week, and drag about by the aid of a stick for another. Flights of stairs, which are generally a delight to me, became a torture. The whole of that fortnight was trying. I can trust my readers to draw the moral of this mistake for themselves.

The last account of testing a tricycle which I shall relate now happened to my friend Mr. Salmon, who was riding his celebrated small machine, the "Shadow," made by Hirst, of Croydon, which weighs only 46 lb., down to Brighton to attend the council meeting of the Tricycle Union. Having to stop to meet some members of the Brighton Cyclists' Club, who were riding out to meet him, he put up at an inn about nine miles from Brighton. For safety he put his small tricycle into a narrow passage, which he selected as a *cul-de-sac.* But there was a pigstye at the end of that passage, and the old sow, requiring to go into her home shortly afterwards, found the machine in her way. She tried to get under it, and as she did so the teeth of the rat-trap racing pedals stuck into her sides, which both frightened and hurt her, so she backed out of the passage, carrying the tricycle aloft, and dashed about the inn-yard, until she was relieved of it, by which time it was a perfect wreck, as depicted.



The engraving, of what might be termed the trial of Sow *versus* Shadow, is from a sketch made by Mr. Schultz, a member of the Brighton Cycling Club, who was an eyewitness of the whole of the proceedings, and assisted at the rescue of the wreck of the "Shadow."

I will conclude with the earnest wish that my readers may not have found this article too trying.

MESSRS. STARLEY & SUTTON ask us to state that they have registered the name and design of their new tricycle, the "Rover."

UNDERGROUND WIRES IN FRANCE.—According to a statement just prepared, it appears that the French Government have determined, in addition to their extensive system of underground telegraph wires recently laid, to lay a further length of 7,296 kilomètres (rather over 4,530 miles) of cables, the cost of which is estimated at 54,000,000 francs, or £2,160,000.

Reviews.

OPTICS WITHOUT MATHEMATICS.*

THIS work contrasts rather strangely with Sir Edmund Beckett's justly celebrated work "Astronomy without Mathematics." The title of the latter work is somewhat deceptive; readers expect an account of matters astronomical which shall be easily read and followed: they find a treatise which needs very close attention and cannot all (we imagine) be understood without much of that faculty which is required in mathematical studies, even if much acquired mathematical knowledge is not required. In Mr. Webb's work many would expect to find the laws of optics explained without mathematics; but this is not done,—for a very good reason; it is not possible. Mr. Webb describes and explains his descriptions; but he does not explain what he describes. It is however of far more importance to have—as here—correct and simple descriptions, than to have, as in so many treatises on optics, inexact yet complex attempts at explanation. Here and there Mr. Webb's account is open to a little question, and some of the illustrations certainly require improvement. For instance, the illustration of the rainbow at p. 89 must prove perplexing to the learner, the sun being put almost vertically over the observer's eye, and the angle between the right lines to the rainbow and those to the sun being far too small.

As an illustration equally of what Mr. Webb does and of what he (wisely) leaves unattempted, we give his account of polarisation:—

Polarisation,—Much of the light around us is more or less polarised without our being aware of it. The name seems to have been taken from magnetism or electricity; but it is not particularly suitable, and it is better not to think about poles, but to try to suppose, that is for our present purpose, that every ray of natural light consists of undulations crossing each other in every possible direction, and that when polarised they are all turned in two directions at right angles, something in this way:—

How this comes to pass is quite beyond our comprehension, and fortunately beside our purpose; but so it is that all polished bodies, except metals, polarise the light that they reflect, provided it is incident at a certain angle, called the *polarising angle*, which is different in different substances; the amount of polarisation diminishing in proportion as that angle is departed from: and so again all doubly refracting crystals polarise their two rays, but always in planes at right angles to each other, the *plane of polarisation* being that which passes through the original and the polarised ray. Now, this being understood, that in certain cases, both of reflection and refraction, all the undulations can be turned two ways at right angles, it also happens, you will not ask me how, that there is something corresponding in the structure of many bodies that will only receive them in those ways: so that when polarised light falls on such surfaces it will either pass freely, or be stopped and extinguished, according as the direction of its undulations agrees or disagrees with the optical structure of the body,† the extinction being most complete when these are at right angles, and more or less partial in intermediate positions. There are several contrivances to show this very curious effect, called *Polariscopes;* the most simple is the *Tourmaline,* a doubly refracting crystal found chiefly in Ceylon, which would be best of all but for its being coloured. If two thin slices of this, cut with reference to its optical property, are so set close together that one may be turned round in front of the other, all the light will pass through if their optical structure lies the same way, or all be quenched if the second is turned so as to cross the first at right angles; a partial extinction taking place at intermediate positions. This is the case of ordinary light, which gets polarised in passing through the first plate, and then transmitted or stopped by the agreeing or disagreeing polarising power of the second: of course, if rays have been already polarised elsewhere, one slice of tourmaline that can be turned round will answer the same purpose. In such experiments we call the first crystal or surface the *polariser*, the second the *analyser*. The use of an analyser will show how much polarised light surrounds us; if we only get it at the right angle we shall find polarised light reflected by glass, water, polished furniture, varnished pictures, shining leaves, and many other things, but not metals, for a reason that it would be very difficult for us to understand. Many media have the power of changing or "rotating" the plane of polarisation; and thus differences are sometimes shown in their nature that would not otherwise be detected. An instrument called a "saccharometer" is used to ascertain the genuineness of sugar; the juice of the cane and that of the grape turning the plane in opposite directions. A more surprising and mysterious fact was discovered by Faraday—that the plane of polarisation is shifted by the influence of a powerful magnet.

* By the Rev. Thos. W. Webb, M.A. Published by the Society for Promoting Christian Knowledge. (London and New York.)

† We must always bear in mind that the direction of a ray, as it passes onward in its course, is quite a distinct thing from the direction of the undulations which form light, and are always at right angles to the path of the ray.

NOTES ON BOOKS.

County Atlas, of England and Wales, with all the Railways and Coach Roads (J. Heywood, Manchester and London). A splendid shillingsworth, each county in its alphabetical place, so that one can turn at once to the county one wants. A most convenient travelling companion.—Chemical Analysis, by *A. H. Scott-White.* A useful little treatise, serving as a convenient text-book for examinations in which a practical knowledge of chemical analysis is required.—History Readers (Moffatt & Paige, London). Our young folks are still taught history too much in the style which of old was thought the only orthodox one; but in the series of historical readings published by Messrs. Moffatt & Paige we see the influence of those historians who in recent times have tried to show that history means, or should mean, something more than a record of rulers, piratical or otherwise, who obtained dominion or territory by means more or less nefarious; and that even battles, sieges, murders, and the like, are not *very* much more important than the progress of art, literature, commerce, and science. The more fanciful illustrations in these books, such as Boadicea addressing the Britons (a young lady of the nineteenth century who by some accident comes to have a spear in her left hand), King Alfred in his study, and so forth, would have been better omitted; they are untruthful; but the pictures of places, costume, &c., are useful and good.—First Lessons on Health, by *J. Berners* (Macmillan & Co., London). A useful series of simple lessons in sanitary matters, given originally to a girls' class in a national school. Light in style and tone, but most important in substance.—Solar Physics and Earthquake Commotions applied to elucidate Locust Multiplication and Emigration, by *A. H. Swinton.* The Solar Physics Endowment folk should reward the author of this pamphlet. It is to be followed, we imagine, by a treatise showing, How, from the observed length of a ship at sea and the number of her masts, to infer the name of the captain and whether the second mate has had the measles.—Whence? What? Where? by *Dr. Jas. R. Nichols* (Trübner & Co., London). A bright and suggestive discussion of the origin, nature, and destiny of the human race; but the best answer to our author's title-questions, seems to us to be *"Here, Thus, Now."*—Bookkeeping no Mystery, by *An Experienced Bookkeeper* (Crosby, Lockwood, & Co., London). Bookkeeping as it was previously taught seemed like a rather involved sort

of algebra, with rules technically worded and made elaborately mysterious. In this little book bookkeeping is made interesting — almost entertaining. It is a really useful work.—COPY-BOOKS. An excellent and very cheap series published by Moffatt & Paige, London. — COMIC POETRY, English and American (W. Kent & Co., London). A miscellaneous collection of humorous poems, pleasing as combining the modern fun of the "Bab Ballads" and Bret Harte with Tom Hood (earlier poems), Cowper, Peter Pindar, Oliver Goldsmith, and Prior; and be it noticed that Prior's English sounds as modern as Mr. Gilbert's,—all but the rhyme "creature" and "nature," which reminds us how our Irish cousins got their pronunciation. Many will be pleased to find here three of Bayley's songs, but Bayley was not quite so funny as the last generation thought him when they sang his songs,—his words want music with them. "Out" even approaches stupidity in parts; but how funny it sounded as a song!—ELEMENTARY CONIC SECTIONS. Part I. By *H. G. Willis M.A.* (Bell & Co., London and Cambridge). A useful treatise on the conic sections treated geometrically, and including well-arranged sections on Harmonic Ratio, Poles and Polars, Reciprocation and Projection, conical and orthogonal. We are particularly pleased with the way in which the corresponding properties of the parabola, ellipse, and hyperbola are treated together, not left to be associated by the student while he is dealing with the curves separately.—MAGNETIC SURVEY OF NORTH-WEST CANADA. By *Lieut.* (now *General Sir J. H.*) *Lefroy* (Longmans & Co., London). This diary of the magnetic survey of the north-western portion of the region of Canada is a work of great value and importance. It indicates features of singular interest which are not found in Sir Edward Sabine's Contributions to Terrestrial Magnetism. The work is of special value, because it must be from observations in this very region that the movements of the northern magnetic pole will for the next century or so be chiefly determined. After that time the north-eastern parts of the Asiatic continent will be the chief region of observation for the purpose. Every one who desires to understand the data of the complicated problem of terrestrial magnetism, should master the evidence gathered together in this important contribution to the work of magnetic survey. — NATURAL PHILOSOPHY. Translated from *M. Ganot's Cours Élémentaire de Physique*, by *Prof. Atkinson* (5th Edition. Longmans & Co.: London.) This may be regarded as an abridgment of Prof. Atkinson's translation of *Ganot's Éléments de Physique.* It is the most trustworthy and withal the most interesting work of the kind known to us. Written by one well versed in the subjects dealt with, and translated ably and thoughtfully by a skilful physicist, it is free throughout from such blunders as disfigure Guillemin's "Forces of Nature," and other as pretentious volumes.

THE WORLD'S PRODUCTION OF IRON AND STEEL.—In 1876, the total amount of pig-iron produced was about 14 million tons; in 1882 it was over 20 million tons, being an increase of 43 per cent. in six years. The production of steel in 1877 was under $2\frac{1}{2}$ million tons; in 1882 it was over 6 million tons, showing an increase of 150 per cent. in five years. In 1876, the production of coal was about 280 million tons; in 1882 it had risen to about 375 millions, an increase of 34 per cent. in six years. In 1882, Great Britain produced 8,493,237 tons of pig-iron, 2,259,649 tons of steel, and 158,499,977 tons of coal; while the United States turned out 4,623,323 tons of pig-iron, 1,736,692 tons of steel, and about 83,500,000 tons of coal, but the comparative increase in American production has been much more rapid than that of British.

THE FACE OF THE SKY.

FROM FEBRUARY 1 TO FEBRUARY 15.

BY F.R.A.S.

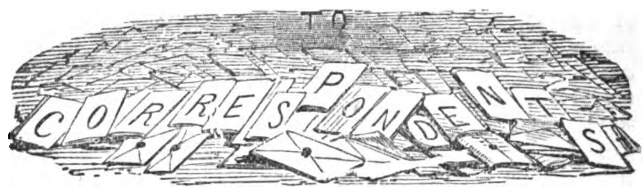
ATTAINING now daily a greater meridian altitude, and being, *pro tanto*, more favourably situated for the observer, the Sun should be examined whenever he is visible for spots and faculæ. Map II. of "The Stars in their Seasons" gives the present aspect of the night sky. Minima of Algol (Map I.) will occur on Feb. 2, 9 min. after midnight; on the 5th at 8h. 58m. p.m.; and at 5h. 47m. on the evening of the 8th. Mercury is very badly placed for the observer, being only visible, if at all, before sunrise. Venus is an evening star, and is daily coming into a better position for the observer. She is a striking object over the west-south-western part of the horizon after sunset. In the telescope she is still gibbous. The meaning of this word has been previously explained in these notes. As Mars comes into opposition to the sun at 11 o'clock to-night he is as well placed for the observer as he will be for a long time to come; and although his angular diameter (16″·6) falls lamentably short of that which he presented during the famous opposition of 1877, he is still a very remarkable and interesting telescopic object; all the superficial details to which we referred a fortnight ago (p. 46) being well marked with adequate optical power. He travels backward in Cancer from a point some 1° south of ξ in that constellation (Map III. of "The Stars in their Seasons"). Jupiter is still the most conspicuous object in the sky, where he may be observed all night long. He, like Mars, is travelling backwards in Cancer, into the blank region between Cancer and Gemini (Zodiacal Map, p. 40). The phenomena exhibited by his satellites before 1 a.m. are numerous and interesting during the next fortnight. This evening (the 1st) Satellite I. will be occulted at 5h. 40m., and reappear from eclipse on the opposide side of the planet at 8h. 16m. 4s. p.m. Satellite III. will be occulted at 10h. 58m. p.m. on the 2nd, but will not reappear from eclipse until nearly 4 o'clock the next morning. An occultation of Satellite II. will occur at 10h. 10m. p.m. on the 3rd. Its reappearance from eclipse at 1h. 46m. 28s. a.m. on the 4th. On the 5th, Satellite II. will pass off Jupiter's face at 7h. 8m. p.m., as will its shadow later, at 7h. 58m. The egress of the shadow of Satellite III. will occur at 6 o'clock in the evening of the 6th, and Satellite I. be occulted 58 minutes after midnight. On the 7th, Satellite I. will begin its transit over the planet's face at 10h. 17m. p.m.; followed by its shadow at 10h. 45m. The satellite will pass off at 12h. 37m. p.m.; its shadow at 1h. 5m. a.m. on the 8th. On the evening of the 8th, the shadow of Satellite IV. will enter on to Jupiter's limb (the satellite casting it having left Jupiter's opposite limb four minutes previously). Then Satellite I. will be occulted at 7h. 24m.; to re-appear from eclipse at 10h. 10m. 42s. p.m.; while at 10h. 21m. the shadow of Satellite IV. will quit Jupiter's disc. On the 9th, Satellite I. will pass off the planet's face at 7h. 3m. p.m.; as will its shadow at 7h. 34m. Satellite II. will be occulted 26m. after midnight on the 10th. On the 12th, the transit of Satellite I. will begin at 6h. 31m. p.m., as will that of its shadow at 7h. 40m. The satellite will leave Jupiter's opposite limb at 9h. 25m.; and its shadow at 10h. 34m. p.m. At 6h. 25m. p.m., on the 13th, the shadow of Satellite III. will begin its transit; Satellite III. itself passing off the face of the planet at 7h. 35m. The shadow will quit Jupiter's limb at 10h. p.m. On the 14th, Satellite I. will begin its transit 2m. after midnight, as will its shadow 38m. afterwards. Lastly, on the 15th, Satellite I. will be occulted at 9h. 9m. p.m.; to re-appear from eclipse at 12h. 5m. 28s. p.m. Saturn should now be looked at as soon after dark as convenient, as he souths about twenty minutes past seven in the evening on the 1st, and nearly an hour sooner by the 15th. He remains sensibly stationary just to the west of ϵ Tauri ("The Stars in their Seasons," Map I.); Uranus may be found about midnight, somewhere about half-way between β and η Virginis ("The Stars in their Seasons," Map V.); Neptune is now getting near the west, and is badly placed for the observer. The age of the moon to-day at noon is 4·3 days, and, quite obviously, she will be 18·3 days old at the same hour on the 15th. Occultations of stars are numerous during the coming fortnight. On February 4 the 6th mag. star, B.A.C. 1,119, will disappear at the moon's dark limb at 11h. 48m. p.m., at an angle of 171° from her vertex; and reappear at her bright limb at 12h. 34m. p.m., at an angle from her vertex of 280°. On February 5 δ^2 Tauri (a star of the 6th mag.) will disappear at the dark limb at 4h. 20m. p.m., at a vertical angle of 357°, to reappear at the bright limb at 4h. 44m. p.m., at an angle of 318° from the moon's vertex. A little later, at 4h. 56m. p.m., δ^3 Tauri, a 5th magnitude star, will disappear at the dark limb of the moon at an angle from her vertex of 105°. It will reappear at the bright limb, at a vertical angle of 223°, at 5h. 54m. p.m. On the 6th,

the 5½ mag. star, 119 Tauri, will disappear at the dark limb at 8h. 16m. p.m., at an angle of 95° from the vertex of the moon ; to reappear at her bright limb at 9h. 31m. p.m., at a vertical angle of 283°. Afterwards, at 9h. 0m. p.m., the 6th mag. star, 120 Tauri, will disappear at the dark limb at an angle of 96° from the moon's vertex, reappearing at her bright limb at 10h. 13m. p.m., at an angle from her vertex of 299°. On the 8th, 68 Geminorum, a 5½ mag. star, will disappear at the dark limb of the moon at 10h. 10m. p.m., at an angle of 23° from her vertex. It will reappear at her bright limb at 10h. 55m. p.m., at a vertical angle of 322°. On the 9th, "B.A.C." 2,872, a star of the 6th mag., will disappear at the moon's dark limb at 11 minutes after midnight, at an angle of 160° from her vertex. It will reappear at 12h. 31m. p.m., at an angle from her vertex of 196°. Lastly, on the 11th, another 6th mag. star, 16 Sextantis, will disappear at the bright limb of the moon at 6h. 31m. p.m. at a vertical angle of 328°. It will reappear at her dark limb, at an angle of 283° from her vertex, at 6h. 53m. p.m. The moon is travelling through Pisces all day to-day and to-morrow, crossing the boundary into Aries at 1 a.m. on Feb. 3. She continues in Aries until 5 p.m. on the 4th, when she enters Taurus. Her passage through this constellation occupies her until 5 a.m. on the 7th, when she enters the extreme northerly portion of Orion. This takes her almost exactly 12 hours to cross, and at 5 p.m. she emerges into Gemini ; which she quits at 7 a.m. on the 9th for Cancer. She leaves Cancer at 9h. p.m. on the 10th, and enters Leo, from which she descends into Sextans during the early afternoon of the 11th. She passes out of Sextans into Leo again about 3 p.m. on the 12th, finally quitting it for Virgo at 5 p.m. on the 13th. She is still travelling through this large constellation when our notes terminate.

When the Northern Pacific Railway was opened last July, the people at Vancouver's Island and Puget Sound came with all sorts of banners, including the Union Jack and the flags of Germany and other countries. Many of the flags bore suitable mottoes for the instruction of the party, and one of them was "Modesty is a great virtue, but you get on better without it."

Telephonic Communication between Moving Vessels. —The telephone has been successfully used in France to communicate between two vessels, one of which was towing the other. The wire was carried along one of the hawsers, and the circuit was completed through the copper on the bottom of the ships and the water. Conversation was carried on distinctly.

Luxotype.—We have been shown some specimens of luxotype, a process by which a printing block ready for use in newspapers or magazines can be obtained from paintings, photographs, natural objects, buildings, &c., without the aid of draughtsman or engraver ! The process is not described, but a scrutiny of the engravings shows how the blocks have been produced ; for the warp and weft of some gauze-like material (used either to intercept or to receive the light, in greater or less degree according to the quantity of light coming from the different parts of the original) can be clearly discerned. But be the process (patented) what it may, the results are marvellous. Some of the prints sent us compare favourably even with the fine wood-engravings which adorn *Harper's Magazine*, *Scribner's* (the *Century*, we would say), and now in Macmillan's *English Illustrated Magazine*. We do not know how the process compares in cost with the fine wood-engraving in these magazines. Possibly at present it may not be much cheaper ; but it is certain to become so before long. One little criticism we would venture to make. Where buildings are represented, great care should be taken in suitably placing the lines of the gauze-like material above mentioned. On the otherwise fine view of the gateway of St. John's College, Cambridge, the slight slant of the lines where their distance apart is about that of the lines of brickwork, gives a curious slanting effect to the part of the first court (just over the passage to the second court) seen through the gateway.



"Let Knowledge grow from more to more."—Alfred Tennyson.

Only a small proportion of Letters received can possibly be inserted. Correspondents must not be offended, therefore, should their letters not appear.

All Editorial communications should be addressed to the Editor of Knowledge; *all Business communications to the* Publishers, *at the Office, 74, Great Queen-street, W.C.* If this is not attended to delays arise for which the Editor is not responsible.

All Remittances, Cheques, and Post Office Orders should be made payable to Messrs. Wyman & Sons.

The Editor is not responsible for the opinions of correspondents.

No communications are answered by post, even though stamped and directed envelope be enclosed.

PONS'S COMET.

[1104]—It may be well to correct the statement of "F.R.A.S." ("The Face of the Sky," page 46), that Pons's Comet is no longer visible. I saw it last evening between 6.30 and 7.30, and again to-night it is a beautiful object in my three-inch Tulley, verifying the correctness of the figure by "Senex" in your current number. Many readers may be glad to be reminded of Mr. Bradgate's note in No. 109, where he says :—"On January 30 the comet will be in Cetus, on February 13 in Sculptor, and after the latter date will be invisible in these latitudes," as, according to him, there is still a good fortnight in which to observe this interesting object.

Shrewsbury, Jan. 27, 1884. Martin J. Harding.

RED SKY-GLOW.

[1105]—On Dec. 23 I sent to Knowledge a letter recording two observations of red sky-glow in daylight, but as it did not appear, I concluded that editorial discretion had relegated it to the w.p.b. Captain Noble's remark in Knowledge of yesterday (Letter 1100), that "observations of the glow in actual sunlight are both interesting and important," has caused me to think that my letter has not reached you—owing, perhaps, to Christmas demoralisation at the P.O.,—and as I have on two occasions since then seen the glow in sunlight, it may be worth while to record my experience. Between 11 and 12 a.m. on Nov. 25, I was watching a big, low-lying black cloud passing across the sky, and as it came in front of the sun I noticed that the sky surrounding the cloud was of a livid purply red colour (my wife called the colour "crushed strawberry"). There was a good deal of ragged cumulus cloud (cirro-c) at the time, and the weather was unsettled, with a very low barometer (28·82 in.). At that time the "after-glow" had not been seen in Sheffield, but was visible that night for the first time. Again, at 2 p.m. on December 18, a cirro-cumulus cloud passing in front of the sun showed a "crushed strawberry"-coloured sky surrounding it. On this occasion the weather was fine, and the barometer high (30·34 in.). Like most other parts of the country, we had a dense fog here on Christmas-day, and at half-past four it became of a beautiful mauve colour. This I saw, with several friends, at my house on the outskirts of Sheffield, and, though the house was enveloped by the fog, I found, when I went out later on, that the fog only extended a few hundred yards further in a south-westerly direction. The last occasion on which I saw the "after-glow" here was on January 11, and it lasted an hour and a-half after sunset. On January 12 the sky was cloudy, and after sunset the clouds were suffused with a pink colour. Since then we have had many clear and golden sunsets, but no "after-glow."

This week the weather has been very wet and squally, and the sky has been overcast much of the time, but on Wednesday, Jan. 23, the cloud canopy broke up, revealing the sun with the sky near it of a red colour; and again on Thursday, Jan. 24, there were many bright intervals of sunshine, and the sky in the neighbourhood of the sun was always of the same red colour as that first seen on Nov. 25.

On the two first occasions that I saw the red sky glow in daylight it was only visible when a cloud was in front of the sun; but on Jan. 23 and 24 it was visible when there was no cloud near the sun, and the colour extended a considerable distance from the sun.

To-day the barometer in Sheffield has been lower than I ever before noticed it. At 9 o'clock this morning the reading was 29·09 in. and at 6 p.m. it had fallen to 28·27 in. From 6 to 10 to-night the weather has been bright and fine, with frequent and vivid flashes of lightning, but no thunder. The barometer readings are all corrected for temperature, and reduced to sea level.

Museum, Sheffield, *Jan.* 26. E. HOWARTH.

BLUE MOON.

[1106]—There is a very old Norfolk proverbial saying, once in a blue moon. Can it have had its origin in the actual and yet very infrequent observation of that phenomenon? Or is it a mere random shot at an illustration of rare events?

The moon here in November was of the intensest sapphire blue, the perfectly clear sky looking rather slaty. This morning at 6.30 there was a fine sky-glow, and so last week. It certainly appeared to come from aqueous vapour. CENTRE-NORFOLK.

Jan. 22, 1884.

THE SENSES IN INFANTS—BLUE MOON.

[1107]—I have read with much interest "A. M. H. B.'s" papers on "The Senses in Infants," and beg to forward a few facts in connection with the subject.

I have a little girl just three years old. Before she was a week old, she fixed her eyes on my face and smiled. Thinking it to be a mere accident, I asked the nurse to take particular notice of her, and she assured me, after a day or two, that there was no doubt in the case as she acted towards her in the same manner.

When a month old she evinced a decided preference for anything *blue*, seeming specially pleased with a little sister's blue dress.

On reading in KNOWLEDGE what is there said about the discrimination of colours by infants, I decided to test my little girl's powers. (It has been common with her to talk of colours by name since she was two years old, and even earlier.) I placed before her some skeins of wool—cuttings from which I enclose—and asked her to give me the *red* one, when she declared there was no red one; and on my calling her a dunce, and pointing to No. 1, averred that it was not red but *pink* (the absence of a pink probably led her into error). [But No. 1 is not red; it is almost exactly carmine lake, and carmine lake *diluted* is not far from pink.—R.P.] Being asked for orange she selected No. 2, and, asked for the best yellow, chose 3. In the absence of 2 [orange], she chooses 5 [brick yellow] as orange; and while 3 [strong yellow] is before her, calls 4 [pale yellow] white. She selects both 6 and 7 as green, but shows a decided preference for 6.

Allow me to express the great pleasure KNOWLEDGE has from its commencement, and still continues to afford me, and to wish you and it continued success. J. G. BRYANT.

A PLANETARY CURIOSITY.

[1108]—It is required to describe on our globe as large a circle as possible with these properties:—

1. That it contains more land than water.
2. The circumference to cross more land than water.
3. No stream to cross the circumference outward; but any number may enter from without.

Another problem is, instead of the largest such circle, the one whose entering streams drain the most land outside it.

E. L. G.

LADYBIRDS—POLARISATION OF LIGHT.

[1109]—In the paper on "Ladybirds" in a late number of KNOWLEDGE, a curious fact connected with these insects is not noticed, and perhaps not generally known. I have noticed that at the end of the year they are in the habit of collecting in small colonies of a dozen or so, and closing themselves up in leaves. At any rate, I have found this frequently in Hampshire. You see an apparently dead leaf rolled up, and stuck together at the edges on the brambles by the roadside, and on opening these they are found to contain a mass of living ladybirds, apparently hybernating.

There is one subject which, I think, has never been touched upon in KNOWLEDGE, and on which, I think, a popular treatise in plain words is much wanted, viz., "Polarisation of Light"—especially as applied to the microscope. Would you, or would some of your correspondents kindly undertake this? Polarising objects for the microscope are so very pretty and interesting for popular exhibition, and always excite questions as to the cause of the varied colours; but I must admit that I find it extremely difficult to satisfy such questions in the case of ladies and people not at all acquainted with the elements of optical science. F.R.C.S.

LETTERS RECEIVED (SUB-EDITOR).

H. DAVEY.—P. S. BAGGE.—J. W. B.—F. W. H.—W. N.—W. M. T. Do not know.—H. B.—C. T.—A. L.—I. C. M. A.—E. P.—SENEX.—H.—E. G.—E. D. G.—A. H.—B. G.—C. H. G.—SOLARIUM.—COSMOPOLITAN.—E. J. C.—P. AND S.—W. A.—W. T.—J. E. LUSH.—L. F. B.—W. O. D.—NAGGDRIS.—W. M. M.—J. D. L.—W. H. S. M.—STARCH.—C. CARUS WILSON.—E. G.—HALLYARDS.—E. H.—A. F. OSBORNE.—W. E. P.—A. E.—E. F. MACGEORGE.—AN INQUIRER.—W. A. P.—J. L. C.—J. M.—J. P. H. G.—J. P.—J. T.—C. T. D.—H. G. W. You have quite misunderstood D. M.'s problem.—J. P.—H. G. M. TOD.—W. G. S.—S. C. T.—VIGNOLES.—J. H. W. LAING.—H. G. B.—A. SPURLING.—C. W. KIND.—TYRO.—R. HODGSON.—J. F.—H. K. P.—F. M. D.—O. R. JONES.—G. E. C.—C. C.—ROSE.—C. R. T.—T. G. S.—H. N. S.—E. A. D.—E. W. P. Queries *re* Bunsen cell answered in article "Batteries, IX." Other questions have really not yet been dealt with, but deferred until the description of an alternating machine (probably the Ferranti) is given.

HY. CLARK—M. J. HARDING—"ONE FOR HIS NOB." How can dealer make "about 185," pegging and scoring from these hands?—NOVICE. The double track with dates on the left.—J. M. GLADSTONE. Have no knowledge about measuring timber, probably some practical detail is involved. Certainly content of a cylindrical body, girth 24 in., and length 10 ft. would be 12 in. × 3·82 in. × 120 in. = 3·2 cubic feet.—H. TINSON. You do not extend the problem to include "crib."—W. J. C. Thanks. Your suggestion in accordance with facts; but just now many correspondence columns crowded out for want of space. It will be well to observe the Zodiacal light carefully in the evening during next two months.—C. J. BROWN. Thanks; but your observation would not help to determine real path of meteor in space.

Our Whist Column.

BY "FIVE OF CLUBS."

W. H. COLLINS.—So far as trick-making in the suit is concerned, the lead of Ace, from Ace, Queen, two small ones, is about as good as the lead of a small one: from Ace, Queen, one small one, not quite so good, because with the shorter suit there is less chance of being ruffed. But in most cases, considering the play of the whole hand, the lead of the smallest is better. Not so much better but that the old-fashioned lead of Ace from Ace, Queen, two small ones may not be defended. What I think fairly turns the scales in favour of the lead of a small card is that if a player systematically adopt this course his original lead of Ace is always understood as from one of two hands, Ace to five, or Ace, Queen, Knave, to four or five,—the second card showing which of the two cases is in question.

It would be better if in dealing with play as in your letter you would call *A*'s partner *Y* and *B*'s partner *Z*, since that is the plan uniformly followed in our illustrative games. In the *Field* these players are called respectively *X* and *Y*; but if the first two letters of the alphabet are given to one pair of partners it is much more natural (and what is more to the purpose, more easily remembered) to give the other pair the last two letters. The *Australasian* for reasons inscrutable gives the letters *W* and *X*.

A. C. M.—It must be merely accidental. Assuredly there is no reason why, after a misdeal, rather than at any other time, one hand of the four should have but one card of a suit. What odds did you take against this event, by the way?

JABEZ.—I would rather not say *what* I think of "purposely hurrying the deal when the opponents have not claimed honours which they were entitled to." A true Whist-player would be apt rather to delay the placing of the last few cards—even if, without knowing his partner's feelings on such matters, he were unwilling to call the opponents' attention to their omission. Of course, if a player thinks more of the money for which the game is played than he does of the game itself, the case is different. We know what to think of *him*, do we not?

MERRY.—More merry I think than your partner felt. You had two trumps, the opponents had indicated no great strength in trumps, having neither signalled nor led trumps; and you had established a long suit; *therefore* you led a forcing card [!]; yet your partner complained that you had ruined his hand. Well you certainly went the right way about it. The chances were he held four trumps (as it turned out). He was likely to regard your force as showing you held four trumps too. In any case, he could not well let the trick pass, nor discard, except the singleton be held in your suit; for he would have had to unguard his King in the fourth suit. He ruffs; the enemy get in their best suits, and you lose two tricks and the game. Thanks wholly to yourself, I fear I must tell you.

Our Chess Column.

By Mephisto.

KING'S GAMBIT.

THE normal variation of this Gambit, as pointed out in our previous article, arises from the following line of play:—

1. P to K4 / P to K4   2. P to KB4 / P takes P   3. KKt to B3 / P to KKt4   4. B to B4 / B to Kt2

White has four modes of continuing the attack; by 5. P to Q4, P to B3, Castles, P to KR4; in any case, however, Black should maintain a superior game.

(a) 5. P to Q4 / P to Q3 (best)

6.	P to B3 / P to Kt5		Castles / P to KR3
7.	QB takes P (a) / P takes Kt	Q to Kt3 / P takes Kt	Kt to B3 / Kt to K2
8.	Castles / B to K3	B takes P (ch) / K to K2	P to KKt3 / P to Kt5
9.	B takes B / P takes B	Castles / Kt to KR3	Kt to R4 / P to B6 (b)
10.	Q to Kt3 / Kt to Q2	B to R5 / Q to Kt sq.	
11.	Q takes P (ch) / Q to K2	Q to B2 / B to Kt5	

(a) If 7. Kt to Kt sq. / Q to R5 (ch)   8. K to B sq. / Q to B3   White may also Castle, in which case the game would become somewhat analogous to the Muzio attack. By a transposition of moves, we arrive at the position given below in variation (b), *i.e.*,

7. Castles / P takes Kt   8. Q takes P / Kt to B3   9. QB takes P / Castles, &c.

(b) These variations mostly merge into each other; for the continuation of the above line of play see under heading of 5. Castles, when, by a transposition of the fifth and sixth moves, the same position will be brought about.

(b) 5. P to B3 (weak) / P to Kt5 (best)

6.	Castles / P takes Kt	P to Q4 / P takes Kt	Q to Kt3 / P takes Kt
7.	Q takes P / Kt to KR3	Castles / P to Q4	B takes P (ch) / K to K2
8.	P to Q4 / Castles	B takes QP / P to QB3	Castles / Kt to R3
9.	B takes P / P to Q3	KB takes P (ch) / K takes B	B to B4 / P to B3
10.	B takes Kt / B takes B	Q takes P / Kt to B3	P to Q4 / P to Q3
11.	B takes P (ch) / K to R sq.	P to K5 / R to B sq. (a)	R takes P / Q to Kt3 (b)

(a) It will be seen that in all these variations, some of which are of a very attacking character, Black maintains a superiority; the continuation of this variation is:—

12. B takes P / K to Kt sq.   13. B to Kt5 / Q to Q4   14. B takes Kt / Q takes Q   15. R takes Q / B takes B, &c.

(b) If White now exchanges Queens, he has not enough attack left to equalise the game. If he plays 12. Q to R3, Black can answer B takes P (ch) with advantage; the best continuation seems to be:—

12. Q to B2 / B to Kt5   13. R takes P / Kt to Q2   14. P to KR3 / B to K3
15. B takes B / K takes B   16. R to R4 / Kt to B2   17. Q to B2 / B to B3

(c) 5. Castles / P to Q3
6. P to Q4 / P to KR3

7.	P to B3 / Q to K2	Kt to B3 / Kt to K2	P to KKt3 / P to Kt5
8.	Q to Kt3 / Kt to Q2	K to R sq. / B to Kt5 (b)	Kt to R4 / P to B6
9.	P to Kt3 / P to Kt5 (a)	P to Kt3 / P takes P	P to B3 / B to B3
10.	QB takes P / P takes Kt	B takes P (ch) / K takes B	Kt takes P / P takes Kt
11.	R takes P / KKt to B3	Kt to K5 (ch) / K to Kt sq.	Q takes P / Q to K2
12.	P to K5 / P takes P	Kt takes B / P takes P	B to B4 / Kt to B3
13.	P takes P / Kt to Kt5	Q to B3 / K to R2	Kt to Q2 / B to Q2
14.	B takes P (ch) / Q takes B	P to K5 / P takes P	P to K5 / P takes P
15.	P to K6 / Q to R4	B takes P / P takes B	B takes KP / B takes B
16.	P takes Kt (ch) / B takes P	Q to B7 / Kt to Kt3	B takes P (ch) / K to Q sq.
17.	P to KR4 / Castles QR	Kt to K4 / White wins	P takes B / Kt takes P (c)

(a) If P to KR4, then Kt to Kt3.

(b) We give this variation for purposes of attack. Black, however, would do better to play either B to K3 or Castles, with a safe game.

(c) Continuing with:—

18. Q takes P / Q to B4 (ch)   19. R to B2 / B to B3   20. Q to Kt3 / Kt to Q6' and wins.

Problems received with thanks from Tom, F. G. D., H. W. Sherrard.

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GAMBLING SUPERSTITIONS.

BY RICHARD A. PROCTOR.

(*Continued from page* 51.)

WHY is it, then, it will be asked, that the inexorable law of averages is yet not to be trusted? For this reason, simply, that the mode of its operation is altogether uncertain. If in a thousand trials there has been a remarkable preponderance of any particular class of events, it is not a whit more probable that the preponderance will be compensated by a corresponding deficiency in the next thousand trials than that it will be repeated in that set also. The most probable result of the second thousand trials is precisely that result which was most probable for the first thousand—that is, that there will be no marked preponderance either way. But there *may be* such a preponderance; and it may lie either way. It is the same with the next thousand, and the next, and for every such set. They are in no way affected by preceding events. In the nature of things, how can they be? But, "the whirligig of time brings in its revenges" in its own way. The balance is restored just as chance directs. It may be in the next thousand trials, it may be not before many thousands of trials. We are utterly unable to guess when or how it will be brought about.

But it may be urged that this is mere assertion; and many will be very ready to believe that it is opposed to experience, or even contrary to common sense. Yet experience has over and over again confirmed the matter, and common sense, though it may not avail to unravel the seeming paradox, yet cannot insist on the absurdity that coming events of pure chance are affected by completed events of the same kind. If a person has tossed "heads" nine times running (we assume fair and lofty tosses with a well-balanced coin), common sense teaches him, as he is about to make the tenth trial, that the chances on that trial are precisely the same as the chances on the first. It would, indeed, have been rash far him to predict that he would reach that trial without once failing to toss "head;" but as the thing has happened, the odds originally against it count for nothing. They are disposed of by known facts. I have said, however, that experience confirms our theory. It chances that a series of experiments have been made on coin-tossing. Buffon was the experimenter, and he tossed thousands of times, noting always how many times he tossed "head" running before "tail" appeared. In the course of these trials he many times tossed "head" nine times running. Now, if the tossing "head" nine times running rendered the chance of tossing a tenth head much less than usual, it would necessarily follow that in considerably more than one-half of these instances Buffon would have failed to toss a tenth "head." But he did not. I shall give hereafter the exact numbers. Here, I note only that in about half the cases in which he tossed nine "heads" running, the next trial also gave him "head;" and about half of these tossing of ten successive "heads" were followed by the tossing of an eleventh "head." In the nature of things this was to be expected.

And now let us consider the cognate questions suggested by our sharper's ideas respecting the person who plays. This person is to consider carefully whether he is "*in vein*," and not otherwise to play. He is to be cool and businesslike, for fortune is invariably adverse to an angry player. Steinmetz, who appears to place some degree of reliance on the suggestion that a player should be "in vein," cites in illustration and confirmation of the rule the following instance from his own experience:—"I remember," he says, "a curious incident in my childhood which seems very much to the point of this axiom. A magnificent gold watch and chain were given towards the building of a church, and my mother took three chances, which were at a very high figure, the watch and chain being valued at more than £100. One of these chances was entered in my name, one in my brother's, and a third in my mother's. I had to throw for her as well as myself. My brother threw an insignificant figure; for myself I did the same; but, oddly enough, I refused to throw for my mother on finding that I had lost my chance, saying that I should wait a little longer—rather a curious piece of prudence" (read, rather, superstition) "for a child of thirteen. The raffle was with three dice; the majority of the chances had been thrown, and thirty-four was the highest." (It is to be presumed that the three dice were thrown twice, yet "thirty-four" is a remarkable throw with six dice, and "thirty-six" altogether exceptional.) "I went on throwing the dice for amusement, and was surprised to find that every throw was better than the one I had in the raffle. I thereupon said, 'Now I'll throw for mamma.' I threw thirty-six, which won the watch! My mother had been a large subscriber to the building of the church, and the priest said that my winning the watch for her was quite *providential*. According to M. Houdin's authority, however, it seems that I only got into 'vein,'—but how I came to pause and defer throwing the last chance has always puzzled me respecting this incident of childhood, which made too great an impression ever to be effaced."

It is probable that most of our readers can recall some circumstance in their lives, some surprising coincidence, which has caused a similar impression, and which they have found it almost impossible to regard as strictly fortuitous.

In chance games especially, curious coincidences of the sort occur, and lead to the superstitious notion that they are not mere coincidences, but in some definite way associated with the fate or fortune of the player, or else with some event which has previously taken place,—a change of seats, a new deal, or the like. There is scarcely a gambler who is not prepared to assert his faith in certain observances whereby, as he believes, a change of luck may be

brought about. In an old work on card-games the player is gravely advised, if the luck has been against him, to turn three times round with his chair, "for then the luck will infallibly change in your favour."

Equally superstitious is the notion that anger brings bad luck, or, as M. Houdin's authority puts it, that "the demon of bad luck invariably pursues a passionate player." At a game of pure chance good temper makes the player careless under ill-fortune, but it cannot secure him against it. In like manner, passion may excite the attention of others to the player's losses, and in any case causes himself to suffer more keenly under them, but it is only in this sense that passion is unlucky for him. He is as likely to make a lucky hit when in a rage as in the calmest mood.

It is easy to see how superstitions such as these take their origin. We can understand that since one who has been very unlucky in games of pure chance, is not antecedently likely to continue equally unlucky, a superstitious observance is not unlikely to be followed by a seeming change of luck. When this happens the coincidence is noted and remembered; but failures are readily forgotten. Again, if the fortunes of a passionate player be recorded by dispassionate bystanders, he will not appear to be pursued by worse luck than his neighbours; but he will be disposed to regard himself as the victim of unusual ill-fortune. He may perhaps register a vow to keep his temper in future; and then his luck may seem to him to improve, even though a careful record of his gains and losses would show no change whatever in his fortunes.

(To be continued.)

OUR EARTH'S DUST ENVELOPE.*

By Prof. S. P. Langley.

At first I supposed the sunset matter a local phenomenon, but when the reports showed it to have been visible all over the world, it was obvious that we must look for some equally general cause. We know but two likely ones, and these have been already brought forward. One is the advent of an unusual amount of meteoric dust. While something over ten millions of meteorites are known to enter our atmosphere daily, which are dissipated in dust and vapour in the upper atmosphere, the total mass of these is small as compared with the bulk of the atmosphere itself, although absolutely large. It is difficult to state with precision what this amount is. But several lines of evidence lead us to think it is approximately not greatly less than 100 tons per diem, nor greatly more than 1,000 tons per diem. Taking the largest estimate as still below the truth, we must suppose an enormously greater accession than this to supply quantity sufficient to produce the phenomenon in question; and it is hardly possible to imagine such a meteoric inflow unaccompanied with visual phenomena in the form of "shooting stars," which would make its advent visible to all. Admitting, then, the possibility of meteoric influence, we must consider it to be nevertheless extremely improbable.

There is another cause which seems to me more acceptable —that of volcanic dust; and in relation to this presence of dust in the entire atmosphere of the planet, I can offer some little personal experience. In 1878 I was on the upper slopes of Mount Etna, in the volcanic wastes, three or four hours' journey above the zone of fertile ground. I passed a portion of the winter at that elevation engaged in studying the transparency of the earth's atmosphere. I was much impressed by the fact that here, on a site where the air is supposed to be as clear as anywhere in the world, at this considerable altitude, and where we were surrounded by snow-fields and deserts of black lava, the telescope showed that the air was filled with minute dust particles, which evidently had no relation to the local surroundings, but apparently formed a portion of an envelope common to the whole earth. I was confirmed in this opinion by my recollection that Prof. Piazzi Smyth, on the Peak of Teneriffe, in mid-ocean, saw these strata of dust rising to the height of over a mile, reaching out to the horizon in every direction, and so dense that they frequently hid a neighbouring island mountain, whose peak rose above them, as though out of an upper sea. In 1881 I was on Mount Whitney, in Southern California, the highest peak in the United States, unless some of the Alaskan mountains can rival it. I had gone there with an expedition from the Alleghany Observatory, under the official direction of General Hazen, of the Signal Service, and had camped at an altitude of 12,000 ft., with a special object of studying analogous phenomena. On ascending the peak of Whitney, from an altitude of nearly 15,000 ft., the eye looks to the east over one of the most barren regions in the world. Immediately at the foot of the mountain is the Inyo desert, and on the east a range of mountains parallel to the Sierra Nevadas, but only about 10,000 ft. in height. From the valley the atmosphere had appeared beautifully clear. But from this aërial height we looked down on what seemed a kind of level dust-ocean, invisible from below, but whose depth was 6,000 ft. or 7,000 ft., as the upper portion only of the opposite mountain range rose clearly out of it. The colour of the light reflected to us from this dust-ocean was clearly red, and it stretched as far as the eye could reach in every direction, although there was no special wind or local cause for it. It was evidently like the dust seen in mid-ocean from the Peak of Teneriffe—something present all the time, and a permanent ingredient in the earth's atmosphere.

At our own great elevation the sky was of a remarkably deep violet, and it seemed at first as if no dust was present in this upper air, but in getting, just at noon, in the edge of the shadow of a range of cliffs, which rose 1,200 feet above us, the sky immediately about the sun took on a whitish hue. On scrutinising this through the telescope it was found to be due to myriads of the minutest dust particles. I was here at a far greater height than the summit of Etna, with nothing around me except granite and snow-fields, and the presence of this dust in a comparatively calm air much impressed me. I mentioned it to Mr. Clarence King, then Director of the United States Geological Surveys, who was one of the first to ascend Mount Whitney, and he informed me that this upper dust was probably due to the "loess" of China, having been borne across the Pacific and a quarter of the way around the world. We were at the summit of the continent, and the air which swept by us was unmingled with that of the lower regions of the earth's surface. Even at this great altitude the dust was perpetually present in the air, and I became confirmed in the opinion that there is a permanent dust-shell inclosing the whole planet to a height certainly of about three miles (where direct observation has followed it), and not improbably to a height even greater; for we have no reason to suppose that the dust carried up from the earth's surface stops at the height to which we have ascended. The meteorites, which are consumed at an average height of twenty to forty miles, must add somewhat to this. Our observations with special apparatus on Mount Whitney went to show that the red

* From the *New York Daily Tribune*, Jan. 2.

rays are transmitted with greatest facility through our air and rendered it extremely probable that this has a very large share in the colours of a cloudless sky at sunset and sunrise, these colours depending largely upon the average size of the dust particles.

It is especially worth notice that, as far as such observations go, we have no reason to doubt that the finer dust from the earth's surface is carried up to a surprising altitude. I speak here, not of the grosser dust particles, but of those which are so fine as to be individually invisible, except under favouring circumstances, and which are so minute that they might be an almost unlimited time in settling to the ground, even if the atmosphere were to become perfectly quiet. I have not at hand any data for estimating the amount of dust thrown into the air by such eruptions as those which recently occurred in Java and Alaska. But it is quite certain, if the accounts we have are not exaggerated, that the former alone must have been counted by millions of tons, and must in all probability have exceeded in amount that contributed by meteorites during an entire year. Neither must it be supposed that this will at once sink to the surface again. Even the smoke of a conflagration so utterly insignificant, compared with nature's scale, as the burning of Chicago, was, according to Mr. Clarence King, perceived on the Pacific Coast; nor is there any improbability that I can see in supposing that the eruption at Krakatoa may have charged the atmosphere of the whole planet (or at least of a belt encircling it) for months with particles sufficiently large to scatter the rays of red light and partially absorb the others, and to produce the phenomenon that is now exciting so much public interest. We must not conclude that the cause of the phenomena is certainly known. It is not. But I am inclined to think that there is not only no antecedent improbability that these volcanic eruptions on such an unprecedented scale are the cause, but that they are the most likely cause which we can assign."

MR. RUSKIN ON A NEW STORM-CLOUD.

MR. RUSKIN gave on Monday a lecture at the London Institution on a new kind of storm-cloud which occurred to him, as a novelty, in 1871. He calls it "The Plague Wind," and would seem to have felt it very much. He commented with fiery wrath on the inaccuracy of "scientific people's" terms. Such folk could tell him about the moon and the seven stars, and how they moved and what they were made of, and he did not care a copper spangle how they moved and what they were made of: *he* (Mr. Ruskin) could not move them nor make them other than they were. But as the new storm-cloud had moved him and made him (apparently) other than he had been, he would like to know what it was. He would describe it accurately, not like "the scientific people." It always blew tremendously, giving the quivering leaves an expression of anger as well as distress! Its sound was a hiss, or as a whistling on a flute made of a file!! It polluted the character as well as enhanced the violence of the storm! The gloom was Manchester devil's darkness, sulphurous chimney-pot vomit (let scientific people note this) and blanched the sun instead of reddening it after the manner of healthy clouds. When filthy, mangy, miserable plague winds blew, the sun was choked out of the sky all day long! If they wished to see how the sun looked through a plague-cloud let them throw a bad half-crown into a basin of soap and water! If he were asked what might be the conceivable cause or meaning of all this—of blanched sun, blighted grass, and blinded man—he could tell them none according to their modern beliefs and knowledge; but he could tell them what meaning it would have borne to the men of old time. For the last twenty years England and all foreign nations, either tempting her or following her, had done iniquity by proclamation—every man doing, by the advice of his superiors, as much injustice to his brother as it was in his power to do. In such moral gloom, every seer of old predicted physical gloom, saying, "The light shall be darkened in the heavens thereof, and the stars shall withdraw their shining." What was best to be done? Whether they could bring back the sun or not, they could assuredly bring back their own cheerfulness, honesty, tranquillity, and hope. They might not be able to say to the winds, "Peace, be still;" but *they could check the insolence of their own lips and the troubling of their own passions*; and that it would be extremely well to do, even though the day were coming when the sun should be as darkness and the moon as blood.

We venture to express a hope that Mr. Ruskin will succeed in checking troubled passions and insolence of lips. As for accuracy of expression,—"mangy," "devil's darkness," "chimney-pot vomit"—we "have them all three ready."

THE CHEMISTRY OF COOKERY.

XXVIII.

By W. MATTIEU WILLIAMS.

I NOW proceed to examine the chemical changes which occur in the course of the cookery of vegetable substances used for food. My readers will remember that I referred to Haller's statement: "*Dimidium corporis humani gluten est*," which applies to animals generally, viz., that half of their substance is gelatine, or that which by cookery becomes gelatine. This abundance depends upon the fact that the walls of the cells and the frame-work of the tissues are composed of this material.

In the vegetable structure we encounter a close analogy to this. Cellular structure is still more clearly defined than in the animal, as may be easily seen with the help of a very moderate microscopic power. Pluck one of the fibrils that you see shooting down into the water of the hyacinth glasses just at this season, or, failing one of these, any other succulent rootlet. Crush it between two pieces of glass and examine. At the end there is a loose spongy mass of round cells, these merge into oblong rectangular cells surrounding a central axis of spiral tube or tubes or greatly elongated cell structure. Take a thin slice of stem, or leaf, or flower, or bark, or pith, examine in like manner, and cellular structure of some kind will display itself, clearly demonstrating that whatever may be the contents of these round, oval, hexagonal, oblong, or otherwise regular and irregular cells, we cannot cook and eat any whole vegetable, or slice of vegetable, without encountering a large quantity of cell wall. It constitutes far more than half of the substance of most vegetables, and therefore demands prominent consideration. It exists in many forms with widely differing physical properties, but with very little variation in chemical composition, so little, that in many chemical treatises cellular tissue, cellulose, lignin, and woody fibre are treated as chemically synonymous. Thus, Miller says, "Cellular tissue forms the groundwork of every plant, and when obtained in a pure state, its composition is the same,

whatever may have been the nature of the plants which furnished it, though it may vary greatly in appearance and physical characters; thus, it is loose and spongy in the succulent shoots of germinating seeds, and in the roots of plants, such as the turnip and the potato; it is porous and elastic in the pith of the rush and the elder; it is flexible and tenacious in the fibres of hemp and flax; it is compact in the branches and wood of growing trees; and becomes very hard and dense in the shells of the filbert, the peach, the cocoanut, and the *Phytelephas* or vegetable ivory."

Its composition in all these cases is that of a *carbohydrate*, *i.e.*, carbon united with the elements of water, which, by the way, should not be confounded with a *hydrocarbon*, or compound of carbon with hydrogen simply, such as petroleum, fats, essential oils, and resins. There is, however, some little chemical difference between wooden tissue and the pure cellulose that we have in finely carded cotton, in linen, and pure paper pulp, such as is used in making the filtering paper for chemical laboratories, which burns without leaving a weighable quantity of ash. The woody forms of cellular tissue owe their characteristic properties to an incrustation of *lignin*, which is often described as synonymous with cellulose, but is not so. It is composed of carbon, oxygen, and hydrogen, like cellulose, but the hydrogen is in excess of the proportion required to form water by combination with the oxygen.

My own view of the composition of this incrustation (lignin properly is called) is that it consists of a carbohydrate united with a hydro-carbon, the latter having a resinous character; but whether the hydro-carbon is chemically combined with the carbo-hydrate (the resin with the cellulose), or whether the resin only mechanically envelopes and indurates the cellulose I will not venture to decide, though I incline to the latter view. As we shall presently see, this view of the constitution of the indurated forms of cellular tissue has an important practical bearing upon my present subject. To indicate this beforehand I will put it grossly as opening the question of whether a very advanced refinement of scientific cookery may or may not enable us to convert nutshells, wood shavings, and sawdust into wholesome and digestible food. I have no doubt whatever that it may. It could be done at once if the incrusting resinous matter were removed, for pure cellulose in the form of cotton and linen rags has been converted into sugar artificially in the laboratory of the chemist; and in the ripening of fruits such conversion is effected on a large scale in the laboratory of nature. A Jersey pear, for example, when full grown in autumn is little better than a lump of acidulated wood. Left hanging on the leafless tree, or gathered and carefully stored for two or three months, it becomes by nature's own unaided cookery, the most delicious and delicate pulp that can be tasted or imagined.

Certain animals have a remarkable power of digesting ligneous tissue. The beaver is an example of this. The whole of its stomach, and more especially that secondary stomach the *cæcum*, is often found crammed or plugged with fragments of wood and bark. I have opened the crops of several Norwegian ptarmigans, and found them filled with no other food than the needles of pines, upon which they evidently feed during the winter. The birds, when cooked, were scarcely eatable on account of the strong resinous flavour of their flesh.

I may here, by the way, correct the commonly-accepted version of a popular story. We are told that when Marie Antoinette was informed of a famine in the neighbourhood of the Tyrol, and of the starving of some of the peasants there, that she replied, "I would rather eat pie-crust" (some of the story-tellers say "pastry") "than starve." Thereupon the courtiers giggled at the ignorance of the pampered princess who supposed that starving peasants had such an alternative food as pastry. The ignorance, however, was all on the side of the courtiers and those who repeat the story in its ordinary form. The princess was the only person in the Court who really understood the habits of the peasants of the particular district in question. They cook their meat, chiefly young veal, by rolling it in a kind of dough made of sawdust, mixed with as little coarse flour as will hold it together; then place this in an oven or in wood embers until the dough is hardened to a tough crust, and the meat is raised throughout to the cooking point. Marie Antoinette said that she would rather eat *croutins* than starve, knowing that these *croutins*, or meat pie-crusts, were given to the pigs; that the pigs digested them, and were nourished by them in spite of the wood-sawdust.

When I come to the other constituents of vegetable food it will be understood that the changes effected in their cookery are but nominal, and that nearly the whole business of vegetable cookery consists in rendering the cellular tissue more digestible than it is in the raw state; or in breaking it up to liberate its contents. When on the subject of cooking animal food, I had to define the cooking temperature as determined by that at which albumen coagulates, and to point out the mischief arising from exceeding that temperature and thus rendering the albumen horny and indigestible.

No such precautions are demanded in the boiling of vegetables. The work to be done in cooking a cabbage or a turnip, for example, is merely to soften the cellular tissue by the semi-solvent action of hot water; there is nothing to avoid in the direction of over-heating. Even if the water could be raised above 212° the vegetable would be rather improved than injured thereby.

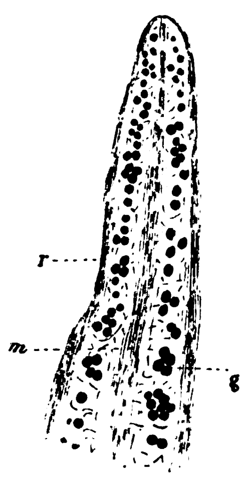
The question that now naturally arises is whether modern science can show us that anything more can be done in the preparation of vegetable tissue than the mere softening in boiling water. In my first paper I said that the practice of using the digestive apparatus of sheep, oxen, &c., for the preparation of our food is merely a transitory barbarism, to be ultimately superseded by scientific cookery, by preparing vegetables in such a manner that they shall be as easily digested as the prepared grass we call beef and mutton. I do not mean by this that the vegetable we should use shall be grass itself, or that grass should be one of the vegetables. We must, for our requirement, select vegetables that contain as much nutriment in a given bulk as our present mixed diet, but in doing so we encounter the serious difficulty of finding that the readily soluble cell wall or main bulk of animal food—the gelatine—is replaced in the vegetable by the cellulose, or woody fibre, which is not only more difficult of solution, but is not nitrogenous, is only a compound of carbon, oxygen, and hydrogen.

PLEASANT HOURS WITH THE MICROSCOPE.

By Henry J. Slack, F.G.S., F.R.M.S.

Among the most curious discoveries in natural history are those relating to the consortion, or dwelling together, of different plants or animals under conditions of enmity, utility, or neutrality. If a disease-producing organism fixes itself on a plant or animal, a one-sided warfare is the result. This kind of parasite lives to the serious loss of its victim. In other cases of parasitism no

such serious harm is produced, but no advantage is gained except by the invader. There are, however, many other cases in which two very different creatures live together as *commensalists*—Fellow-Boarders—and in some way promote each other's well-being. Fellow-Boarders may be free, or one fixed to the other. "Every fish," says Von Beneden,* "is a living and moving territory, on which a fauna is developed possessing special interest. When a small animal claims to profit by the fins of one larger than itself, accompanies it in its chase, and picks up spoils which the larger one disdains or abandons, we see none of the motives which characterise parasitism." In the paper from which this passage is quoted many interesting cases are mentioned; and one of the most curious occurs on our coasts, where the sea anemone, *Adamsia palliata*, attaches itself to the shell occupied by a soldier-crab. The anemone gets the advantage of the crab's locomotion—in fact, keeps his carriage—and for some reason its presence is so pleasant to its coachman, that he helps to feed it and looks after it when he changes his shell. Passing from examples of the free fellow-boarders, we may come to others connected inextricably for life, and perhaps the most remarkable is the union of fungi and algæ to form lichens. This permanent lodging and growing together is called *symbiosis*, or life partnership, and may be understood by the microscopical examination of any of the common lichens hanging from foliage or trees. The reader who wishes to investigate this matter is very likely to gather *Ramalina farinacea*, a greyish, greeny, mealy-looking, moss-like tuft. If not wet and soft, it should be soaked well in water, and some of the thinnest possible slices cut with a sharp penknife from one of its slender branches. If these are examined with a power of about 300 in a drop of water under the microscope, some of the pieces will look like a fragment of the annexed figure, copied from Sachs, but to make a clean and perfect section requires care and skill. It will be seen that the lichen is composed of layers of different materials, and that the grey or colourless structure encloses green cells—the black dots in the sketch. If these are compared with some



Usnea Barbata—Longitudinal section of a slender branch, soaked in potash solution (*r*, the cortex; *m*, loose medullary tissue; *g*, the gonidial layer).

of the simple algæ that give a green colouring to wet stones, a family likeness will at once be seen, and the observer will also notice in the colourless portion of the lichen, tubular threads like those of common moulds. This complex character of lichens was long a puzzle, but a few years ago Schwendeuer startled biologists and enraged lichenologists by declaring their favourites to be fungi parasitic upon algæ, and, although recent investigations relieve the fungi from the bad character of parasitism, the lichen has been proved to afford an instance of life partnership.

The general character of fungi assimilates them in some particulars to animals. They do not, like true plants, digest carbonic acid, but obtain their nutriment from matter that plants or animals have already organised. The typical plant contains chlorophyll, the peculiar green matter of leaves, and by its means, under the stimulus of light, they decompose carbonic acid, use the carbon to build up their own structure, and evolve oxygen gas, which they also take up, as animals do, in their process of respiration. "Algæ," as Sachs observes, "are never true parasites, although they very commonly live upon other plants." Fungi, as all know, can live in the dark, and many hate the light.

The chlorophyll of algæ, as in many sea-weeds, does not always give a green tint to the plants. The colour may be masked by red or brown, or other pigments, and, as Mr. Sorby has shown, the chlorophyll of the higher plants comprises several materials. If the green cells of the algæ in a lichen are removed they will grow in moisture, and lead an independent life, and various experimenters—Stahl in particular—have produced lichens by placing the algæ under circumstances that enabled the fungoid filaments to embrace them. In a recent lecture, delivered before the Botanical Society of Belgium, M. Leo Errera imagines a fungus of the Ascomycetes sort* meeting with a suitable algue, and thus making love to her:—"Mademoiselle, there are vast desert regions which I have a great desire to colonise. Deign to unite with me, for they are not habitable for either of us separately. Delicate as you are, you would be roasted by the sun; you would not be able to fix yourself and absorb the animal aliments which are indispensable for you. For my part, I am more capable of endurance: I could stick myself tight, but I should not find the organic substance I require, and I have not, like you, the talent of feeding upon air, and especially carbonic acid. Let us associate, and we two will reign over immense spaces, where there is none to challenge us. You will supply the household with organic matter. I will shelter you. I will attach myself to the soil, from which I will extract the inorganic substances required for our common interest." The algue can only reply, "Willingly, sir." The bargain is concluded—the lichen is made.

M. Errera says the algues which act as gonidia to the lichens belong to divers groups—green or bluish *confervæ*, *palmellaceæ*, *nostocs*, &c. On the other hand, he observes, we do not see why the *Ascomycetes* alone of fungi should have the privilege of making lichens, and in fact it has been recently shown that in lichens belonging to the genera *cora* and *rhipidomena* the fungus is not an Ascomycete, but a basidiomycete allied to *stereum*.†

Lichens are remarkable for longevity, and some of the rock-adhering kinds are believed to reach the term of a thousand years. The union of fungus and algue, each belonging to organisms classed low down in the biological series, thus leads to a connection that outlasts many generations of human beings, and survives their cities' decay.

* See *Student and Intellectual Observer*, vol. v., p. 143.

* Having fructifying bodies, *sporidia*, enclosed in sacs *asci*, the morell belongs to them.

† *Stereum hirsutum* is the pretty flat banded fungus that grows in roundish projections from rotting wood.

THE AMERICAN ENCYCLOPÆDIA.*

THE action of the laws of international copyright, or rather the effect of the absence of a law of copyright between Great Britain and America is illustrated somewhat singularly by American Cyclopædias. Americans have an excellent Cyclopædia, known as Appleton's because published by Messrs. Appleton of Broadway, New York. In many respects that work contrasts favourably with our best cyclopædias; but it is hardly necessary to say that a work in sixteen not very thick octavo (imp.) volumes cannot contend in fulness throughout with the series of thick quarto volumes constituting the "Encyclopædia Britannica." The very completeness of "Appleton's Cyclopædia" makes a difference of scale an absolute necessity,—though it might be readily shown that many subjects are treated much more satisfactorily in the smaller American than in the larger British work.

Failing a cyclopædia of their own on the scale of our chief work of the kind, the American public have largely used the "Encyclopædia Brittanica," Chambers' "Cyclopædia," and other works of the sort, of which pirated editions (we use the word in no offensive sense—properly published editions would simply be sold at a loss in America) have been issued at a price bringing them into favourable competition with home productions. But our British cyclopædias are, on the one hand, very uneven in treatment, and, on the other, they are not in all respects perfectly suited to the American public,—which is very "live," and demands therefore the latest information about peoples, countries, inventions, and discoveries. Even accounts of living persons which our British cyclopædias are for the most part careful to exclude, form not only a desirable but an essential feature in a truly American work of reference. In other directions—less questionable perhaps, (for men's works, not their lives, should concern us)—Americans seek much earlier and more complete information than suffices for the old-fashioned Britisher.

It has been to meet this demand that the *Encyclopædia Americana* has been designed. It is intended, as stated in the preface, to supplement and accompany the great encyclopædias, completing them in particulars where they appear deficient and bringing down to the latest date their statements and descriptions.

Regarded with due reference to its purpose (and we regret to notice that in more than one review which has appeared on this side of the Atlantic, the expressly stated purpose of the work has been entirely and most unfairly overlooked) the *Encyclopædia Americana* is a marvel of fulness, and in most cases of accuracy of information. Among those cases in which matters are not stated with perfect accuracy may be cited certain statements respecting European persons of prominence in dynastic, political, and military affairs. These mistakes do not concern us here, nor perhaps do they matter much anywhere; still if such matters are introduced into a Cyclopædia "of Arts, Science, and General Literature," it is as well they should be given accurately. Persons with fairly good memories who followed with interest the events of the Franco-German war will note here several remarkable mistakes. Even the account of the cause of the Prince of Wales's illness in 1871 might have been given correctly in the same space as the incorrect cause here mentioned. (In glancing over the article on this gentleman, we note with some amusement what looks really like a travestie of an old and ludicrous story. We have all heard how one who spoke of the birth of some king's "daughter" was gravely told it was "not a daughter but a princess;" here, however, we have an almost funnier combination, being informed that the Prince of Wales "has five children, two princes and three princesses!")

When we return, however, to such articles as those on Agriculture, Ants, Architecture, Artesian Wells, Birds, Bridges, and a host of others which might be mentioned, we forget such slips in matters of no real importance. These are altogether admirable, carefully written, excellently illustrated, and brought closely up to the present time. Some articles, as those on Bacteria and the like, relate to matters about which all our really valuable information has been obtained since the current edition of the "Encyclopædia Britannica" was commenced. The question is suggested whether the plan of publishing a large cyclopædia in a series of volumes produced according to their alphabetical order might not be improved upon. Why, for instance, should the present edition of the "Encyclopædia Britannica" be (for the moment) up to date in regard to Meteorology and altogether out of date in regard to Astronomy? By the way, we see with some surprise that the "Encyclopædia Americana" adds nothing on the subject of Astronomy (except three short articles on the Altazimuth, Asteroids, and the Astrolabe) to what the editor of KNOWLEDGE (who also contributed the articles on Astronomy to Appleton's "Cyclopædia") has given in the "Encyclopædia Britannica" on the subject. The nine years which have passed sinse the present edition of this work was begun have been far from unfruitful in astronomical discovery. We trust that in the treatment of special astronomical subjects this will be rectified.

Many of the articles in this excellent cyclopædia while specially valuable to Americans will be exceedingly interesting to us on this side of the Atlantic. Those relating to matters,—as laws, customs, manners, modes of speaking, and so forth,—in which our cousins over the "big water" differ from ourselves are full of interest. Amongst such articles may be cited especially Americanisms, American literature, Army, Adultery (showing the variety of American law thereon), Blind (America takes singular care of the afflicted), &c. Articles relating to the separation of the States from the mother country, or to circumstances in American history connected with that event, also possess (or should possess) great interest for British readers. The struggle for just rights which with us ended without actually displacing from (titular) sovereignty the lineal representatives of those exemplary freebooters, Cerdic the Saxon and Rollo the Norman, ended otherwise in America. But this does not in reality separate us more from our kinsfolk across the water than we must in any case have been eventually separated as they rose in power and influence among the nations. Most unfortunately separation was not effected without fighting: but Americans have long since learned to blame for this those really responsible, not a nation as much wronged as themselves by the wrong-headed obstinacy which caused the contest. The progress therefore of America as a nation is as full of interest for all Britons, save the unwise and narrow-minded, and is viewed with feelings as free from all lingering trace of the animosity engendered during conflict, as if no struggle had taken place. So viewing matters American, a large portion of the volume before us has a special interest on this side of the Atlantic. The rest of the work forms a most valuable supplement to our chief national cyclopædia. Every part of the work in fact is valuable, except only those few passages relating to matters European where errors have been made in matters which after all are of no intrinsic importance.

* *The Encyclopædia Americana:* a Supplemental Dictionary of Arts, Sciences, and General Literature. Illustrated. Vol. I. A—Cen. (J. M. Stoddart: New York, Philadelphia, London.)

SONG AND SPEECH.*

HOMER, the great singer of old, takes articulate speech as the distinguishing characteristic of man, creating by poetic insight a more perfect definition than the philosophers of Greece in later times could frame after careful thought. This divine gift of speech with its near kinsman song, this distinctively human power, is treated by most as if it were a possession needing neither culture to improve nor care to preserve. Though scarce one person in a hundred can use his voice either in speech or song as it might be used, though hundreds lose their voices or all the beauty from their voices many years before advancing years might fairly be expected to rob the vocal organs of their power, yet scarcely any seem to learn the lesson so often taught, or to note the warning thus often conveyed. Such a work as the present, therefore, is much needed, and deserves the careful attention not of those only who have to employ the voice professionally, whether in singing or speaking, but of all who care for the voice. We may apply specifically to this case the general saying of the great teacher of our age. "If any one doubts," says Mr. Herbert Spencer, "the importance of an acquaintance with the principles of physiology as a means to complete living, let him look around and see how many men and women he can find in middle or later life who are thoroughly well." If any doubt the importance of knowledge respecting the principles of vocal utterance and vocal culture let him consider how few men and women in middle or later life have the thorough command of their vocal organs.

In this work, the authors, one with wide experience in vocal and aural surgery the other especially skilled in vocal training, deal first with the laws of sound bearing on the voice. It will interest some to know that the lowest tone yet attained, so far as is known, by a bass voice, is F_{III} (with 44 vibrations per second); while the highest on record is that of Bastardella who is said to have sung B^{II} (with 1,980 vibrations per second). This gives as the entire range of the human voice about five octaves and a half.

It is worthy of notice that the tones of a good bass voice have at least twenty partials. The quality of a tone depends on the number of partials of which a tone consists, on their relative position, and on their relative degrees of loudness. Simple tones though soft are wanting in power; musical tones with a moderately loud series of the lower upper partials are richer and more harmonious. With only uneven partials the tone is hollow; and when a large number of the upper uneven partials are present *nasal*. High partial tones of considerable strength give to the voice a rough and strident tone. Though the human voice resembles a reed instrument in many respects yet the comparison requires some qualifications.

Our authors next consider the organs of song and speech. In dealing with respiration they touch on the curious difference of breathing in men and women which every one who has ever noticed operatic singers must have recognised,—viz., that with men the action of the midriff (shown by abdominal movement) plays the chief part, while with women the greater part of the work is done by the extension of the ribs sideways (lateral or costal breathing).

The description of the voice-box or larynx is very full and complete. It is illustrated not only by numerous drawings, but by photographs of Mr. Behnke's larynx while he was in the act of singing. As objection might be taken to these photographs on the imagined plea that no one can possibly produce a pure vocal tone with a mirror at the back of his throat, it may be well to note that Mr. Behnke has repeatedly put this matter to the test, showing his larynx to eminent musicians, who agreed that no fault could be found with the quality of his voice under laryngoscopic observation. Our authors state also that in some cases the power of thus producing pure vocal tones under these conditions is present from the beginning, while others acquire it so soon as they become accustomed to the touch of the mirror.

Vocal hygiene occupies as it deserves a most important position in this volume. Diet, exercise, and dress are duly considered. A subject which last year occupied a good deal of space in these columns is touched on in passing. It may be remembered that "A Lady" speaking of the divided skirt and of the effects of a change of dress which if properly made cannot be detected by the keenest observer, said that the power of the voice was greatly increased when that light but warm skirt was worn and stays were no longer necessary. We can hardly wonder when we learn that stays, "even where the charge of *tight*-lacing cannot be made, but the corset is of unyielding materials" diminish the breathing power as tested by the spirometer by one-third. (Illustrative cases were published in KNOWLEDGE). Of course to observations of this kind "An Observer" might oppose any number of old ladies who have survived long years of pinching by which a third of the supply of air nature fitted them to breathe was excluded. But we are here considering respiration only as part of the vocal apparatus, and however clearly it may be shown by "Observers" that women were unwisely provided with half as much lung power again as they really want, vocal requirements—as tested experimentally—are certainly not provided for in excess by a wastefully beneficent Nature.

The later sections on the Reach of the Voice, Daily Life of a Voice User, the Ailments of a Voice User, and Defects of Speech are full of interest and value. We can cordially recommend this work as the most thoughtful and suggestive treatise on the voice known to us. We hope before long to find space for several passages as paragraphs which we had marked for quotation in the course of this review.

* *Voice Song and Speech:* a practical guide for singers and speakers; from the combined view of vocal surgeon and voice trainer. By LENNOX BROWNE, F.R.C.S., and EMIL BEHNKE. 2nd edition, Sampson, Low, Marston, and Rivington: London.

DRAWING THE PLANETS.

BY A FELLOW OF THE ROYAL ASTRONOMICAL SOCI

THE excellent and instructive paper by Prebendary Webb, which appeared on p. 302 of the last volume of KNOWLEDGE, can scarcely fail to have induced a considerable number of our younger students of the heavens to attempt the delineation of some of the details with which the face of the moon is crowded. Almost certainly, its gifted author would find his chief reward in learning that his plain and perspicuous directions had had this effect; ensuring, as this must do, a real and substantial addition to our Selenographical knowledge. The moon, however, is by no means the only body in the sky of which it is important that drawings should be made. Probably, in no way could our knowledge of the physical structure of the planets be more greatly advanced than by the comparison of numerous carefully-executed and accurate sketches of their superficial detail, made at sufficient intervals; and very notably does this apply to the three bodies immediately exterior to the earth: Mars, Jupiter,

and Saturn. It is more especially, then, to facilitate the delineation of these particular planets that the present paper is written. We do not, however, mean here to enter into the question from an artistic point of view; all we propose to do is to instruct the student how to draw the *outlines* of the planets with ease and accuracy; as this always forms a stumbling-block in the way of the beginner. Commencing with Mars, he is, at present, sensibly circular; and subtends an angle of some 17″. Hence we need only take a pair of compasses, and with centre C′ (Fig. 1) and a radius C′A or C′D of half-an-inch,

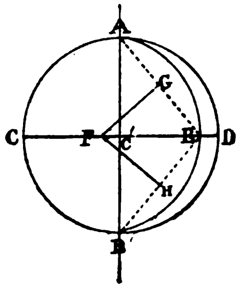


Fig. 1.

describe a circle A C D B—of course 1 in. in diameter—to obtain the outline we require. But Mars is sufficiently near to the earth to exhibit a sensible phase, and when near "quadrature" with the sun is very perceptibly gibbous—or like the moon about a couple of days before or after she is full. Suppose, then, that we wanted to draw the outline of Mars on May 15. Turning to p. 468 of the "Nautical Almanac," we find that only 0·9 of that diameter of the planet passing through the sun is illuminated (this is not a strictly scientific description, but will be better understood than "the versed sine of the illuminated portion of the disc"). Let C D be this diameter, and A B one at right angles to it. Then C E will be the part in light. First, with centre C′ as before, and radius C′D, describe the circle A C B D. Measure off one-tenth of C D to E. Join A E, B E, and bisect A E in G and B E in H; from G draw G F at right angles to A E and from H, H F at right angles to B E. Finally, from F, where these last two lines intersect, and with radius F E or F A, describe the arc A E B. Then will A E B C represent the outline of Mars at our specified period.

If now we try to draw Jupiter as we see it in the telescope, we perceive at once, from its pronounced elliptical outline, that it is impossible to do so, merely by the aid of compasses at all. The equatorial diameter of Jupiter just now approaches 43″, so that, adhering to our original scale, we may represent this in Fig. 2 by *e q*, which we must make equal to 2·4 in. The preface to the "Nautical Almanac" tells us that Jupiter's polar diameter is only ·939 of his equatorial one, so that we take *p a*=2·25 in. Then from the centre *c*, where the two diameters cut each other (of course, at right angles), we take the distance *c e* or *c q* in the compasses, and placing one leg of the compasses on *p* or *a*, move them about until the other leg touches the line *c q* in the points *f* and *f′*. Into these points, technically called the *foci* of the ellipse, we stick two pins, and round them lie a loop of thread of such length that when stretched tight by a pencil, the pencil point shall just touch either *a*, *e*, *p*, or *q*. *f p f′* represents this thread in our figure, and if it be kept tightly stretched as the pencil be carried round, the curve *p e a q* will be the correct elliptical outline of Jupiter to our adopted scale.

The description of the outline of Saturn and his rings only involves a repetition of this process. Its successive steps will be understood by the study of Fig. 3.

As the diameter of Saturn was 17·8″ on January 14 of the present year, we revert to our original scale of 1 inch for the equatorial diameter of the planet. But he is even more elliptical than Jupiter, his polar axis only measuring ·895 of that passing through his equator; so, to begin with, we

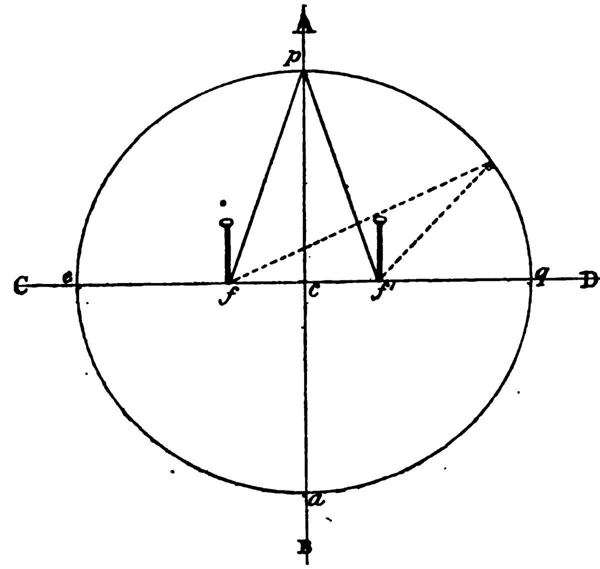


Fig. 2.

have *e q*=1 inch, and *p a*=·895 inch. As before, with one leg of the compasses on *p* or *a*, and with the distance *c e* or *c q*, we find the foci *f f′*, and describe the outline of the ball of the planet. From p. 468 of the "Nautical Almanac" we ascertain that, at the epoch chosen, the outer major axis of the outer ring *o d* was 44·62″. and its outer minor axis *i d′* 19·11″; converting these into linear measure by a rule-of-three sum we find *o d*=2·5 inches,

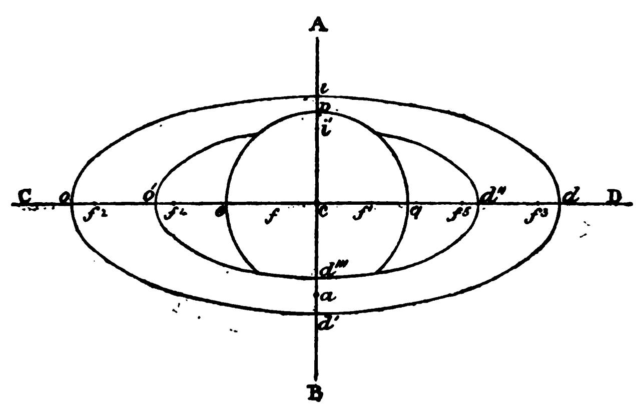
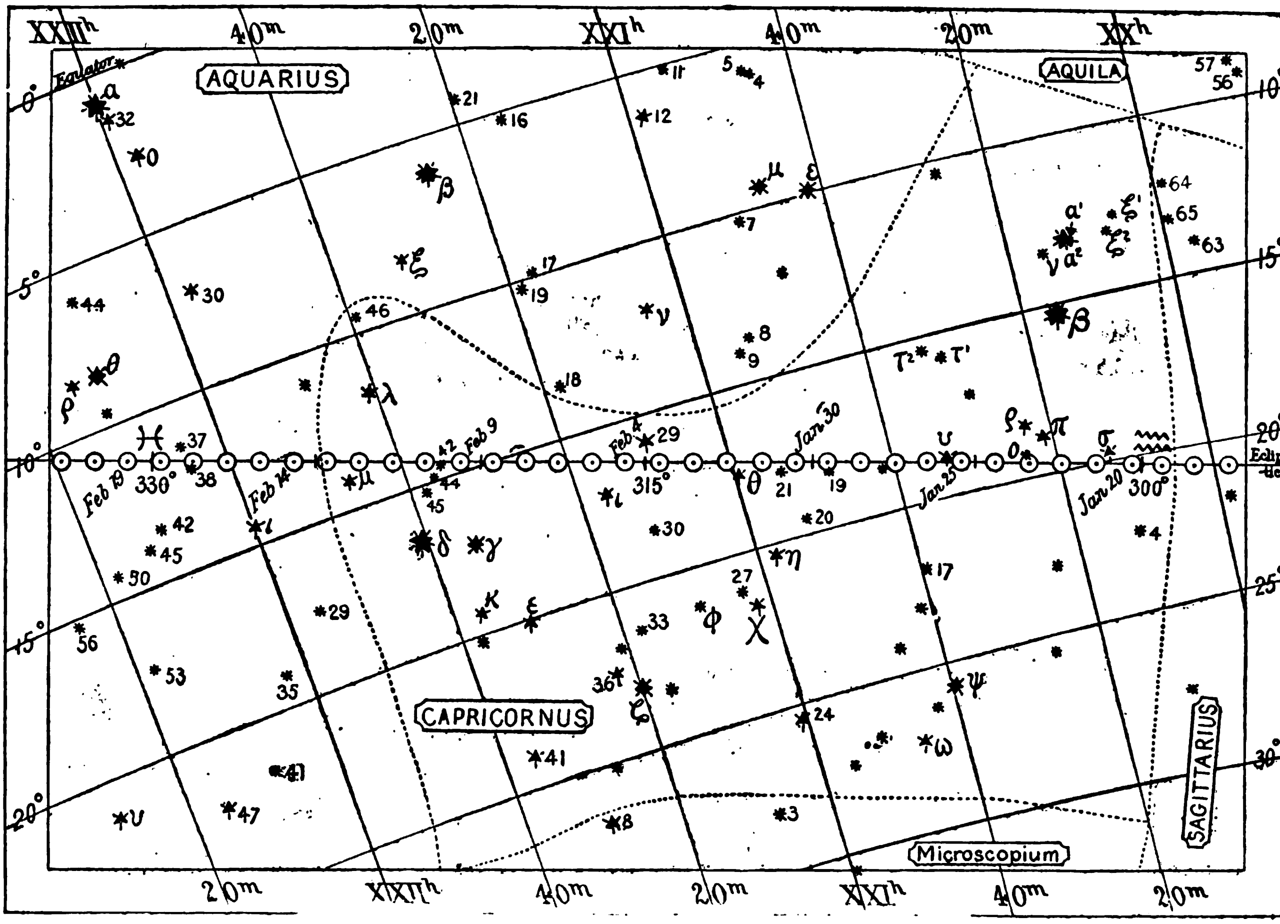


Fig. 3.

and *i d′*=1·07 inch. In like manner we find that the inner major axis of the inner ring *o′ d″* was 29·67″, and its inner minor axis *i′ d‴* 12·71″; quantities which, as before, we turn into 1·66 inch and 0·72 inch respectively. Then, in the manner explained two or three times previously, we find the foci f^2, f^3, and f^4, f^5, insert our pins, and describe the ellipses to which they respectively pertain; the result being shown in our figure.

Possibly by this time the beginner who has followed us so far may feel tempted to exclaim, "Good gracious! am I to go through all these elaborate reductions of angular into linear measurements, these findings of foci, sticking



The Zodiacal Sign for February, with the Sun's path from day to day.

in of pins, carefully tying loops of thread of a rigidly accurate length, and all the rest of it, every time I want to make a drawing of Jupiter or Saturn?" To which we would reply, "Certainly not." The outline of Jupiter is, for our present purpose, absolutely invariable, while those of Mars, and especially those of the Saturnian system, vary so slowly that the outline once drawn will be sufficiently accurate for many weeks. All, then, that the student has to do is to transfer such outline to a sheet of what is called in the shops latten-brass, and, cutting it very carefully out, to thus make a stencil-plate. This is held firmly down on to the paper, and very thick Indian-ink rubbed over and round it with a stencil-brush or tooth-brush with the hairs cut short, the result being a white figure of the planet on a black background. If latten-brass cannot be procured card may be used, but the brass will be found the more satisfactory of the two. Furthermore, for the mere purpose of obtaining an outline, a sharp-pointed pencil may be run round the edges of the stencil-plate, although the Indian-ink will be found much more effective. In this way we have ourselves for some years past prepared outlines of the planets for the purpose of sketching, with results so successful that we unhesitatingly recommend it to all who may care to address themselves seriously to the very interesting task of delineating the detail visible on the surfaces of our nearest neighbours in space.

The delay in the appearance of the papers on the "Planets in a Three-Inch Telescope" has had its origin wholly and solely in the recent extraordinarily bad weather. Every single object referred to in this series of articles has been actually seen, drawn, and described *at the telescope*, and it has been the impossibity of even catching a glimpse of those remaining to be treated of, which has compelled us to defer the concluding essays of the series.

THE DAY SIGN FOR FEBRUARY.

WE give this week the sign of the Zodiac—viz., Aquarius—through which the sun passes in February. It must be noticed that the sun's path as indicated along the ecliptic in this chart, does not correspond to this current year 1884, nor indeed exactly to any year during many last past or to come, but to the month of February in a year supposed to have begun at solar noon on the preceding March 20 with the sun just entering the sign Aries. The sun's place is not quite one day's journey wrong for any year, as represented in this map; but since each year there arises nearly a fourth of a day's difference in the time of the sun's entering Aries or any other sign, no set of maps can show the sun's daily position for all years.

It will be noticed that at this part of the year the sun's path crosses the meridians aslant, but at parts where they lie nearer together than on the equator. The slant of his course would make the sun take a longer time in passing from meridian to meridian than if he crossed them squarely. The greater proximity of the meridians would make him take a shorter time than when near the equator. The two causes of difference about counterbalance each other, so that

so far as they are concerned he would cross the meridians at about the same rate as if he travelled along the equator. But as he is nearer perihelion than aphelion he passes them rather more quickly. After the middle of February, however, he attains his average rate of meridian passage and thereafter falls more and more behind.

MR. SPENCER AND THE *EDINBURGH REVIEW.*

[*To the Editor of* KNOWLEDGE.]

AS I shall not be within reach of Mr. Spencer's "First Principles" for two or three weeks, and as it is not usual, even it the hottest controversy, to charge your adversary with making "garbled extracts," and "omitting several important words" in quotations, without giving people the means of appreciating such charges on the spot, I must ask you to print the whole passage (showing the omitted words) which you charge me with having so "misrepresented"—from his fourth edition, mind, in case there been any change. If I did it was doubly contrary to my intentions—first, to quote fairly, and, secondly, to exhibit his prolixity in full, as I said. Till I see it I can say no more.

I am sorry you find yourself unequal to the task of "parsing" my sentence which you quote, and invent an absurd parallel to "which we should be sure to be told that we have misunderstood, or overlooked qualifying statements somewhere else." Do you think you could manage to parse, "We should be told that we had misunderstood *it*, or overlooked something else"? And what is the grammatical difference?

It is equally good for my argument and bad for Mr. Spencer's, whether Newton's "Axioms or Laws of Motion" depend upon experience or are necessary truths; and I dealt with both alternatives. So I ask with perfect indifference as to that, whether you consider Newton's references to experience thereon real and sufficient proofs, or only illustrations for common people of what he perceived to be necessary truths.

Altogether your review of my review is about the best compliment it could receive from such an ardent Spencerian as you. And as other ardent Spencerians are doubtless satisfied with it, we are pleased all round, and I need ask no more questions.

Feb. 2. THE EDINBURGH REVIEWER OF SPENCER'S "FIRST PRINCIPLES."

[I should be glad to think the "Edinburgh Reviewer" regarded my note on his review as a compliment, though scarcely "the best it could receive." I can assure him that at least one sentence in my note (for my paper is no review) is intended in a complimentary sense. I have seen many attacks on Mr. Spencer, but before his none which saddened me in the reading. The coarse vituperation and vulgar ridicule with which some have assailed Mr. Spencer have excited other feelings than sadness. That such men as *they* should abuse what they have not even attempted to understand, has been but too easily understood. It is otherwise with the "Edinburgh Reviewer." But I say no more on that point.

The word "garbled" was ill-chosen; I should have said, simply, "imperfect." I am certain the reviewer did not intentionally mutilate the passage. That he should have copied the passage imperfectly, read it in MS., corrected it in proof, and yet failed to see that it was imperfect, shows not wilful mutilation, but want of interest in the subject-matter of the passage, and *a fortiori* of the treatise from which that passage was quoted—the only passage he thought fit to quote unreduced. Yet it *was* reduced. I quote (from the same edition) the sentence from which words were dropped, unintentionally no doubt but *very* significantly:—"If x and y are two uniformly connected properties in some outer object, while a and b are the effects they produce in our consciousness; and if, while the property x produces in us the [indifferent mental state a, the property y produces in us the] painful mental state b (answering to a physical injury); then all that is requisite for our guidance is, that x being the uniform accompaniment of y externally, a shall be the uniform accompaniment of b internally." The bracketed words are those omitted. The omission makes nonsense of the passage. Very probably the reviewer can show that his remarks were intended to relate to the complete passage. But it is noteworthy that the properties which he considers in the case of a tiger do not correspond with Mr. Spencer's statement. They are beauty and ferocity, producing the pleasing (not indifferent) sensation of admiration and the painful sensation of physical injury. Moreover the abstract proposition as finally stated is that "when we always find some harmless or pleasant appearance accompanied by some noxious property, we know that we had better keep clear of it w thout further inquiry," which has no real correspondence with Mr. Spencer's proposition. To show this it is only necessary to point out that the latter would begin "If noxious properties *are* avoided," &c.

I am not surprised that the critic of Mr. Spencer's grammar claims accuracy for a sentence which neither I nor he could parse; because the boldly irregular form of the sentence is one of the characteristics of his writing. I rather like it, though it *is* unparsable. I only touched on the point because he sneers at Mr. Spencer's grammar, who occasionally, like himself, trusts in the reader's sense to connect right and not wrong words together. I will content myself by pointing out that in the sentence, as it stands in the *Edinburgh Review*, there is only the context to show that "overlooked" belongs to "we should be sure to be told that we have" and that the "which" on the other side, though it is to be connected with "we have misunderstood" is not to be connected with "we have overlooked." Once you put "it" after "misunderstood", all is right: it belongs *there* to "misunderstood" and not to "overlooked;" but where the reviewer puts its relative "which," it belongs to both "misunderstood" and "overlooked,"—grammatically to one, quite ungrammatically to the other.

I did not touch on the reviewer's argument respecting the laws of motion, only on his statement that Newton advanced them as necessary truths, whereas Newton regarded them as established by Induction,—or, as Whewell says in speaking of the first and simplest, "Men culled the abstract rule out of the concrete experiment."

If my notice of his review pleases the reviewer it is well; yet not nearly so well as it would be if he had seen that he has thoroughly misapprehended the teaching of the philosopher he has attacked. That I am not so ardent a Spencerian but that I can examine Mr. Spencer's propositions dispassionately, and criticise them as keenly as if they came from one whose general teachings I disliked, I have shown in my article here on "The Family of Small Planets." That the usually keen and clear-seeing mind of the "Edinburgh Reviewer" is not so dispassionate is shown throughout his review. If I could here find space to review instead of merely noticing that review, I could make this abundantly clear. I have cited but one really important instance, his

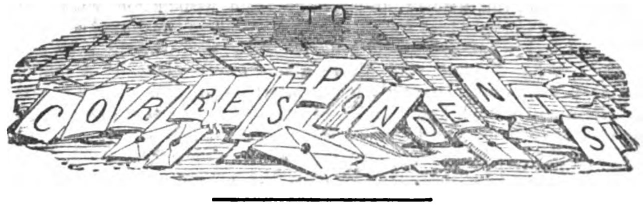
travestie of Mr. Spencer's remarks about a First Cause. Here is another, of many such :—Mr. Spencer has shown wherein all forms of religion agree, viz., in "the consciousness of an Inscrutable Power manifested to us through all phenomena." His reviewer finds that in just this respect religions have differed widely,—as though the thought did not underlie belief in many gods as certainly as it underlies faith in but One Supreme Power. On the other hand, he says that all religions have agreed in just that one point where Mr. Spencer says they have differed, namely, in professing to know the ways and attributes of the several deities or of the single Supreme Being they have acknowledged. I rather wonder what a lawyer would say if he were told that he must not question the statements of witnesses who gave irreconcilable accounts of some event, seeing that although no two of them agreed they were all at one in claiming exact knowledge of those very matters about which they made contradictory statements.

Throughout, the review is unfair in this way,—not wilfully I am sure; but the writer simply cannot see a single pleasing tint through his sad-coloured spectacles, and unfortunately he *will* wear them. That in dealing with views thus distasteful to him he should find dulness and prolixity where others find only sufficient fulness of elucidation is altogether natural.

As perhaps the best way of answering the sarcasms of the *Edinburgh Review* I propose in the next number of KNOWLEDGE to illustrate, by an extract from "First Principles," the calmer and more dignified tone of the philosopher attacked. The extract, already in type, is entitled "Patience and Courage for the Truth." R. P.

THE Edison Company is said to have 22,500 lamps in use on the Continent, of which 7,000 are in Germany, 4,500 in France, 4,000 in Austro-Hungary, 3,000 in Russia, and 2,200 in Italy.

MORNING GLOW.—Professor Guy, of Saint Louis, Perpignan, France, writes to *Cosmos Les Mondes* that at 4 a.m. on the 8th ult., on getting out of bed, and before lighting the candle, he saw the reflection of a flash of lightning in his room. He ran to the window and saw a beautiful whitish glow extending over the whole of the southern part of the sky, from the horizon to the zenith. His window, he says, looked south, and he could see nothing of the northern part of the sky. It being 4 a.m., and the moon having set at Perpignan at 2 hours 42 minutes, Professor Guy says that what he saw could not possibly have been clouds lighted up by the moon. Along the southern horizon was a band of black clouds of about 15° in height. The upper part of this band was illuminated at intervals by flashes of lightning like those seen in summer. This intermittent illumination was undoubtedly, says the professor, due to lightning, but the cause of the general glow, which lasted till a quarter to five, when the sky became clouded over, is not so easy to determine, though it may be attributed to electricity. Possibly the upper currents of electricity flowing towards the poles are able to illuminate the layers of rarefied air which they traverse, and to give us the feeble whitish glows which are sometimes seen. Such was probably the cause of the effect seen at Perpignan, and the professor thinks that the frequent flashes of lightning which illuminated the horizon showed that there was a great deal of electricity in the air, and gives some semblance of truth to his supposition.



"Let Knowledge grow from mere to more."—ALFRED TENNYSON.

Only a small proportion of Letters received can possibly be inserted. Correspondents must [illegible] *offended, therefore, should their letters not appear.*

All Editorial communications should be addressed to the EDITOR OF KNOWLEDGE; *all Business communications to the* PUBLISHERS, *at the Office, 74, Great Queen-street, W.C.* IF THIS IS NOT ATTENDED TO DELAYS ARISE FOR WHICH THE EDITOR IS NOT RESPONSIBLE.

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THE FELLOWSHIP OF THE LEARNED SOCIETIES.

[1110]—There is probably no single subject on which the genera public is so misinformed and ignorant, or in connection with which so much popular misapprehension exists, as that of the value and significance of the letters which indicate the membership or fellowship of the so-called "learned societies"; and which are found appended to the names of so many merely notorious as well as really eminent men; to say nothing of those of the crowd of nobodies who claim the right to use them. There can be no greater mistake than to imagine that, because a man puts the letters F.R.S., F.R.A.S.; F.G.S., F.L.S., F.R.G.S., F.S.A., &c., &c., &c., after his name, he has, merely on that account, any more claim to be listened to than the veriest penny-a-liner or contributor to a "halfpenny dreadful." For it can never be too widely known that while every competitor for the humblest post in the public service has to submit to a preliminary educational test of fitness, no examination of any sort or description is exacted from the aspirant to the fellowship of a single one of the associations of which I am speaking. All that is needful in the majority of cases is that the candidate shall have his nomination paper signed by three or more Fellows of the Society he wishes to enter; and that *nothing whatever, good, bad, or indifferent, shall be known about him.* I am careful to emphasize this last condition, inasmuch as the government of the societies, one and all, has drifted more or less into the hands of cliques, who will take especial care to exclude any man personally unpopular with their own members, be his scientific attainments what they may; while, on the other hand, a man may have publicly paraded such crass and extraordinary ignorance of that branch of science for whose promotion a particular society was founded and chartered, that the consensus of the whole of the Fellows as to his manifest unfitness must insure his rejection. I repeat, then, that the person who may cherish the most reasonable hope of writing any given combination of letters after his name is he of whom the members of the society he delights to honour are in previous ignorance.

Viewed in this aspect, the condition of every one of the learned societies is, in one sense, deplorable. Two of them—the Royal Astronomical and the Royal Geographical Societies—seem especially singled out by a certain class of advertising schoolmasters and tradesmen as a means of advancing their respective businesses. These gentry get elected (heaven only knows how, in many cases, they find people even to nominate them!), and forthwith the advertisement columns of the newspapers announce chronically that "The Salisbury Plain Universal Educational Institute is conducted by T. Brown, Ph.D., F.R.A.S.," or that "The Soudan Portable Cooking Apparatus can be obtained only from J. Robinson, F.R.G.S., &c." The odds are heavy against "Dr." Brown knowing altitude from aberration, or North Polar distance from azimuth; while it is more than likely that Robinson could not say off-hand where Tiflis is, even to save his soul alive. To the outside public however, the half-educated pedagogue and the puffing shopkeeper, seem in some sort to rank with an Airy or a Markham. But, it may be imagined that, granting that this is true, surely the *fons et origo* of all the Scientific Societies, the oldest of them all, the Royal Society, stands in a different category from its so comparatively recent progeny, and the Fellowship of that Society which has numbered Newton, Wren, Davy, Faraday, Wollaston, and Young among its members, must confer honour and afford testimony

of scientific status, utterly underivable from association with the more special ones. Alas! for this delusion. What the Royal Society once was and what it now *is* are two very very different things indeed. What I have said above about government by a clique applies perhaps more strongly in its case than in almost any other. At present it is "worked" and exploitéd by a compact association for their own benefit and that of their friends; and the chances of election of any given candidate depend very largely indeed upon his capacity and inclination to go with the stream and make himself useful to the wire-pullers. Let anyone study the lists of elected candidates during the last few years and note the number of obscure doctors, dummy astronomers, pseudo-chemists, and the like, who now dub themselves F.R.S., and a better idea will be gained of the value of this "distinction" than can be derived from any iteration of what I have previously said, either in the way of illustration or application. A man must have a distinct taste for some branch of science, be "a good fellow," and be orthodox in the views of the dominant clique in the matter of assaults on the public purse. Possessed of these qualifications he need not be either an Airy, an Owen, a Crookes, or a Geikie; the mere fact of his having them, in conjunction with the capability of paying his entrance-fee and subscription, being sufficient to insure his right to employ the coveted letters, and to go on his way rejoicing. N.W.

[There can be no doubt that our correspondent's remarks are in great degree justified by facts, though they are somewhat too sweeping. Undoubtedly a large number of those who get themselves nominated and elected to learned societies have no other object but to improve their own position; and quite a large proportion of the members of some societies, even of highest repute, have scarcely any knowledge in the subject or subjects for whose advancement the society was founded. The former trouble is most mischievous when the wish to work a society involves the direct influence of the society itself, and not such indirect influence as is derivable from the society's name. For, those interested in advancing their own interests will always work more steadily and zealously, and in the end more successfully than those (more in number it may be hoped) who object to jobbing. A concrete object has more power than an abstract objection. It is noteworthy, too, how the introduction of these personal objects interrupts the good work of a learned society. For example some twelve years have passed since Col. Strange first tried to use the influence of the Royal Astronomical Society to advance his scheme for the endowment of research—a piece of mere jobbery as then presented, though before and since, on abstract principles the endowment of research has been supported by honest but simple-minded men: now I think I am within the mark in saying that in the last ten years not ten papers published by that society have been noticed outside the society's rooms. Nearly all the good work which the members of the society have had to do they have brought otherwise before the public than in the old-fashioned and excellent way which was in vogue before that unfortunate business began. The misuse of the influence of the grand old Royal Society is an older matter, as De Morgan long since told the world,—though this evil has I believe increased of late years. I have very little knowledge in that direction, however, my experience in the Royal Astronomical Society, a dozen years ago, making me determine never to become a candidate, or allow myself to be nominated for election, in any society whatsoever. I made an exception recently in the case of that "infant Hercules" as Dr. Huggins has called it the Liverpool Astronomical Society, which has done me the honour to elect me an associate, in company with my esteemed friend that excellent astronomer Captain W. Noble, and the zealous and skilful M. Terby, who has done so much to extend our knowledge of the planet Mars. But except in this case I have not (of my own free will) consented to be nominated for any society since the time when, in 1866, Dr. Huggins invited me to become a Fellow of the Royal Astronomical Society.* The reasons I have had are closely akin to those suggested by my friend, the writer of the above letter. Undoubtedly the matter to which he has directed attention is one of somewhat serious import. —Richard A. Proctor.

* In strict accuracy I may mention that I have been elected to other societies, as to several in America; also, somewhat to my surprise (seeing that I had already declined an invitation to be nominated), to the St. George's Chess Club; but in the last-named case I considered the approach of the International Tournament (a rather costly matter, I suppose) had something to do with the unexpected compliment; and therefore, on election, I cheerfully paid entrance-fee and a year's subscriptions, and (after a few months' interval to save appearances) withdrew, without as yet knowing exactly where the rooms of the club may be, except that they are somewhere in Albemarle-street.

EXTRAORDINARILY LOW BAROMETER.

[1111]—I possess one of the old-fashioned "clock-face" barometers, and on Saturday evening last, January 26, it performed a feat which I think is worthy of record and may, perhaps, interest some of your meteorological readers.

The face is, of course, marked from 28 in. (stormy), near the bottom on the left hand side, round the top through 29 in. (rain), 30 in. (fair), and so on down to 31 in. (very dry), near the bottom on the right hand side; but at the extreme bottom of the face, between the figures 28 and 31, there is a blank space, entirely unmarked. Well, all day on Saturday, the finger dropped rapidly down the left-hand side (the falling side) until, about 3 o'clock in the afternoon it had passed below the lowest figure (28 in.), and continued its career along the blank space towards the extreme bottom of the face, which point it passed about five o'clock, and still continued its course towards the right, until about ten o'clock, p.m., when it had nearly traversed the whole of the blank space, finally stopping at a point very near to the figure 31 in. (Very Dry). Here it stood for about a couple of hours, and then began slowly to retrace its "steps." By calculating the blank space according to the markings on the other parts of the face, I found that the extreme point reached by the finger would be about 27·55 in., indicating a terrible fall of the mercury.

I never in my life knew my barometer to act in such a manner before, and I might have thought there was something wrong with it if a friend and neighbour had not informed me that his barometer—one of similar description—performed exactly the same extraordinary feat.

The weather prevailing at the time was certainly extraordinary for the time of the year, for during the latter part of the afternoon there was a strong gale blowing, accompanied by heavy rain, and between 5.30 and 6 o'clock there were several very vivid flashes of lightning and moderately-loud peals of thunder, soon after which the wind became comparatively calm for an hour or two, and then rose again to a terrible gale, which continued all night, with rain, hail, and snow. On looking at the barometer next morning I found the finger had worked its way off the blank space again just on to the marked part, registering a fraction above 28 inches, and still slowly rising, the gale continuing. Excelsior.

AFTER-GLOW IN AMERICA.

[1112]—In reply to the request in a note to the article on the extraordinary sunsets in Knowledge of December 7th, which has only just reached me, I beg to inform you that on three successive evenings towards the close of November—I forget the exact dates—the after-glow at this place was very remarkable. I did not notice anything extraordinary at the time of sunset, which was a little after half-past four, but from about half-past five to quarter past six the whole western sky was lit up by a very vivid crimson light, rising very nearly to the zenith.

I described it in my home letters, but was not aware until English papers reached us that the same phenomenon had attracted notice over the greater part of the world.

I have observed no discolouration of the sun either at sunrise or sunset, and I believe no such effect has been observed in these parts.

On the morning of Dec. 18 I happened to be up at quarter-past six. Looking out of an east window I saw all the effects of sunrise over the south-eastern horizon. The colour was amber shading into orange, and the clouds were tipped with an orange-red. At a few minutes before seven I left my house, and saw the spires of the churches and the roofs of the higher houses gilded so strongly with sunlight that I fancied my watch must have deceived me and turned round to the eastward several times to see if the sun had actually risen.

From seven to half-past seven, or a little later, I was in a building. Coming out I looked to the east again, and saw the ordinary effects of a rather dull sunrise. The sun actually rose a few minutes later.

Last night again (26th) there was a remarkable after-glow, lasting from about quarter-past five to near six. It was at its brightest at about half-past five, and was of an orange-red colour, different to the crimson in November.

Halifax, Nova Scotia, *Dec.* 27. Chas. S. Akers, Col. R.E.

THE HEIGHTS ABOVE—AND THE DEPTHS BELOW.

[1113]—A remarkable illustration of human degradation reaches me from the other side of St. George's Channel. An Irish gentleman of station and fortune recently undertook, in concert with some of his English brethren, a series of observations of variable stars. He has, perforce, been compelled to relinquish his share of the task for the simple, but sufficient, reason that he dares not

cross his own grounds at night to proceed to his observatory, for fear of being murdered. This gentleman—whose name, for obvious reasons, I suppress—is resident on his own estate, and actually offered all his tenants leases in perpetuity three years before the Land Act was even heard of. Hence, it is the mere fact of his being a landlord at all which imperils his life. During all the ghastly horrors of the French Revolution, when the guillotine was perennially sodden with the life-blood of some of the best and wisest Frenchmen, Lalande was suffered to pursue his astronomical studies in peace; and, when the death of Robespierre enabled innocent men and women to breathe freely again, "thanked his stars" for his escape. It seems to have been reserved for the pitiful curs who fire through windows at night at defenceless women and children; who maim helpless cattle; who slink behind stone fences to shoot their unsuspecting victims; but who run like hares at the mere sight of a policeman; it seems, I say, to have been reserved for vermin like this to lie in wait for the life of a man going forth to study the wonders and beauties of those heavens, whose very canopy they pollute by crawling under it. ANTI MURDER LEAGUE.

SOLUTION OF CRIBBAGE PROBLEM.

[1114]—Dealer: Two sixes and two sevens. Opponent: The same. Crib: Three fives and Knave of Clubs. Turn-up Card: Five of Clubs.

The Play.			Dealer's Score.
Opponent ... 7	Dealer ... 7	pair	2
„ ... 7	„ ... 7	and go	13
„ ... 6	„ ... 6	pair	2
„ ... 6	„ ... 6	and last card	13
		Play	30
		Hand	16
		Crib	29
		Answer	75

H. H. H.

MIGRATION OF BIRDS.

[1115]—Mr. Grant Allen's contribution to a "partial elucidation" of the so-called migratory instinct of birds, KNOWLEDGE, Jan. 4, is interesting, but "the modus operandi of migration" is surely connected with the instinct of locality in general. Savages possess the latter well developed, and some among ourselves have personal evidence of the presence of its germ. The writer, for instance, commonly knows in what direction he is going, when travelling in a railway carriage or on board a steamer, by a sort of half conscious intuition, but occasionally feels a sensation of being carried in a direction opposite to the real, so that if journeying west the illusion that the north is on his left is not to be shaken off. This illusive sensation is commonly first felt by him when his back is turned towards the direction in which he is being carried, but once formed, no effort and no change of position has the slightest effect upon it, and it is to him unaccountable unless a reminiscence or survival of a far more perfect sense of local direction possessed by remote ancestors. Do certain animals, the carrier pigeon, for instance, possess a sensation of locality similar to this in kind, but which has the infallibility of the mariner's compass, and may conceivably be connected with the magnetic current? He has been led to suppose that the *instinct of locality* may be at bottom a sense of direction, by the observation of an old Australian squatter, that a dog carried away from home by train was noticed by him never to lose sense of direction, but when taken on to the platform at several stations, to pull a person leading it either straight back along the line or at a considerable angle from it, but always directly towards its old home.

J. C. MURRAY AYNSLEY.

LETTERS RECEIVED (SUB-EDITOR).

J. E.—E. R. M.—J. SINCLAIR.—ARTHUR STANLEY. The notice appeared as sent by the person chiefly interested; why not blow *him* up?—J. W. See KNOWLEDGE, page 58.—ALPHARD. *No such comet expected.*—J. R. FERRER. Your paper, "strictly for publication," cannot be published here, nor can reply be sent either by mail, as you wish, or by post-card, which you deprecate. We cannot however keep from our readers the pleasing fact you have established by mathematical demonstration, that while every known cause = 1, the ultimate first cause = 2·718281828 ..... *Now* we understand what had so long seemed infinitely mysterious.—W. CORDWILL. The Editor has been prevented by illness from carrying out the further observations referred to.

Our Whist Column.

BY "FIVE OF CLUBS."

THE following game is from the *Westminster Papers*. It shows how a game which seems lost, may be saved by care to the last in placing the leads:—

THE HANDS.

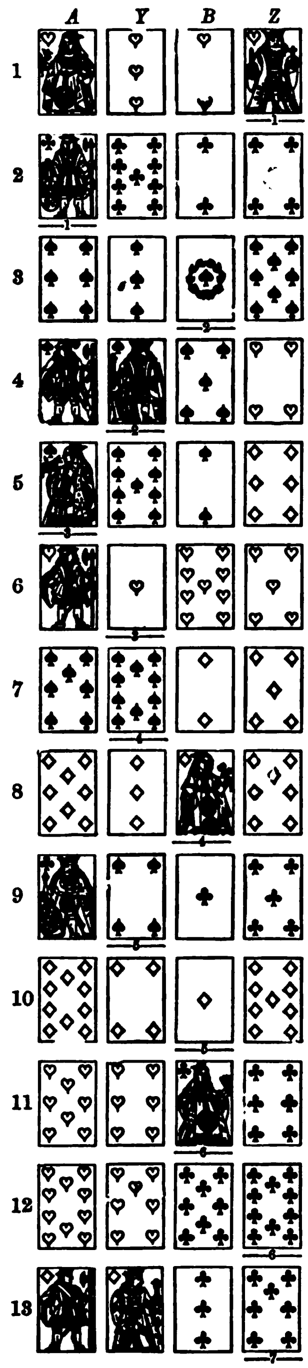
Y.
Spades—K, 10, 9, 4, 3.
Hearts—A, 7, 6, 3.
Clubs—9.
Diamonds—K, 4, 3.

B.
Spades—A, 5, 2.
Hearts—9, 2.
Clubs—A, Q, 8, 3, 2.
Diamonds—A, Q, 2.

Y B
Score—
Four all.
Dealer.
A Z
Trump S. 8.

A.
Spades—Q, Kn, 7, 6.
Hearts—Q, Kn, 10, 8.
Clubs—K, Kn.
Diamonds—Kn, 10, 8.

Z.
Spades—8.
Hearts—K, 5, 4.
Clubs—10, 7, 6, 5, 4.
Diamonds—9, 7, 6, 5.



NOTES AND INFERENCES.

1. *A* has Knave, Ten, another, almost certain. *Y* passes, having length in the suit and five trumps.

2. *Z* leads lowest of his five-card suit. This was before the "penultimate" signal was invented. *Z* sees that *Y* has no more Clubs; *A-B* know that he can hold none unless it be the Ten.

3. *A* leads trumps, because the score being at four all it is important to prevent the enemy from ruffing freely, as seems threatened.

4. *B*, of course, returns the highest of two, so that

5. *A* now finds *Y* with two trumps left. He also finds he has played the adversaries' game.

6, 7, and 8. The play here is simple enough. *Y* draws *A*'s last trump, and leads what must be *B*'s suit. It is the only chance of getting in *Z*'s Clubs. The game at this point looks bad for *Y-Z*. The one bit of blue sky is the certainty that neither *B* nor *Z* can have a Heart. If *A* can get a lead after *Y*'s long trump has been forced, the game is lost.

9. The King falls, and *Y*'s hopes are strengthened.

10. He throws the lead again into *B*'s hand, and as

11. *Z* luckily holds the minor tenace in Clubs

12, 13. *Z* makes the last two tricks and *Y-Z* win.

WHIST ENDINGS, p. 62.—We have received several letters asking whether *A-B* (especially in the second ending) can possibly win all the tricks against any play—or only if *Y-Z* play in specified ways. We reply that these endings are, as in all such cases, precisely like chess problems. A line is in each case to be found by which *A-B* can win, no matter how *Y-Z* may play. We give next week the solutions and the names of the successful solvers.

AMBR. PERRY.—You should certainly have passed the trick. It was clearly fatal to you to have to lead. True the odds were at least 20 to 1 that you lost anyway. But 1/21 is many million times 0.

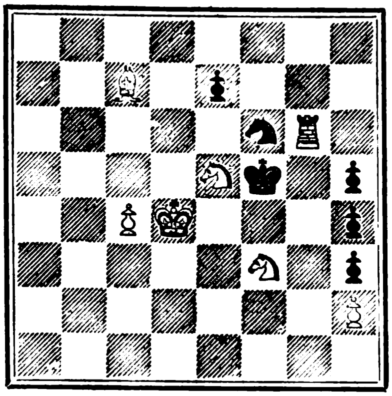
Our Chess Column.

By Mephisto.

PROBLEM No. 114.

By F. J. D.

Black.



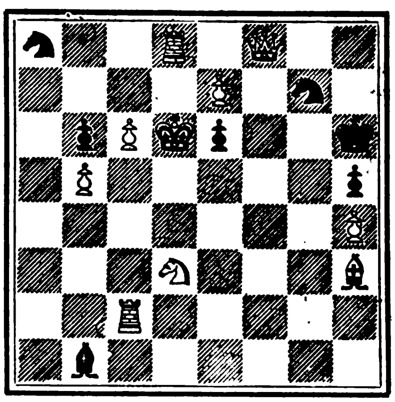
White.

White to play and mate in three moves.

SUI-MATE PROBLEM,

By H. W. Sherrard.

Black.



White.

White to play and sui-mate in six moves.

Solution.

1. Q to R8 (ch)	1. K to Kt3
2. Kt to K5 (ch)	2. K to B3
3. R to B2 (ch)	3. B to B4
4. R to Q7	4. Kt to B2
5. P to K8 (Kt) (ch)	5. Kt takes Kt (ch)
6. Q takes Kt	6. Kt takes Q (ch)
Mated.	

SOLUTIONS.

Problem by E. N. Frankenstein, p. 62.

1. B to Kt2	P to Kt4		
2. Kt to Kt3	P to Kt3, or		P takes Kt
3. Q to B7	Any	P to B4	Any
4. Q or Kt mates		Q or B mates	

(a)	1. R to K2
2. Kt to Kt3	2. B moves
3. B or Q takes B (ch)	3. R moves
4. Q or Kt mates	

Problem by the Rev. H. Bolton, p. 62.

1. Q takes R	B to Kt3	5. K to R2	B to Kt7
2. Kt to Kt4	R to B2	6. B to B6	B moves
3. Q tks Q (ch)	R takes Q	7. B tks R, mate	
4. Kt to R6	B to K5 (ch)		

Sui-mate Problem, by By W. A. Shinkmann, p. 62.

Position.—Black.—Q, QR5; K, KB5. White.—Q, QB5; K, KR4; R, K2; B, KKt4; P, KKt2.

The few pieces employed, together with the unrestricted liberty of movement of the Black Queen, render this problem one of the best of its kind. The principle is that the Queen is caught everywhere, as follows:—

1. B to R3	Q to R sq, K sq, B3, or 7	Or (a)	Q to R4, 6
2. Q to Q4 (ch)	Q to K5	Q to Kt4 (ch)	Q takes Q
3. Q to B6 (ch)	Q to B4	P to Kt3 (ch)	K to B6 (ch)
4. Q to Kt5 (ch) Mated	Q takes Q (ch)	B to Kt4 (ch) Mated	Q takes B (ch)
1. (b)	Q to R3, 7, Kt4, or 6	(c)	Q to R2, 8, Q2, or 8
2. Q to B4 (ch)	Q takes Q	Q to Q4 (ch)	Q takes Q
3. P (ch)	K (ch)	P (ch)	K (ch)
4. B (ch)	Q mates	B (ch)	Q mates

MR. R. A. PROCTOR'S COURSE OF LECTURES.

1. LIFE OF WORLDS.
2. THE SUN.
3. THE MOON.
4. THE PLANETS.
5. COMETS.
6. THE STAR DEPTHS.

See Advt. Pages for full Syllabus.

The following arrangements are complete: the numbers in brackets referring to above list.

BRISTOL (Colston Hall), Feb. 19, 22, 26, 29; March 4, 7 (the full course).

CHELTENHAM (Assembly Rooms), Feb. 8, 12, 15 (2, 4, 6). At 3 o'clock, Feb. 12 (3, 5).

BATH (Assembly Rooms). Morning Lectures at 3 o'clock, Feb. 9, 13, 16 (3, 4, 6); Evening, Feb. 13 (5).

GLOUCESTER (Corn Exchange), Feb. 20, 21, 27, 28 (1, 2, 3, 4).

TAUNTON (Victoria Rooms), March 5, 6 (1, 2).

SURBITON, Feb. 18, 25, March 3.

BIRKENHEAD, March 10.

ALTRINCHAM, March 11 (5).

CHESTER, March 12, 13 (1, 2).

HAVERSTOCK-HILL, March 14 (2).

REIGATE, March 19 (2).

HITCHIN, March 20 (5).

UXBRIDGE, March 21 (1).

LONDON (Brixton Hall) March 28, April 1, 4 (1, 2, 3).

„ (Memorial Hall), March 24, 27, 31, April 3 (1, 2, 3, 4).

Note.—*All communications respecting Lectures should be addressed to Mr. John Stuart, Royal Concert Hall, St. Leonards.*

Contents of No. 118.

SPECIAL NOTICES.

Volume IV., comprising the numbers published from July to December, 1883, is now ready, price 7s. 6d.; including parcels postage, 8s.

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PLAINLY WORDED - EXACTLY DESCRIBED

LONDON: FRIDAY, FEB. 15, 1884.

CONTENTS OF No. 120.

PATIENCE AND COURAGE FOR THE TRUTH.*

BY HERBERT SPENCER.

THE spirit of toleration which is so marked a characteristic of modern times, and is daily growing more conspicuous, has a far deeper meaning than is supposed. What we commonly regard simply as a due respect for the right of private judgment, is really a necessary condition to the balancing of the progressive and conservative tendencies—is a means of maintaining the adaptation between men's beliefs and their natures. It is therefore a spirit to be fostered; and it is a spirit which the catholic thinker, who perceives the functions of these various conflicting creeds, should above all other men display.

To this end let him ever bear in mind three cardinal facts.

The first is the existence of a fundamental verity—under *all* forms of religion, however degraded. In each of them there is a soul of truth. Through the gross body of dogmas, traditions, and rites which contain it, it is always visible—dimly or clearly as the case may be. This it is which gives vitality even to the rudest creed; this it is which survives every modification; and this it is which we must not forget when condemning the forms under which it is presented.

The second of these cardinal facts is that while these concrete elements in which each creed embodies this soul of truth, are bad as measured by an absolute standard, they are good as measured by a relative standard. Though from higher perceptions they hide the abstract verity within them; yet to lower perceptions they render this verity more appreciable than it would otherwise be. They serve to make real and influential over men, that which would else be unreal and uninfluential. Or we may call them the protective envelopes without which the contained truth would die.

The remaining cardinal fact is that these various beliefs are parts of the constituted order of things; and not accidental but necessary parts. Seeing how one or other of them is everywhere present; is of perennial growth; and when cut down redevelops in a form but slightly modified, we cannot avoid the inference that they are needful accompaniments of human life, severally fitted to the societies in which they are indigenous. From the highest point of view we must recognise them as elements in that great evolution of which the beginning and end are beyond our knowledge and conception—as modes of manifestation of the Unknowable; and as having this for their warrant.

Our toleration should therefore be the widest possible. Or rather we should aim at something beyond toleration, as commonly understood. In dealing with alien beliefs, our endeavour must be, not simply to refrain from injustice of word or deed, but also to do justice by an open recognition of positive worth. We must qualify our disagreement with as much as may be of sympathy.

But, although existing religious ideas and institutions have an average adaptation to the characters of the people who live under them; yet as these characters are ever changing the adaptation is ever becoming imperfect; and the ideas and institutions need remodelling with a frequency proportionate to the rapidity of the change. Hence, while it is requisite that free play should be given to conservative thought and action, progressive thought and action must also have free play. Without the agency of both there cannot be those continual re-adaptions which orderly progress requires.

Whoever hesitates to utter that which he thinks the highest truth, lest it should be too much in advance of the time, may reassure himself by looking at his acts from an impersonal point of view. Let him duly realise the fact that opinion is the agency through which character adapts external arrangements to itself—that his opinion rightly forms part of this agency—is a unit of force, constituting with other such units the general power which works out social changes; and he will perceive that he may properly give full utterance to his innermost conviction; leaving it to produce what effect it may. It is not for nothing that he has in him these sympathies with some principles and repugnance to others. He, with all his capacities and aspirations and beliefs, is not an accident, but a product of the time. He must remember that while he is a descendant of the past, he is a parent of the future; and that his thoughts are as children born to him, which he may not carelessly let die. He, like every other man, may properly consider himself as one of the myriad agencies through whom works the Unknown Cause, and when the Unknown Cause produces in him a certain belief, he is thereby authorised to profess and act out that belief. For, to render in their highest sense the words of the poet:—

> Nature is made better by no mean,
> But nature makes that mean: over that art
> Which you say adds to nature, is an art
> That nature makes.

Not as adventitious, therefore, will the wise man regard the faith which is in him. The lightest truth he sees he will fearlessly utter;* knowing that, let what may come of it, he is thus playing his right part in the world—knowing that if he can effect the change he aims at—well: if not—well also; though not *so* well.

* From "First Principles," Part I.

* How admirably the poet philosopher of America presents the spirit of as fearless inquiry which leads to the recognition of new aspects of eternal truths,—

This should be, he says, our

> —— homage to the mightier powers,
> To ask our boldest question, undismayed
> By muttered threats that some hysteric sense
> Of wrong or insult will convulse the throne
> Where Wisdom reigns supreme.

Far back in the days of old we see the same reverent daring in the grand utterances of the Book of Job,—I say *reverent* daring, for here *doubt*, not daring, means irreverence, even as the tremors of a suppliant for justice are an insult to the righteous judge.—R. P.

GHOSTS AND GOBLINS.

By Richard A. Proctor.

Another circumstance which seems to have considerable effect in preparing the mind to entertain superstitious emotions is intense or long-continued brooding on sorrows, and especially on the loss of one dear to us. Mingled with our thoughts at such times, the idea is always more or less consciously entertained that our lately-lost friend is near to us and knows our thoughts. The reason may be convinced

> No spirit ever brake the band
> That stays him from his native land,
> Where first he walk'd when claspt in clay:

while nevertheless something within us teaches (wrongly or rightly, who knows?) that the spirit itself

> May come
> When all the nerve of sense is numb,
> Spirit to spirit, ghost to ghost.

Surely it is not the weak and ignorant alone who have this experience. The mind of strongest mould need not be ashamed to have entertained the thought, to have even prayed the prayer,—

> Descend, and touch, and enter; hear
> The wish, too strong for words to name,
> That in this blindness of the frame
> My Ghost may feel that thine is near.

Under the influence of emotions such as these the mind is prepared to be deceived. It is at such times that visions of the departed have been seen. I do not here speak of visions called up out of nothing—the healthy mind cannot be so far betrayed—but of visions none the less imaginary. The mind has no creative power to *form* such visions, except when there is diseased and abnormal action; but it possesses a power to combine real objects so as to form pictures of the unreal, and this power is singularly active in the time of sorrowing for a near and dear friend.

It is probable that the experience of every reader of these lines will supply instances in point. Sometimes the deception of the mind is singularly complete, insomuch that it is only by the determination to approach the seeming vision that the ghost-seer is able to remove the impression. I will cite an instance which occurred to myself, as somewhat aptly illustrating the principal circumstances tending to make such illusions effective:—

My mother died during the long vacation of my first year at Cambridge. It chanced that I was in Germany at the time, and I suffered much distress of mind from the thought that I had been enjoying a pleasure-tour during the days of her last illness. Letters had followed me from place to place, but it was only the circumstance of my staying my journey one Sunday at Heidelberg which enabled me to receive news from England; and I only reached home in time to attend her funeral. Yet the full effect of these circumstances was only experienced when I found myself again settled in my rooms at Cambridge. There is a singular mixture of society and solitude in university life, which at times of trouble produces unpleasant feelings. Throughout the day there is abundant opportunity for intercourse with friends; but although amongst one's college friends are some who will be friends for life, yet at the time the interchange of ideas even with these special friends relates almost wholly to college work or college interests. There is nothing homelike in social arrangements at college. So soon as the "oak is sported" for the evening a lonely feeling is apt to come on, which affects even some of those who have no recent sorrows to brood over. There is a refuge in hard reading. But hard reading, in my case, had come to an end on my mother's death. I had so far accustomed myself to associate college successes with the idea of pleasure given to her that I now looked with aversion on my former studies. They could no longer gain the prize I had alone cared for. I ought, no doubt, to have had quite other feelings, but I speak of the effects I actually experienced. Now, whether the breaking up of my old plans for work had upset me, or in whatever way it happened, I certainly had never found college life so lonely and unpleasant as during the first term of my second year. And it seems to me likely that the low spirits from which I then suffered may have had something to do with the singular instance of self-deception I have now to relate:—

I had on one evening been particularly, I may say unreasonably, low-spirited. I had sat brooding for hours over dismal thoughts. These thoughts had followed me to bed, and I went to sleep still under their influence. I cannot remember my dreams—I did dream, and my dreams were melancholy—but although I had a perfectly clear remembrance of their tenour on first waking,* they had passed altogether from my recollection the next morning. It is to be noted, however, that I was under the influence of sorrowful dreams when I awoke. At this time the light of a waning moon was shining into the room. I opened my eyes, and saw, without surprise or any conscious feeling of fear,—my mother standing at the foot of the bed. She was not "in her habit as she lived," but "clothed in white samite, mystic, wonderful." Her face was pale, though not with the pallor of life; her expression sorrowful, and tears which glistened in the moonlight stood in her eyes. And now a strange mental condition followed. My reason told me that I was deceived by appearances; that the figure I saw was neither my mother's spirit nor an unreal vision. I felt certain I was not looking at "a phantom of the brain which would show itself without"; and I felt equally certain that no really existent spirit was there before me. Yet the longer I looked, the more perfect appeared the picture. I racked my memory to recall any objects in my bedroom which could be mistaken for a shrouded ghost; but my memory was busy recalling the features of the dead, and my brain (against the action of my will) was tracing these features in the figure which stood before me. The deception grew more and more complete until I could have spoken aloud as to a living person. Meantime, my mind had suggested, and at once rejected, the idea of a trick played me by one of my college friends. I felt a perfect assurance that whatever it was which stood before me, it was not a breathing creature self-restrained into absolute stillness. How long I remained gazing at the

* One of the most singular facts connected with the condition of the brain during and directly after sleep, is this, that although on waking one may recollect every circumstance of a dream, and even go carefully over the events of the dream with the express object of impressing them on the mind, yet if one sleeps again the whole seems, on our next waking, to have vanished completely from the memory. One can barely remember the circumstance that there had been the desire to retain the recollection of the dream. I doubt even whether this is not generally forgotten; so that in fact in most cases there is nothing to recall either the dream or the first waking thoughts concerning it. There is a story of a person who solved a mathematical problem in his sleep, and found the solution written out on his desk, yet had no recollection of having left his bed for the purpose. Something similar once occurred to myself; but I could just recall the circumstance that I had got up to put on paper the ideas which had occurred to me in sleep. I wish I could make the story complete by saying the solution was singularly ingenious, and so on; but truth compels me to admit that it was utter rubbish. I could not have been in the full possession of my faculties—though seemingly wide awake—when I wrote it out as something worth remembering.

figure I cannot remember; but I know that I continued steadfastly looking at it until I had assured myself that (to my mind in its probably unhealthy condition) the picture was perfect in all respects. At last I raised my head from the pillow, intending to draw nearer to the mysterious figure. But it was quite unnecessary. I had not raised my head three inches before the ghost was gone, and in its place,—or rather, not in its place, but five or six feet farther away, *hung my college surplice*. It was quite impossible to restore the illusion by resuming my former position. The mind which a moment before had been so completely deceived, rejected completely the idea of resemblance. There was nothing even in the arrangement of the folds of the surplice to justify in the slightest degree an illusion which, nevertheless, had been perfect while it lasted. Only one feature of the apparition was accounted for. I have said that the eyes shone with tears: the explanation was rather commonplace; over my surplice I had hung a rowing belt, and the silvered buckles (partly concealed by the folds of the surplice) shone in the moonlight.

The event here narrated suggests the explanation of many ghost stories which have been related with perfect good faith. I believe the imagination only acts so as to deceive the mind completely when the latter has been painfully affected and is in an unhealthy condition. When this is the case, and a vision of some departed friend is conjured up out of realities indistinctly seen, the effect on the mind will depend greatly on the ideas entertained by the victim of the illusion on the subject of ghosts and visions generally.' A believer in ghosts will be too startled to inquire further. If (as happens in many instances of the kind) he can retreat from the dread presence, he will commonly do so, and remain satisfied ever after that *he* at least has "seen a ghost." And in this way, I doubt little, many veracious persons have been led to give their evidence in favour of the common notions about ghosts and visions.

It is a singular circumstance, however, that sometimes several persons may be deceived by an illusion such as I have been considering. There is an instance of this kind in a book on the supernatural which I read many years ago. I cannot at the moment recall the name. It dealt with all forms of mental deception, mesmerism, witchcraft, necromancing, and so on. In the part relating to visions, it cited the case of Sir Walter Scott, who soon after the death of Byron, and while his mind was dwelling on the painful circumstances of that event, saw in the dusk of a large room a vision of the poet which presently *resolved itself into furniture*. Then came the case I have in my thoughts. As nearly as I can remember, the story ran thus:—A gentleman who had lately lost his wife, looking out of window in the dusk of evening, saw her sitting in a garden chair. He called one of his daughters and asked her to look out into the garden. "Why," she said, "mother is sitting there." Another daughter was called, and she experienced the same illusion. Then the gentleman went out into the garden, and found that a garden-dress of his wife's had been placed over the seat in such a position as to produce the illusion which had deceived himself and his daughters.

I know of a more curious instance, where no explanation was ever obtained, simply because the deceived persons were too frightened to seek for one. In a house in Ireland a girl lay dying. Her mother and father were with her; and her five sisters were praying for her in a neighbouring room. This room was well lit, but overhead there was a skylight and the dark sky beyond. One of the sisters looking up towards the skylight, saw there the face of her dying sister looking sorrowfully down upon them. She seized another sister by the hand and pointed to the skylight: and one after another the sisters looked where she pointed. They spoke no word; and in a few moments their father and mother called them to the room where their sister had just died; but when afterwards they talked together about what had happened that night, it was found that *they had all seen the vision of the sorrowful face.*

A remarkable circumstance in these and many other instances of supposed visions, is the utterly unreasonable nature of the supposition actually made in the mind of the ghost-seer. In the stories where a ghost appears for some useful purpose, as to shew where treasure has been concealed or to reveal the misdeeds of some person still living, the mind does not reject the event as altogether unreasonable though the circumstances may be (and commonly are) sufficiently preposterous. But one can conceive no reason whatever why a departed wife and mother should make her appearance in a garden-chair on a dusky evening, and still less why the vision of a dying sister should look down through a skylight. It is singular that on this account alone the mind does not reject the illusion in such cases.

(*To be continued.*)

THE MORALITY OF HAPPINESS.

By Thomas Foster.

CARE FOR SELF AS A DUTY.

(*Continued from page* 70.)

THE thought seems strange to many that in conduct which appears to them mere care of self there may be farther-seeing regard for others than in simple self-sacrifice. Yet the matter is so obvious when pointed out as to suggest later a different sort of retort,—namely that it was scarce worth pointing out. Only as it happens that this truly obvious matter has been grievously overlooked, as the teacher of this essentially true and therefore demonstrable lesson has been rebuked for inculcating mere self-seeking, it is tolerably clear that the lesson was very much needed.

Let us consider how obviously true it is, however, as he presents it. Take for instance the matter on which I touched in my last,—viz., the consideration of the known laws of heredity. "When we remember," says the clear calm teacher of our time, "how commonly it is remarked that high health and overflowing spirits render any lot in life tolerable, while chronic ailments make gloomy a life most favourably circumstanced, it becomes amazing that both the world at large and writers who make conduct their study, should ignore the terrible evils which disregard of personal well-being inflicts on the unborn, and the incalculable good laid up for the unborn by attention to personal well-being. Of all bequests of parents to children the most valuable is a sound constitution. Though a man's body is not a property that can be inherited, yet his constitution may fitly be compared to an entailed estate; and if he rightly understands his duty to posterity, he will see that he is bound to pass on that estate uninjured if not improved. To say this is to say that he must be egoistic to the extent of satisfying all those desires associated with the due performance of functions. Nay, it is to say more. It is to say that he must seek in due amounts the various pleasures which life offers. For beyond the effect these have in raising the tide of life and maintaining constitutional vigour, there is the effect they

have in preserving and increasing a capacity for receiving enjoyment. Endowed with abundant energies and various tastes, some can get gratifications of many kinds on opportunities hourly occurring; while others are so inert, and so uninterested in things around, that they cannot even take the trouble to amuse themselves. And unless heredity be denied, the inference must be that due acceptance of the miscellaneous pleasures life offers, conduces to the capacity for enjoyment in posterity; and that persistence in dull monotonous life by parents diminishes the ability of their descendants to make the best of what gratifications fall to them."

All this is clear and obvious enough when thus pointed out; though the very passage in which Mr. Spencer here so clearly shows that to be happy so far as by due regard of personal well-being one can make oneself happy, is a duty, has been selected for abuse as though he taught simply this—seek to gratify self in every available way. The kind of rebuke justly passed on those who in the search for pleasure, in mere self-gratification, ruin their health, lose happiness, become morose, gloomy, and misanthropic, lose taste for all pleasures lower as well as higher, and hand on to their children and their children's children these and other evil effects of the grosser forms of self-indulgence, has been passed upon the teacher of that far-seeing care of self by which the health is preserved, happiness obtained, the whole nature strengthened and sweetened, the enjoyment of all forms of pleasure increased, and in all these respects the lot of posterity improved to many—nay, to uncounted generations.

On the other hand, there are those who, seeing that the doctrine taught is unassailable on that side, assert that it is and always has been obvious,—forgetting how many morose and gloomy people there are who show by their mere existence that in the past (of which they are the descendants) the contrary doctrine has prevailed, as it still exists in the present (which they in part represent), and will continue doubtless for many generations.

If it be agreed that Mr. Spencer's teaching in this matter is needless where it is accepted and useless where it is needed (because none who would be benefited by it will listen), I answer that the case is otherwise. There are thousands now and their number will be largely increased in the future, who have found in this teaching the lesson which they needed to make their lives happy and their influence in their own time and in the future blessed. It has come as a new and cheering light to them (I was going to say as a revelation, but the word would be misinterpreted) to see in happiness, their own included, the answer to the doleful question, Is life worth living? If by self-mortification, overwork, wear and worry, I make myself wretched and fail to make those around me happier, I may well ask in mournful accents that foolish question. If I not only fail so to make others happier but make them less happy, and hand on gloom and misery to future ages, I may not only ask it gloomily but answer it sadly, Life is *not* worth living. Better, were it lawful, to cease the painful and useless, the worse than useless, contest. But if by due care and thought of self, by reasonable enjoyment of the bright and pleasant things which life brings to most, I in some degree or wholly counterpoise such pains and sorrows as life brings to all, and at the same time help to brighten the lives of those around, and those also of generations as yet unborn, how shall I doubt what answer to give to the question, Is Life worth Living? Not sad is the answer, but bright and cheering.

There is still not a little to be said respecting the due care of personal well-being. Just here I close by remarking that in the attempt to simplify Mr. Herbert Spencer's nomenclature, I certainly did not improve the title of this chapter by calling it "Self *versus* Others" as I did till now, instead of "Egoism *versus* Altruism," as he called the chapter in the "Data of Ethics" bearing on the same subject. Due care of self is not a matter of "Self *versus* Others," seeing that care of personal well-being is essential to the influence of self for the good of others. I have therefore given to this section a new sub-title.

(*To be continued.*)

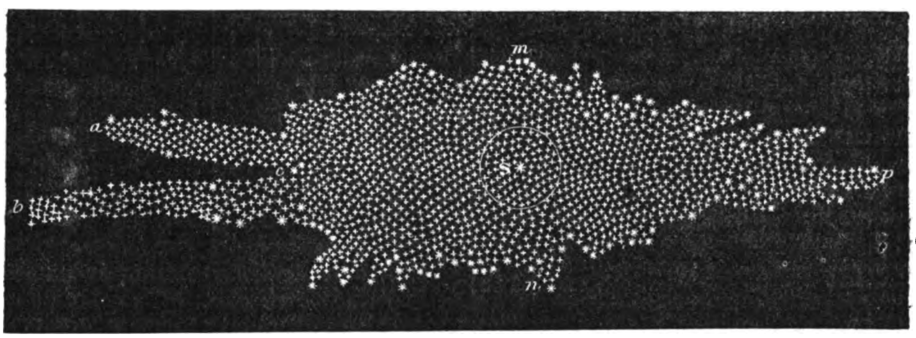


Fig. 5 *bis.* Illustrating Sir W. Herschel's observations up to 1785.

THE UNIVERSE OF SUNS.

By Richard A. Proctor.

(*Continued from page* 69.)

THEN follow the gauges already described, and the section of the galaxy as pictured in Fig. 5, is deduced, from a certain number of the gauges. On the scale on which Fig. 5 is drawn the sphere enclosing all the stars visible to the unaided eye was considered by Herschel to have probably no greater extent than the small circle around S. It will also be remembered that this is only a section of our sidereal scheme according to Herschel's views in 1785. But it is now also to be particularly noticed that Herschel even at this time, by no means regarded the stellar system thus figured in section, as composed of stars spread with a general uniformity throughout the space occupied by the system. He fully recognised the fact *that our own star-system is made up of clusters and nebulæ*, with relatively vacant spaces between, as well as many isolated stars. He describes the star-system of the theory, not as of that relatively-simple form which has been almost invariably assigned to it since, in our text-books of astronomy,* but as "*a very extensive, branching,*

* The star system has been so described even by Sir John Herschel. It may seem perplexing that though he had many oppor-

compound congeries of many millions of stars; which most probably owes its origin to many remarkably large as well as pretty closely scattered small stars that may have drawn together the rest."

Let the reader carefully study the following extract, and he will find that whereas it is perplexing in the extreme if the sidereal system be regarded as a mere cloven stratum of stars, pretty uniformly distributed, it becomes perfectly clear (and wonderfully striking) when we remember that Sir W. Herschel considered the Milky Way to be compound in structure and branching in figure.

"If it were possible," says Herschel, "to distinguish between the parts of an indefinitely extended whole, the nebula we inhabit might be said to be one that has fewer marks of profound antiquity upon it than the rest. To explain this idea, perhaps, more clearly, we should recollect that the condensation of clusters of stars has been ascribed to a gradual approach, and whoever reflects upon the numbers of ages that must have past before some of the clusters could be so far condensed as we find them at present, will not wonder if I ascribe a certain air of youth and vigour to many very regularly scattered regions of our sidereal stratum." "There are moreover many places in the stratum where there is the greatest reason to believe that the stars, if we may judge from appearances, are now drawing towards various secondary centres, and will in time separate into different clusters, so as to occasion many sub-divisions. Hence we may surmise that when a nebulous stratum consists chiefly of nebulæ of the first and second form, it probably owes its origin to what may be called the decay of a great compound nebula of the third form; and that the subdivisions which happen to it in the length of time, occasioned all the small nebulæ which spring from it to lie in a certain range, according as they were detached from the primary one. In like manner our system, after numbers of ages, may very possibly become divided so as to give rise to a stratum of two or three hundred nebulæ; for it would not be difficult to point out so many beginning or gathering clusters in it. This view of the subject throws a considerable light upon the appearance of that remarkable collection of many hundreds of nebulæ which are to be seen in what I have called the nebulous stratum of Coma Berenices. It appears from the extended and branching figure of our nebula, that there is room for the decomposed small nebulæ of a large, reduced, former great one to approach nearer to us in the sides than in other parts. Nay possibly there might originally be another very large joining branch, which in time became separated by the condensation of the stars; and this may be the reason of the little remaining breadth of our system in that very place; for the nebulæ of the stratum of Coma are brightest and most crowded just opposite our situation, or in the pole of our system. As soon as this idea was suggested I tried also the opposite pole, where accordingly I have met with a great number of nebulæ though under a much more scattered form."

It will thus appear that even at this early stage of his researches Sir W. Herschel was so far from regarding all the nebulæ and clusters as external Milky Ways, that he felt free to broach the striking theory that the vast groups and clusters of nebulæ seen in certain regions of the heavens are decayed (or at least ancient) branches of our own star-system.

In the following passage he even more clearly indicates his belief that nebulæ and clusters may be formed in progress of time from portions of the Galaxy:—

"Some parts of our system seem indeed," he says, "to have already suffered greater ravages of time than others, if this way of expressing myself may be allowed. For instance, in the body of the Scorpion is an opening, or hole, which is probably owing to this cause." He describes the opening—a place where close by rich galactic fields he found none but a few scattered and pretty large stars. Then he proceeds: "This opening is at least four degrees broad; but its height I have not yet ascertained. It is remarkable that the nebula 80 Messier, which is one of the richest and most compressed clusters of small stars I remember to have seen, is situated just on the western border of it; which would almost authorise a suspicion that the stars of which it is composed were collected from that place, and had left the vacancy. What adds not a little to this surmise is, that the same phenomenon is once more repeated with the cluster of stars 4 Messier, which is also on the western border of another vacancy, and has moreover a small miniature cluster, or easily resolvable nebulæ, following it at no very great distance."

In fact Sir W. Herschel clearly distinguishes from among the rest those nebulæ which he regards as Milky Ways like our own. He mentions that "there are some very remarkable nebulæ which cannot well be less but are probably much larger than our system; and being also extended, the inhabitants of the planets that attend the stars which compose these nebulæ must likewise perceive the same phenomena. For which reason these nebulæ may also be called Milky Ways by way of distinction." He limits this term to those nebulæ which are of considerable apparent extent, but shine either wholly or in part with a milky light not resolvable into stars even in his most powerful telescope.

(*To be continued*).

tunities of conversing with his father upon this and kindred subjects, Sir John Herschel should to any extent misapprehend the meaning of Sir W. Herschel's papers. I would point out, however, that we have no evidence whatever tending to show that the view of 1785, and the progress of the elder Herschel's theorising from that date until the year 1818 (when his last paper appeared) were the subject of discussion or conversation between the elder and younger Herschel,—or were likely to be so. I fancy Sir W. Herschel talked little about his theories within his family circle. I cannot recall a single instance in which Sir J. Herschel has written, "I remember that my father used to say" so-and-so respecting the universe. In his "Outlines of Astronomy," as well as in the "Observations at the South Cape," Sir John Herschel invariably refers to his father's published statements. I might also refer to passages in letters of his to myself, in which he seems to imply that his knowledge of his father's theories came from a study of Sir W. Herschel's papers. I would venture indeed to express my belief,—which may, however, be a mistaken one,—that Sir John Herschel had not studied any portion of his father's work save only that relating to the double stars, during his father's lifetime. It is known that his undertaking to continue the labours commenced by his father, was prompted by filial piety, his own tastes tending rather to chemical and mathematical research.

TRICYCLES IN 1884.

BY JOHN BROWNING,

Chairman of the London Tricycle Club.

LEST any of my readers should suppose that the experiences I have given in my last article on "Trying Tricycles" are matters of everyday occurrence, I should wish to point out that they are the only unfortunate events that have happened to me during the last three years in trying dozens of tricycles, that none of them have been with machines which I have recommended in my articles, and that the machines themselves might fittingly be considered as a series of "frightful examples" only to be known to be avoided. It would be well, however, to recollect that a

tyro should not choose a machine for himself without advice, nor, if he has chosen one, should he ride it on the first occasion without the assistance of an experienced rider—if he can possibly get it.

As I have, during the last month, had the opportunity of pretty severely testing several novel machines, some of my readers may probably be glad to have some information about them at as early a date as possible, as they may be contemplating a purchase.

My first trial was made of the "Traveller, No. 2" (I wish the makers, Messrs. Singer & Co., would give this excellent machine a distinctive name). Although, if the "Humber" had not been born before it, the "Traveller" would probably never have seen daylight, yet the "Traveller, No. 2," has a character quite peculiar to itself. Its special feature is that it is a triple-steerer—that is, the three wheels all move together in such a way that the somewhat erratic steering of the "Humber" is completely overcome, while the steering is completely under control either with the hands or the feet.

At first starting with the machine down-hill I came very nearly in for an accident, and I finished with a closer shave still. Both arose from the same cause. When the machine is running with the three wheels in a line parallel to the backbone, it answers the helm with great facility; but when the three wheels are all inclined to the right—in which case the machine will, of course, travel to the right—and you wish to change the course, there is an appreciable resistance to your steering it in the opposite direction. Possibly this arises from the grip or bite of the three wheels on the ground, all of which must be moved together before a change in the direction can be effected. If so, it is a good quality once it is fully understood and taken fairly into consideration.

Just after starting I had to steer close to the near side to keep clear of a horse coming up the hill in the middle of the road. Just below was a horse and cart standing on the near side facing me. Before steering back into the middle of the road again to avoid them, I found myself in front of the horse, and only saved myself from running full face into him by back pedalling and applying my brake as hard as I dared, for as it was I lifted my hind wheel off the ground. I had done too much in the way of steering, but I did not make the same mistake again throughout the day.

On returning, as I was riding up a short, stiff hill, the surface of which was loose and rough, I heard a heavy horse, with an empty cart, trotting up behind me. As I could tell it was gaining on me, I attempted to steer out of its path, and at the same time spurted to get out of its way. The resistance to the change in the direction of the steering was so great that, although the machine is a double driver, both the wheels skidded, and I found myself with my cranks on dead centres and helpless. The incline was so steep that my brake would not hold it, so that I was compelled to dismount hurriedly from the pedal as best I could, with the near wheel of the cart almost touching my off wheel.

This shave was my own fault again, as the machine should not have been turned or allowed to turn when riding up a steep hill.

Had it been driven up in a straight line, I feel sure it would have gone up without skidding, as I have ridden the hill many times easily on inferior machines.

It must be remembered that a novice would not have attempted to ride this machine as I did on the first occasion, and so would have avoided the risks I ran.

"The Traveller, No. 2," is a *front and rear-steerer*, by means of bicycle-handles. The first day I took it out was a most trying one. There had been a sharp frost in the night and a rapid thaw in the morning, and the roads were rotten, and a good part of the way I had to struggle against a strong head-wind. I never knew a machine go better under such unfavourable conditions. It is a good hill-climber, and perfectly safe down hill with the slightest attention. I do not consider it against the machine that at the outset it requires learning. A writer in a recent number of the *Tricycling Journal* very justly said that if a man buys a piano, he expects to have to learn to play it. If he buys a horse, he knows he will have to learn to ride it; but, strangely enough, every person supposes he can ride a tricycle without learning.

The "Coventry Rotary" has been generally admitted to be the best machine made for nice steering. This is due to its being a double-steerer. I do not see why the triple-steering "Traveller" should not equal or even eclipse it, though it will require some practice on the part of the rider to bring out its best qualities in this respect. The violin is the acknowledged king of musical instruments, but one would scarcely suppose this to be so if they heard it for the first time in the hands of an uninstructed performer.

I promised my readers to say more respecting the new rear-stearer, the "Rover," when I had been able to test it more severely. Having now ridden it almost daily for nearly a fortnight, I am able to report confidently that it is a first-rate machine. It is about 10 lb. heavier than it need be, but it would be difficult for the most critical rider to find any other fault with it. It is an excellent hill-climber, fast and easy on the level, and fairly safe down hill. I have not, owing to the time of year, been able to find any hills in sufficiently good condition to run down at a great velocity, but I have run down several at the rate of more than ten miles an hour without the machine swerving or showing any tendency to oscillate.

The brake of the "Rover" is by far the most perfect brake I have ever used. While it is powerful enough to stop the machine in a few feet, it will not stop it dead, so that there is no risk of throwing yourself out in front with it, and if you wish to you can bring up in ascending the steepest hill it is possible to ride, with the certainty that the brake will hold you. Scarcely any other brake will do this. As a rule brakes will not check a machine efficiently when it is running backwards.

The "Rover" is one of the handiest machines to mount and dismount from, and the handiest to handle when you are dismounted.

It is also a first-rate luggage-carrier. If it should prove slightly unsteady downhill when travelling at a great pace, I should advise the makers to make the steering-wheel larger, say 20 in. in diameter. This alteration I have been advocating for years past, particularly in rear-steering machines.

I may say that when testing the "Rover" downhill I had my seat so far forward that I was actually in advance of my pedals, and my handles were underneath and almost behind me; yet I was unable to detect the slightest tendency on the part of the hind wheel to leave the ground.

The best form of the "Rover" would be the machine weighing not more than 75 lb., with 40-in. driving-wheels, geared up to from 46 to 50 in., according to the strength of the rider; or with a good two-speed gear (Bown's would be worth trying), geared for power to 35 and for speed to 55 in. I am obliged to Mr. Leni for bringing this capital machine before my notice so early in the season.

The following rough rule may be useful as a guide to the adoption of gearing. If a standard machine, with, say, 44-in. wheels, geared level, weighs 90 lb., then for every

10 lb. the machine can be reduced in weight, a weak man may gear-up 2 in. extra, a man of average strength $2\frac{1}{2}$ in. extra, and a very powerful rider 3 in. extra with advantage; while if a two-speed gearing be used, a weak man may increase his high speed still further by 6 in., a man of average strength by 8 in., and a very powerful rider by 10 in. This would give a first-class rider, on a machine weighing only 60 lb., a high speed of 63 in., about the utmost he could possibly use with advantage, even for racing on good roads.

About a year or eighteen months ago, I had an opportunity of riding the Centre-Cycle on a level flooring. A public company is now being formed to bring out this machine. Last Saturday I was very kindly invited to test the machine again at the Crystal Palace.

The "Centre-Cycle," which was invented by Mr. Burstow, of Horsham, is of very novel and ingenious construction. It consists of one large wheel, like a bicycle wheel, on which the rider sits astride, and four small wheels at the corners of a long, low platform, of which the bicycle wheel occupies the centre.

The steering is effected by means of bicycle handles, which bring two of the small wheels to incline in opposite directions, and lower the machine, and incline the large wheel on the side towards which the machine has to be turned. The four small wheels can be lifted off the ground at the will of the rider, and feats that would shame a bicycle-trick rider, such as going over two bricks standing on their ends, can be performed upon it easily and gracefully.

But the machine appears to me to possess faults which are incurable. It is unstable, having a very much smaller wheel base than any tricycle; it is difficult and fatiguing to mount and dismount from, and, worst of all, *it is only a single driver*, that is, only one wheel out of the five can be driven. This is a great disadvantage. For the single large wheel of the machine would skid, and the rider, however strong and skilful, would be brought to a standstill in many places where a worse rider on a good double-driving tricycle would travel well. Possibly when carrying heavy weights the purpose for which it is now being put forward, the grip of the single driving-wheel might be increased, and the stability would certainly be greatly increased by carrying a large amount of weight well below the centre of gravity of the machine, but even for carrying purposes I should greatly prefer Singer's "Carrier" or Starley and Sutton's "Despatch."

WILD BEES.

By S. A. Butler, B.A., B.Sc.

(*Continued from page 54.*)

THE next business is to provide a store of food sufficient for the support of the young during the whole period of its growth, that is, throughout its larval existence; for as the grub is completely destitute of limbs, it is utterly unable to go forth into the world to seek its own living, and, therefore, if no provision were made for it, starvation would immediately ensue. The careful mother, however, relaxes not her efforts till she has provided against such a calamity; an excellent manager is she; for though she has hitherto had no experience in providing for the wants of bee-grubs (unless, indeed, she has some recollection of the days of her own larvadom, and remembers the store laid up for her by her own mother), she knows exactly the amount of food that will be needed by each one of her expected offspring, and provides for each the requisite amount, even before it is born. The food consists principally of the pollen of flowers, that golden dust with which the central organs of flowers are so abundantly supplied; this she collects from the flowers, and passing it from hand to hand, packs it up amongst the pubescence of her hind legs, or underneath her abdomen, and so conveys it home. Here it is mixed with a little honey and made up into a ball, in many cases about the size of a garden pea. One of these pellets is placed at the end of each burrow, and then a single egg is deposited upon each, after which the burrows are closed up, and when all the stock of eggs has been deposited, the mother's work is ended. Though she has lived only through one season, she is now in the decrepitude of old age. She can look back with calm satisfaction at the successful accomplishment of her important work, and can, therefore, philosophically compose her mind to her rapidly-approaching demise. The eggs are small, some very minute, and are usually slightly curved and tapering at one end. Left to themselves, they soon hatch; a wormlike, footless grub issues from the shell, and begins a vigorous attack upon the store of rich and most nutritious food provided for its sustenance. Its sole object now is to eat, and its persistent efforts in this direction receive their due reward in a growth so rapid as very speedily to bring it to the close of its larval existence, and at the same time to the end of its appointed store of nutriment. This sensuous life over, if the time of year be suitable, the creature becomes transformed into the chrysalis or pupa. This is at first semi-transparent, and through the skin can be seen the gradually-developing body of the bee, with its wings, legs, and antennæ devoutly pressed lengthwise along its breast. On reaching perfect maturity it bursts through its shroud, casts aside its grave-clothes, and crawls forth, a feeble, tottering thing, to the light of day.

Warm sunshine and fresh breezes soon raise its drooping spirits, and it spreads its wings and, for the first time in its life, leaves the sordid earth, and soars away, rejoicing in the beauty of its newly-acquired garb, and exulting in the possession of faculties far superior to those with which it has hitherto been endowed. And now commences the serious business of its life—that which all its previous stages have had in view, and for which they have been preparatory — the perpetuation of its kind. Soon mated, and almost equally soon widowed, the female bee devotes herself most assiduously to those labours I have already referred to as preceding the deposition of the eggs, and thus the whole cycle is again passed through.

Such is the general outline of the life-history of the majority of our wild bees. But there are some kinds which are structurally incapacitated for the life of unremitting toil that forms so conspicuous a characteristic of the industrious bees. The so-called Cuckoo-bees are quite unprovided with the pubescence necessary for the collection of pollen, and, consequently, totally unable to obtain by their own personal exertions the sustenance needful for their young. But still the maternal instinct is strong within them, and they cannot quit life till their helpless young have been, in some way or other, provided for. They adopt, therefore, the only resource left to them; they watch for the burrow of some industrious bee, and as soon as they see that it has been furnished with a supply of pollen, they dart in during a temporary absence of the rightful owner and deposit an egg on the mass. From this habit, which is paralleled by the cuckoo amongst birds, they have received the name of Cuckoo Bees. They are frequently very brilliantly coloured, their bodies being banded and

striped something like that of a wasp (Fig. 4), and they are not necessarily at all like the bees on which they are parasitic. Each species of Cuckoo Bee usually confines its visits to the nests of one particular species of industrious

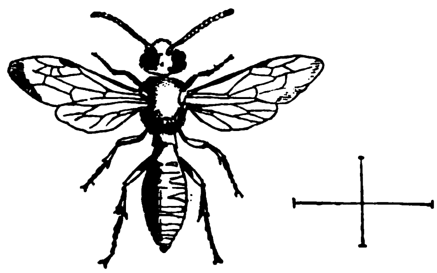


Fig. 4.—Nomada alternata. A Cuckoo Bee parasitic upon several species.

bee, though sometimes it rings the changes on two or three; each kind of industrious bee, however, may have several kinds of Cuckoos parasitic upon it. The egg of the Cuckoo Bee hatches before that of its host, and the larva consumes the pile of pollen before the other poor creature is able to prevent it, so that when the legitimate owner awakes to consciousness he finds his larder empty and himself a ruined being, with no alternative but starvation.

This is not the only way in which the careful exertions of the industrious mother on behalf of her offspring are liable to be frustrated. There is a most brilliantly-coloured hymenopterous insect, with bright red body and green head and thorax, which may often be seen lurking about in the neighbourhood of the burrows of bees. It is no very distant relation of the bees themselves, but it is their

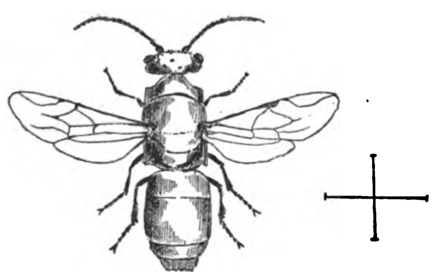


Fig. 5.—Chrysis neglecta. Firetail.

inveterate foe. It is called the Ruby-tailed Fly, Firetail, or Golden Wasp (Fig. 5). Unlike the Cuckoo Bees, its grubs are carnivorous, and attack, not the store of food laid up by the industrious bees, but the larvæ themselves. The young grub of the Firetail seizes the much larger grub of the bee, and appears, at first, to suck out some of its juices, thereby gradually rendering the poor creature flaccid and feeble. After these sucking operations have gone on some time, and there is not much vitality left in the poor victim, though the destroyer has flourished grandly on the juices abstracted, the latter proceeds to complete the filling out of its own bulk by adding the solid parts, as it has previously the fluid of its victim, to its own body, and so the hapless bee-grub is devoured, and scarcely a fragment is left behind.

Not only are the nests of bees liable to the invasion of parasites, but their very bodies are not exempt from attack. There is a most curious little insect which is nourished within the bodies of many bees; so quaint a being is it that much debate has taken place as to its systematic position in the animal kingdom, and its relationship to other insects. By some naturalists it is considered to be an outlying member of the great order of beetles, but by others it is regarded as of so curious a type as not to fit well into any of the ordinary groups of insects, but to necessitate the formation of a division exclusively its own. The creature is named *Stylops;* the male is a little, black, impish-looking thing, with milky-white wings (Fig. 6), but the female is a worm-like creature without wings, and never leaves the body of its host.

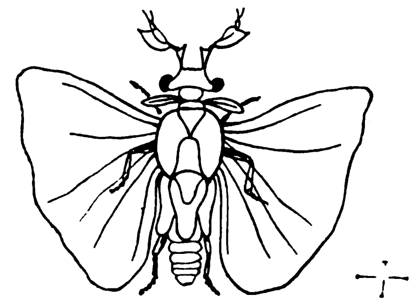


Fig. 6.—Male Stylops.

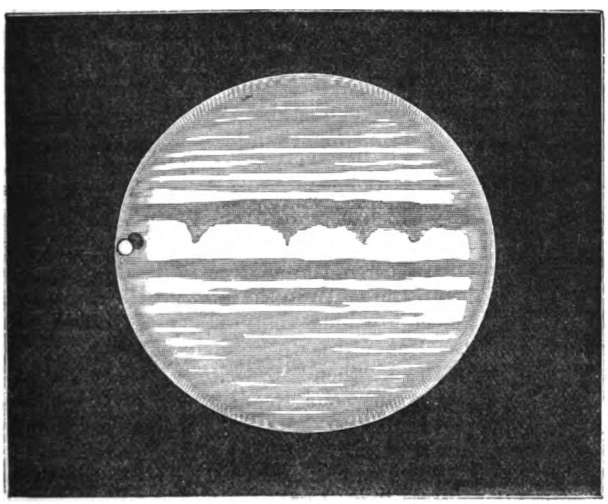
These curious little insects were discovered by Mr. Kirby, well-known as one of the earliest of the historians of our British bees. Seeing a small protuberance on the body of a bee, he endeavoured to remove it with a pin, supposing it to be a mite of some kind, such creatures being often found on bees. Instead of a mite, however, he drew forth from the bee's body a whitish grub, about $\frac{1}{4}$ in. long, the head of which had formed the protuberance observed. Surprised and interested he made further search with a result which is best told in his own words: "After I had examined one specimen, I attempted to extract a second, and the reader may imagine how greatly my astonishment was increased, when, after I had drawn it out but a little way, I saw its skin burst, and a head as black as ink, with large staring eyes, and antennæ consisting of two branches, break forth and move itself briskly from side to side. It looked like a little imp of darkness just emerged from the infernal regions. I was impatient to become better acquainted with so singular a creature. When it was completely disengaged and I had secured it from making its escape, I set myself to examine it as closely as possible; and I found, after a careful inquiry, that I had not only got a nondescript, but also an insect of a new genus, whose very order seemed dubious." It is possible to breed these little creatures from the bodies of the bees they frequent. The following directions were given by the late Mr. F. Smith, the zealous Hymenopterist of the British Museum:—Place a bee which contains in its body a female Stylops, and is, therefore, said to be stylopised, in a box 5 in. or 6 in. square, and covered at the top with gauze; supply the bee daily with fresh flowers, such as it is accustomed to visit; in a few days she will probably appear as though her abdomen were covered with dust; this dust, on microscopical investigation, turns out to be a vast number of minute creatures, the larvæ of the Stylops, which have been hatched within their parent's body. If a bee, covered in this way with the larvæ of the Stylops, should happen to settle on a flower, many of the tiny creatures may perhaps be deposited; if another bee then visit the flower, they may be able to attach themselves to its body by clinging to its pubescence, and so get carried to its nest, where they will attack the bee-larvæ, boring into their bodies.

(*To be continued.*)

JUPITER IN A THREE-INCH TELESCOPE.

BY A FELLOW OF THE ROYAL ASTRONOMICAL SOCIETY.

THERE is assuredly no member of the planetary system which offers so diversified a series of phenomena to the contemplation of the student, as the noble one which we propose to examine to-night. Exceeding the earth in volume between thirteen and fourteen hundred times, and reflecting (as has been calculated) some sixty-three out of every hundred parts of the sun's light that falls upon him, Jupiter exhibits a disc and shines with a lustre which renders him a conspicuous object in the smallest telescope. Now, it might be supposed, from the brightness of the planet, that a high magnifying power would be most applicable to his examination; as a matter of fact and practice, however, it is found that he will not bear so much amplification, with advantage, as his much duller neighbour, Saturn. Moreover, all the curious detail of which we are immediately about to speak is much better seen when a slight haze overspreads the sky and softens the glare of light on Jupiter's disc, which, in itself, forms an impediment to the perception of very delicate markings. Happily for us, such a haze does cover part of the sky on the night which we have selected for our drawing. Wishing to use as much magnifying power as we can without impairing definition, we, as a limit, fix on 50 to each inch of aperture of our telescope. Arming it then with a power of 150, we turn it on to the planet, to behold the spectacle, of which the subjoined engraving gives a pretty accurate idea.



Jupiter. Jan. 24, 1884, 9h. 5m. p.m. Power 150.

The first thing that will probably arrest the attention of the young observer is the shape of the planet. Instead of presenting a circular disc it will be seen to be very notably elliptical; in other words, flattened at the poles, and bulging out at the equator. And next, as the eye gets accustomed to the image, a series of belts of different depths of shading, and even of markedly different colours, will be seen, striping Jupiter's face in a direction parallel to his equator. Let us take those visible when our drawing was made. We will begin at the top of the planet, which, as all astronomical telescopes invert, is, of course, its south pole. For some little distance the tint is pretty uniform—or as an artist would say, "flat;" but then it is seen to consist of a series of stripings; the lighter divisions between them being well marked and easily visible as we approach the northern edge of this polar capping. Then comes a distinct white streak, bounded on the north by the principal belt in Jupiter's disc. This is the most conspicuous feature on the whole face of the planet. It is of a decidedly brownish tint, and its northern edge shows a marked tendency to throw out small projections, so as to give a kind of "scalloped" effect. With a larger instrument the dark matter of this belt is seen to emit prolongations or streamers of a wispy character from these projections, diagonally across the broad bright equatorial interval; but what we have drawn above shows everything that it is within the power of a three-inch telescope to reveal. The northern and fainter of the two equatorial dark belts is in its turn succeeded by yet another white streak; that by a fainter dark one still, while a multiplicity of stripes covers the north pole of the planet with a shading which, like the south polar capping, looks practically "flat" or homogeneous. A little attention will show that the east and west "limbs" (or edges) of the disc are not quite so bright as its central parts, and that a slight fading away of the belts is perceptible as they approach the limb. The satellite and its shadow visible on the left hand, western, (or "preceding") limb of the planet will be dealt with by and bye. It must not, however, be supposed that the markings we have described are constant or permanent, like the oceans, seas, continents, and islands of our own earth; or that a map of Jupiter constructed from observations now would be of much use in, say, 1886. Moreover, confining ourselves to a single night's observation, the details on the surface of the planet will be seen to undergo a very marked change in the course of four or five hours' persistent watching of them, for the simple reason that Jupiter is rotating on his axis at a speed so tremendous as to be beyond our power of realisation. The notable markings which appear from time to time upon his face give obvious indications of proper motions or driftings of their own, and this complicates and renders uncertain the exact determination of the period of the planet's rotation. It would, however, seem that he turns on his axis in a period not differing greatly from 9h. 56m., so that a spot on his equator must travel at the rate of over seven miles a second! A simple plumb-line must form an effective transit instrument in such a favoured locality! Well then, by his mere rotation fresh features are brought into view; but after the lapse of nine or ten hours we shall revert to that aspect of the planet which it presented when we commenced our watch. These changes, therefore, are simply such as arise from viewing in succession the markings extending over the whole of Jupiter's spheroidal surface—of which, of course, only one-half is visible at any one given instant. We have now to speak of the much more remarkable changes which occur in the markings themselves in the course of months or years. As a familiar example, we may refer to the wonderful great, oval, red spot which appeared on the face of the planet to the south of the southern one of his equatorial belts in the year 1879, and which persisted in a perfectly visible form up to last year; although it has now vanished, save in large and powerful telescopes, in which it has been perceived as the very ghost of its former self. It may be seen in the curiously "smudgy" little caricature of Jupiter in p. 221, Vol. I. of KNOWLEDGE. In August, 1878, a great circular white spot formed a most conspicuous object on the planet's equator, and in the succeeding year one enormous dark belt, covering Jupiter's equatorial regions, was broken up, or perforated, as it were, with similar but more irregularly-shaped white markings. In 1880, a sinuous continuous white marking separated the equatorial belt into two—the red spot at this time appearing of a pale scarlet tint. In

1881, the red spot persisting, the belts became much narrower, and the "vandyking" or "scalloping" of the northern edge of a dark one south of the Equator was even more marked than the similar phenomenon in the somewhat corresponding dark streak shown in our sketch above. And so we might go on detailing a series of most curious changes which have occurred during the last five-and-twenty years; but for the fact that our object in these papers is the practical one of teaching the student exactly what to look for, rather than merely the giving a list of other people's observations. With one concluding remark, then, on the phenomena of Jupiter's disc proper, we will pass to the consideration of those of his satellites. It is this. Jupiter is much too far off to exhibit phases; but he does show an indication of doing so, when (what is technically called) in quadrature, or when he is 90° east or west of the sun, as measured along the ecliptic. Under these circumstances the limb farthest from the sun exhibits a perceptible shading, much too deep to be confused with the slight fading away of light all round the limb which is always visible.

(To be continued.)

Shooting Stars.—Perhaps, there is no instance in the whole history of astronomy in which so marked a change of view has had to be recorded as in the case of shooting-stars. If we take up an old treatise on astronomy, and by an old work we mean simply such an one as might have been in use a generation ago, we shall find no mention made at all of falling or shooting-stars. These objects were not recognised, in fact, as astronomical, save by a few advanced inquirers. To learn anything about meteors from the works in use thirty or forty years ago we must turn, not to astronomical, but to meteorological treatises. In these, among electric phenomena, or among such appearances as Will-o'-the-Wisps or Jack-o'-Lanterns, we find falling-stars more or less lucidly dealt with. We are told that they are generated (in some way not explained) in the upper regions of air, and being thence attracted by affinity, or in some other conveniently unintelligible way, descend with more or less rapidity to the earth according to the state of the atmospheric electricity. It is pleasing to turn from these vague and unsatisfactory notions to the results which have rewarded modern scientific research into the nature of these interesting bodies.

Conversion of Light into Electricity.—The conversion of electricity into light is now a fact of every-day utility, but the reverse process has been very slow of accomplishment. It has, however, been effected by Herr Sauer, whose sunlight battery has been described in the *Electrotechnische Zeitschrift*. The chemical rays furnish the power, and the battery will only act in sunlight. It consists of a glass vessel, containing a solution of fifteen parts of table salt and seven parts of sulphate of copper, in 106 parts of water. In this is placed a porous cell containing mercury. One electrode is made of platinum, and the other of sulphide of silver, and both are connected with a galvanometer. When not in use the whole is enclosed in a box. When in use the platinum electrode is immersed in the mercury, and the other in the salt solution; the battery is placed in the sunlight, and the galvanometer needle is then found to be deflected, the sulphide of silver being the negative pole. If the sun is clouded, or any other change in the intensity of the light occurs, it is indicated by the needle. The exact effect produced by the light rays does not as yet appear very clear, but their presence distinctly produces electrical action, and their absence suspends it.—*Engineering.*

TO READERS.

Already an overwhelming balance of letters in favour of the proposed change of size from twenty-four pages to thirty-two, and of price from 2d. to 3d., shows that not only will the change be cheerfully agreed to, but that it has long been desired. A large proportion of those who have thus written note as the chief (they are good enough to say the only) fault of Knowledge that the articles are too short or broken up too much. About one in twenty-five express doubt whether we may not have among our readers many to whom the increase of price may be displeasing, though as yet, among all who have written, only one has actually said that he will have to cease subscribing if the change is made. Small though I believe the section of our readers to be who will thus view the change, it is a section to which great consideration is due. I may even say that the chief purpose I had in view in planning Knowledge (I hear some cynic say, "Oh bosh! we know what that purpose was"; but even if the implications were just it would not affect my reference to the chief want which I intended Knowledge to meet) was to bring simple yet accurate and attractive accounts of the progress of Knowledge before a section of the reading public including many to whom lowness of price might be an object. But it has become clear from the correspondence which has reached me that the proportion of our present readers likely to care in this sense for the change of price is far too small to justify me in disregarding on their account the emphatically expressed wish of the great majority.

In three among the relatively few letters deprecating change,—comparisons are made between the quantity of matter provided in Knowledge, and that given in other weekly journals like it in being low-priced. I may venture to remark, though it is obvious that few among our readers make a similar mistake, that it is really absurd to compare Knowledge—for example—with such papers as *Chambers's Journal*, *All the Year Round*, and *Cassell's Magazine*. These excellent weekly papers have an enormous circulation—which they owe chiefly to what may be called narrative matter. Knowledge most certainly has nothing to complain of in the matter of circulation. I believe I am within the truth in saying that its circulation is at least thrice as great as that of any weekly of the same character. But until we introduce the narrative element ("in such a 'then' I write a 'never'") we cannot hope to count our readers by the hundred thousand, nor therefore pretend to fill the same space for the same money as the capital serials I have named.

On the other hand, there is a weekly paper of an entirely different kind—the *English Mechanic*—which some few seem to think akin to Knowledge, and cheaper. But there is not the remotest resemblance between the purpose of that useful paper and our own. I speak very confidently and strongly on this point; because I am myself responsible for the completeness of the dissimilarity. At the beginning of the career of Knowledge it was pointed out to me that however different our plan and scope there were certain features in which Knowledge resembled the *English Mechanic*. Those features I immediately altered. I think it wrong and unfair to try to step in between a useful and successful worker and his work—to note how another has achieved a well-deserved success, and to say, "Now will I come in and reap where I have not sown." Knowing that the *English Mechanic* was catering well and successfully for its readers, and doing admirably the work it was started to do, I would not allow even the semblance of similarity

between the two papers to remain. How entirely distinct they now are is shown in this, that except in the correspondence columns (which occupy a much larger space there than here) there is scarcely a single paper or article in the *English Mechanic* which would not be (and be felt at once to be) entirely out of place in KNOWLEDGE. As regards cheapness ("caparisons are odorous," as Mrs. Malaprop did *not* say), I may put the case thus, One paper gives more matter for its price, the other pays better price for its matter. Each strives to the best of its ability to do its proper work, and there neither ought to be, nor is, the least rivalry between them.

One writer, and one only, calmly assumes that the relatively small number of advertisements in KNOWLEDGE when as yet the paper was young, involved a sort of promise that we would never insert more! But, as I said, those who have written are nearly all (certainly more than 20 to 1) in favour of the proposed change,—and even the few who seem against it are most kindly, courteous, and reasonable in their suggestions.

I think the change in KNOWLEDGE had best begin with the first number in March. Before then I will point out how we propose to make use of the larger space made available to us. I shall ask all the kindly readers of KNOWLEDGE to help during the next few weeks in preventing (by getting new subscribers if they can) our constituency from being in any large degree reduced while the change is in progress.

I may take this opportunity of thanking readers for their kindly interest in my health. It will be inferred from the date at which I resumed work that I felt more quickly the beneficial effect of rest than I had expected. I may say I am now for the first time *myself* since the railway accident of last July, a jarred sensation at the back of the head and neck having been an almost constant accompaniment of my travelling and working hours (it almost disappeared when I rested for a few weeks last autumn) until the lecture trip commenced this week. Not a trace of it seems now to remain,—and I think I may fairly say in response to kind inquiries "Richard 's himself again."

RICHARD A. PROCTOR.

THE death is announced of Mr. Julius Pintsch, of Fuerstenvalde and Berlin, the originator and successful patentee of the now widely-used system of lighting railway carriages and floating buoys by means of compressed air. He was widely known in Germany as a successful gas engineer. Mr. Pintsch was seventy years of age.

THE SECRET OF THE NORTH POLE.—The greater part of the earth's surface has been explored more or less thoroughly by our inquisitive race. We know the length and the breadth of our domain, even of those parts which we have never yet been able to reach. Here and there are regions of greater or less extent, which no traveller has yet crossed. But it is only at the two poles of the earth that spaces exist which it has hitherto seemed hopeless to attempt to penetrate. What lies beyond those icy barriers which have again and again foiled our seamen? The great Antarctic ice-continent, from whose mysterious recesses the tidal waves issue without ceasing, and the yet more perplexing Arctic region, in which those waves come to rest, and whither the great Gulf-stream is perpetually bearing its waters—what do these strange regions conceal within their recesses? Will they never give up their secret, these frozen fortresses?—or will the time come when man shall stand a victor at one or other pole, and solve the questions which now perplex geographers?

THE FACE OF THE SKY.

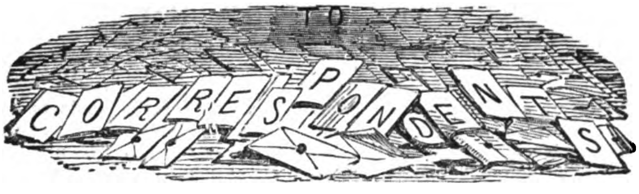
FROM FEBRUARY 15 TO FEBRUARY 29.

BY F.R.A.S.

THE Sun will be examined whenever visible for the spots and other indications of disturbance now so frequent. The Zodiacal light, too, may now be looked for after sunset. The night sky will be found delineated in Map II. of "The Stars in their Seasons." Minima of Algol (Map I.) will occur at 1h. 53m. a.m. on the 23rd, 10h. 43m. p.m. on the 25th, and 7h. 32m. p.m. on the 28th. Mercury is still very unfavourably placed in the sky, and is a morning star. Venus as an evening star improves in her position for the observer, and is a very conspicuous object just to the south of west after sunset. She is still gibbous in the telescope. Mars is visible all night long, and is in an excellent position for the observer. His path may be gathered from the Zodiacal Map on p. 70.* Jupiter continues to be admirably placed in every respect for observation. His exact position will be found laid down in the Zodiacal map on p. 40. Should the nights prove fine the student will find pleasant occupation in watching the phenomena of Jupiter's satellites. To-night (the 15th) Satellite I. will be occulted at 9h. 9m., and reappear from eclipse at 12h. 5m. 28s. p.m. On the 16th the Transit of Satellite I. will begin at 6h. 29m. p.m.; followed by that of its shadow at 7h. 9m. The egress of the satellite will happen at 8h. 49m., and the shadow pass off at 9h. 28m. Meanwhile, at 9h. 20m., Satellite IV. will be occulted. Satellite I. will reappear from eclipse at 6h. 34m. 13s. p.m. on the 17th. On the 19th Satellite II. will enter on to Jupiter's disc at 8h. 49m. p.m.; its shadow not until 10h. 16m. At 11h. 43m. the satellite will quit the opposite limb of the planet, the shadow not following it until between 1 and 2 o'clock the next morning. The Transit of Satellite III. will begin at 7h. 25m. p.m. on the 20th; that of its shadow at 10h. 25m. The satellite passes off at 10h. 59m; the shadow not until after 2 a.m. on the 21st. On the evening of the 21st Satellite II. reappears from eclipse at 8h. 16m. 9s. On the 22nd an occultation of Satellite I. will happen at 10h. 56m. p.m. On the 23rd Satellite I. will begin to cross Jupiter's face at 8h. 15m. p.m.; as will its shadow at 9h. 3m. The egress of the satellite occurs at 10h. 35m.; that of its shadow at 11h. 23m. p.m. On the 24th Satellite I. will reappear from eclipse at 8h. 29m. 10s. p.m. The transit of Satellite II. will begin at 11h. 10m. on the night of the 26th, and its shadow will follow it 52 minutes after midnight. The egress of both will occur during the early morning of the 27th. On the night of that day (27th) Satellite III. will enter on to the planet's limb at 10h. 52m. On the 28th, Satellite II. will re-appear from eclipse at 10h. 51m. 50s.; and finally, an occultation of Satellite I. will take place 43 minutes after midnight on the 29th. Saturn continues to approach the West, and is best seen as soon after dark as possible. Our description of his position on p. 75 is still applicable: a remark which applies equally to Uranus, which, however, is travelling slowly towards β Virginis. Neptune is very poorly placed for the observer. The moon's age at noon to-day is 18·3 days, and, obviously, 29·3 days at the same hour on the 26th. On the 27th at noon her age will be 0·7 day, and 2·7 days at noon on the 29th. One occultation only will occur between the hours to which these notes are limited, during the next fortnight. It is that of the 4½th magnitude star λ Virginis, of which the reappearance alone will be visible, as the Moon will not have risen on the night of the 16th at the time she covers the star. It may, however, be seen to emerge from behind her dark limb at 15 minutes after midnight, at an angle of 186° from her vertex. The Moon is passing across Virgo to-day and to-morrow, and at 2 a.m. on the 17th enters Libra. Her passage across this constellation occupies until 4 a.m. on the 19th, when she crosses into the narrow northern strip of Scorpio. Over this she travels in about 11 hours, and at 3 o'clock in the afternoon of the same day, leaves it for the southern part of Ophiuchus. She quits Ophiuchus for Sagittarius at 11 a.m. on the 21st. It takes her until 11 p.m. on the 23rd to cross Sagittarius into the north-western corner of Capricornus. She leaves this at 1 p.m. on the 24th to enter Aquarius, her journey across which constellation occupies until 7 a.m. on the 27th, at which hour she passes into Pisces. She is still in that great constellation when our notes terminate.

A BRUSSELS correspondent of the *Times* says it is seriously proposed at Antwerp to bring there during the Universal Exhibition in 1885 the *Great Eastern* as a floating restaurant and hotel.

* By a slip of the pen the opposition of Mars was erroneously stated on p. 75 to occur "at 11 o'clock to-night" (Feb. 1). This should have been 11 o'clock in the morning of that day.



"Let Knowledge grow from more to more."—Alfred Tennyson.

Only a small proportion of Letters received can possibly be inserted. Correspondents must not be offended, therefore, should their letters not appear.

All Editorial communications should be addressed to the Editor of Knowledge; *all Business communications to the* Publishers, *at the Office, 74, Great Queen-street, W.C.* If this is not attended to delays arise for which the Editor is not responsible.

All Remittances, Cheques, and Post Office Orders should be made payable to Messrs. Wyman & Sons.

The Editor is not responsible for the opinions of correspondents.

No communications are answered by post, even though stamped and directed envelope be enclosed.

MR. RUSKIN ON MODERN STORM CLOUDS.

[1116]—There seems to be a great deal more in a name than people imagine, or the theatre of the London Institution would scarcely have been crowded on Monday evening last to hear Mr. Ruskin lecture on "The Storm Cloud of the Nineteenth Century," that is if the lecture in question is to be taken as a criticism of his merit as a teacher of the public.

A printed report may be likened to a photograph—it reproduces, so to speak, but minus the tint and glow of colouring that may be imparted by gesture and inflexion. Denuded of these possible embellishments, and as sketched in the newspaper reports of the following day, Mr. Ruskin's lecture would appear to have been a very sorry affair indeed. He is reported to have "commented with some scorn on the scientific people's explanation of things, and on the inaccuracy of scientific people's terms." Scientific people are supposed to aim at the most scrupulous exactness and perspicuity in their selection of terms; but these conditions would scarcely recommend themselves to one who could say that he "spoke of a storm-cloud, or rather a plague-cloud, which had never been seen but by living and lately living persons." The inference here is that clouds other than this particular cloud may be seen over and over again. If one cloud may be seen over and over again, by what process of reasoning does Mr. Ruskin conclude that his particular cloud was *never* seen before?

Then, again, he saw a wind at Matlock in 1871, about which he is anxious to know a great deal, and goes on to say, "Scientific men could tell by this time all about the moon and the seven stars." (Why seven stars? Possibly he means the other seven planets of our system, but perhaps "planets" is one of the "terms" to which he objects), how they move, and what they are made of. He did not care a copper spangle how they move, or what they are made of, but he would like to know what this wind is whose "sound was a hiss, *or a whistling as on a flute made of a file.*" Then follows a little of Mr. Ruskin's word-painting for which he is so celebrated. This wind produced gloom. "The gloom was Manchester devil's darkness, sulphurous chimney-pot vomit." What *would be* the vomit of a "sulphurous chimney-pot"? Then because he wants to know all about this wind, Mr. Ruskin proceeds to complain that "an anemometer would give no information except as to the strength of the wind, and this shows the silliness of observing the *sky* with machines instead of with eyes," as if an anemometer could do more than indicate the strength of the wind. "If they wished to see how the sun looked through a plague cloud, let them throw a *bad* half-crown into a basin of soap and water." It is not very clear what all this means; possibly it is a protest against fogs (?); at any rate Mr. Ruskin's audience must have left the London Institution in a state of utter bewilderment as to what the lecture was about, or the *Daily News'* report, from which I have quoted, is the grossest piece of bungling that has appeared in print for some time. A. McD.

RED GLARE.

[1117]—In the many accounts of the brilliant sunsets and sunrises of the past few weeks I have not seen any reference made to the sky being of a ruddy colour at any other part of the day, and as I have noticed the red glow on two occasions when the sun was well above the horizon, it may be worth while to describe it. The first experience we had here of the brilliant sunsets was on the evening of Sunday, Nov. 25. On the same day, between eleven and twelve o'clock in the morning, I was watching the passage of a very low, black cloud across the sky, and when it had passed away I saw higher patches of rather jagged cumulus (cirro-cumulus). Behind one of these the sun was hidden, and surrounding it the sky was of a deep red colour (my wife called it "crushed strawberry"). The cloud moved out slowly, and the colour remained for a long time, until, in fact, that part of the sky became entirely overcast with low cumulus, and I did not again observe it. The same occurred on Tuesday, Dec. 18, at two o'clock, the sun again being hidden behind a patch of cirro-cumulus cloud, the sky surrounding which was of a deep red colour. On Nov. 25 the barometer was very low, the reading at nine a.m. being 28·82, while on Dec. 18 it read 30·40.

The light-green colour of the moon was also noticed here on December 3 and 4, and I was so struck with its appearance that I asked my assistant to look at the moon, and he also remarked upon its green colour. On Thursday (December 20), we had a remarkably brilliant sunset, the sky above a bank of cloud, which was passing slowly along the horizon from N.W., being first of a dark purple colour, and then vivid red. This produced a very beautiful effect on the road I was walking along, in a south-south-westerly direction, with the red sky in front of me. Rain was falling slightly, and had been all day, and the wet asphalted road appeared of a rich purple colour, while all the lights in the gas-lamps were green (not the usual colour of our gas). This was from half-past four to five o'clock. E. Howarth.

Sheffield, *Dec.* 22, 1883.

TAME UNCONFINED ROBINS.

[1118]—I have two beautiful red-breasted robins, named Bobby the First and Bobby the Second. They were hatched last spring in a garden which is separated from mine by a high hedge. During the winter and spring their father used to fly into my kitchen, a distance of about a hundred yards from his nest, to get food for himself and family. I always kept a good supply of crumbs and minced meat on a side-table, so that he could help himself, and I noticed that when he had little ones to feed he carried away five pieces each time, from which I knew that he had five mouths to fill.

As soon as his boys were able to travel, he brought two of them, by short stages, to me one day in the garden; he took crumbs out of my hand and fed them, and after saying something to them, which I am sorry I could not understand, he flew off and left them with me for a long time. He did the same every day until they were able to pick up, and then they came alone, whenever they saw me in the garden, and followed me about as long as I remained out.

Not wishing to disappoint the "Dear little Dots," I made a point of going out at stated hours every day to feed them, viz., at seven, nine, and twelve o'clock in the morning, and at three and five in the afternoon, and at sunset. During the whole summer and autumn they came punctually at the above-mentioned hours, never missing a time, nor coming ten minutes too soon or ten minutes too late. What I consider most remarkable is their knowing the time so accurately. I used to look at my watch, and I knew my little birdies would be waiting at the time I had taught them to come. If I went ten minutes before the time they were not there, and did not come when called.

On account of the dark mornings and evenings and the cold weather, I have not been able keep to the regular routine during the past two months; still, during the day I keep my little favourites as much as possible to their former habits. I simply go into the garden and say "Bobbie! Bobbie!" and at once Bobbie the First flies from a tree or shrub on to my knee, and sits and eats biscuit crumbs, sometimes, at my request, giving me a song while there, and Bobbie the Second flies from the hedge to my left side and picks up biscuit-crumbs off the skirt of my dress. They sometimes indulge in a little fight while being fed, when I part them with my hand, and they return to their places and finish their meal. They are the most friendly, brave, tractable robins I have ever seen.

A. Jarvis.

THE CRIBBAGE PROBLEM, BY H. H. H., p. 60.

E. C. H. makes 74, by the following arrangement:—Opponent 6, 6, 4, 4 (in order of play) dealer 6, 6, 4, 4; trump, 5; crib, knave (of trumps), 5, 5, 5.—J. A. Miles makes 61, or game in one hand, opponent making only 5; thus, Opponent 10, 6, kn., 8 in order of play; dealer, 5, 5, 5, kn. of trumps; trumps, 5; Crib, 4, 4, 6, 6.—J. H. H. makes 70; opponent 3, 3, 4, 4; dealer 3, 3, 4, 4; trumps, 5; crib the four knaves.—H. Tinden's first incomplete solution unfortunately misplaced; crib sent separately later.—C. F. Dunlee cites as occurring in an actual game, 29 made in play.—J. E. Willis makes 61 with 3 only to opponent (!) thus, opponent knave, 5, knave, 9; dealer 5, 5, 5, knave (trumps); trumps, 2; crib 9, 9, 10, 10.—

T. F. W. H. makes 73; opponent, 6, 6, 4, 4; dealer 6, 6, 4, 4; trumps, 5; crib, 5, 5, 5; knave (trumps).—E. C. H. later makes 75, thus: Opponent, 6, 6, 8, 4; dealer 6, 6, 4, 4; trumps, 5; crib knave of trumps, 5, 5, 5.—F. M. Duplock and A. B. tie for first place by making 78, thus:—Opponent, 4, 4, 3, 3; dealer 4, 4, 3, 3; trumps, 5; crib, knave of trumps, 5, 5, 5. Can this be beaten?

LETTERS RECEIVED.

E. H. Stritter.—H. J. M.—J. E.—E. W. P.—G. J. W.—F. P.—L. J. E. H.—C. H. J.—C. P. K.—G. C. G.—W. M. Walker.—E. C. Geach and H. P. B. Rigby. Mr. Proctor's opinion has been given in Knowledge.—J. W. Staniforth.—A. McD.—C. P. Kemball and R. M. Fuller. Mr. Proctor's lectures are not published.—E. Munroe.—W. Whitaker.—A. C. Bruce.—R. H. Batchelor.—An Amateur. I think 1878. What opposition of Mars do you mean?—A. E. F. Think I remember such an article by W. Cave Thomas. Cannot tell; am away from India.—J. Whitelaw.—Curmudgeon. The editor might, if disposed to be as keen as you, say you *have* sent your true name. As a matter of fact, he merely notes that it is better, not necessary; and it is not usual when a correspondent has something specially disagreeable to say.—C. Moon. Thanks for kindly letter, and the hearty laugh your closing story gave me.—C. Carrie Wilson. A flat disc, arctic pole at centre, antarctic ice at circumference.—W. T. H.—C. Pearson.—H. B. Heath.—L. C. W.—H. Ritchie.—J. J. C. Valpy.—E. Y.—L. A. E. You ask a little too much.—C. E. Fletcher.—J. O. Lindsay.—T. Duplock.—C. H. Johns. Thanks; but the action would be too slow. The aërial comets known to exist would do the work efficiently.—W. Fitzgerald. Thanks for much better than Whistlerian sketch. But that could not have been Pons' comet. Yet not a Pons' asinorum as you pathetically suggest. Have not yet seen any reference to Zechariah's prophecy, "At evening time it shall be light."—D. C. My good sir, you are somewhat *too* cool. When Knowledge was first started, we were "living on hope"—the hope that advertisements would grow, as in fact they have grown. Till they so grew we had perforce to fill twenty pages out of twenty-four—much oftener than we liked—with what you call "readable matter." "Some time ago" you "had pen in hand to protest against the increase of advertisements,"—well as a matter of fact you had pen in hand ("all unbeknown") to protest against the existence of Knowledge. Thanks so much.—T. B. Smith. Why ask me to explain all that when you go on to show you know so much more about it than I do, or than any one living knows?—M. Poole. Why not write a book or take the last answer; and let T. B. S. take this.—J. Clayton. Why that's Newton's first law. See Newton's "Principia" thereon. You find the best proof in the planetary movements. Mr. Newton Crosland thinks (I fancy) the law is wrong; for he asks somewhere what keeps the velocities up? But then Newton C. is not Newton. See!—Jas. Ellis. Ah, but you see my friend happens to be a gallant and chivalrous gentleman who speaks from knowledge as well as in Knowledge. So many of my own best friends and kinsfolk are of Irish blood, that the idea of defending as *Irish* those whom he has roundly denounced seems to me offensive in the extreme. One might as reasonably defend the inmates of Newgate as *English*.

SUB-EDITORIAL.

Constant Subscriber.—John Hall. The eye-piece of a telescope *is* a microscope.—F. R. C.—P. H. Farady.—R. W. Browne. Thanks for very kind letter.—And. Aitken. M.S. on subject completed.—J. Edwards.—T. M. D.—H. I.—R. Irvine.—John Peers.—S. M. B.—Eye-Witness. You surely cannot see any parallelism between the paper you name and Knowledge; the two papers are intended for readers of entirely different classes. Supposing we started a series of papers on "The Organ: its Procural and Lodgment;" on "Plumbing in all its Departments;" on "The Screw-driver: its Development and Employment," we might fill the space you mention at no cost at all, or even be paid for allowing pictures to appear in illustration of valuable patents. That is quite outside our plan and purpose; and however suitable, even desirable, in its proper place, would be a gross wrong to readers whom our promises entitle to expect something very different. For editing, one number of Knowledge costs more than five numbers of the other paper.—Longworth.—Jupiter.—W. Grandy.—Lucretius. Do not know.—J. N. Kirby. Thanks.—J. A. Gee. Many thanks. It seems to me a series of harmonographic curves, systematically arranged, with short but sufficient explanation, would be very interesting. To serve as copy the curves should be in black ink.—O. Rohde.—An Old Subscriber.—S.—Tom Fred. I should say,—the boys in smaller room with the window open at top.—W. H. T.—G. G. G.—E. Anderson.—W. H. M.—W. J. F.—Accumulations.—G. W. H. G.—S. Stewart.—C. W. B.—W. H. B.—J. Matthews. Knowing by effects that there is a cause, is very different from knowing what that cause is. Persistent force shows there is a First Cause, but the nature of that cause remains "Unknown, yea ever more Unknowable."—A. McD.—C. Smith.—W. H. P.—D. C. Thanks.—T. J. O'Connor. The value of The New Principles of Natural Philosopy may be summed up in one word—"Bosh!"—H. A. B. Very little is known about the origin of the Zodiacal signs, or the time when they were first used. Mr. Kinn's views are scarcely worth considering; he offered us a paper, expressing what he has since more fully embodied in a book: but his reasoning was so utterly futile that we declined-with-thanks.

Our Whist Column.

By "Five of Clubs."

PROBLEMS, p. 62.

Simple Ending No. 1.

Y's cards. *C K*; H Kn; S Kn, 2; D 9.	B's cards. *C A*, 5; S 4; D 4, 2.
A's cards. H A, 10; S 10; D Kn, 7.	Z's cards. H K, 5; S 6; D 10, 5.

Clubs trumps; *A* to lead; *A B* to make all five tricks.

Simple Ending No. 2.

Y's cards. *C* 10, 7; H A, Kn; S Q, 10, 2.	B's cards. H K, 7, 4; S K, 9; D A, Q.
A's cards. *C A*, Kn; H 6; S 4; D 10, 9, 8.	Z's cards. *C* 9, 6; H 9, 8; S 8, 7, 6.

Clubs trumps; *A* to lead; *A B* to make all seven tricks.

Simple ending No. 1 is correctly solved by VAENOL, Q. T. V., Hiero, J. M., C. E. Bell, Ruff, H. J. Ph., Old Westminster, Cousin Jack, M. Hartington, and J. Scantling. The second, and slightly more difficult problem, by VAENOL, J. M., C. E. Bell, Ruff, H. J. Ph., and Old Westminster. The solutions are as follows:—

1st Ending.—*A* leads Ace Heart, *B* discarding Spade Four; *A* leads Spade Ten; *B* trumps if necessary (the play being simplified if he has not to), leads winning trump, and the remainder of the hand plays itself according to the discards.

2nd Ending.—*A* clears out trumps, *B* discarding his Diamonds; *A* continues with his long Diamonds, and *B* wins the remaining tricks, his play varying according to the discards, but being obvious in every case.

H. Dennys.—It is not very unusual to see *Slam*, or all the tricks won by one side. It would be rather difficult to determine even roughly the odds against the occurrence. We should say it happens about as often as a Yarborough hand,—that is, a hand with no card above a nine.

H. Lewis.—Many thanks; but you rather misunderstood our note.—On trick 11 (p. 91), we note "*Z* luckily holds the minor tenace in Clubs," and (12, 13) "makes the last two tricks and *Y-Z* win." It was so clear that holding the minor tenace *Z* could not possibly win two tricks against the major tenace guarded, that we did not think it necessary to dwell on *B*'s generosity in giving *Y-Z* the game by leading the Queen. We were rather cramped for space, or the note would have dwelt on the badness of *B*'s play as well as on *Y-Z*'s good fortune. For a similar reason (and because *Y*'s play more concerned us) we did not dwell on *B*'s bad play at trick 9, in leading Club Ace, when it has become practically certain that *Y* has not the Club 10, and that *A* has the Club K. Our note on trick 11 might have run, "*Z* holds the minor tenace in Clubs, and as *Z* is good enough to make it the major tenace by leading the King card in that suit, *Z* makes the last two tricks and *A-B* win."

A. C. M.—So long since I played "long Whist" that I forget the rules about "Can you one, partner?" My impression is that the question could not be asked until the first trick has been completed. If playing first card stopped the question, leader might by hurrying deprive the opponents of their right before either knew whether he had two honours or not. By the way what a ridiculous law it was, any way. As to the other question there is no doubt: honours can be counted if claimed, or even asked about before the trump card is turned.

Coventry.—No player of any repute known to me leads (as a rule) Ace, from Ace, King, three or more. The argument that partner if he has but two trumps might be tempted to ruff is invalid. For even if partner *knew* that the Ace lay on his left, he would as a rule do ill to ruff. Defend us from a partner who systematically blocks our long suit by ruffing. He makes his wretched little trump, which probably he could have made just as easily in the same suit, later on; and when a chance for bringing in the long suit comes, the enemy with his King card nips the suit in the bud.

Our Chess Column.

By Mephisto.

KING'S GAMBIT.

(*Continued from p. 78.*)

1. P to K4 / P to K4 2. P to KB4 / P takes P 3. Kt to KB3 / P to KKt4
4. B to B4 / B to Kt2 We will now examine the continuation of 5. P to KR4,

Black's best reply is 5. P to KR3, *i.e.*—

5.	P to KR4 / P to KR3		
6.	P to Q4 / P to Q3		
7.	Q to Q3 / Kt to QB3		Kt to B3 / P to QB3
8.	P takes P / P takes P		P takes P / P takes P
9.	R takes R / B takes R		R takes R / B takes R
10.	P to K5 / B to Kt2, or	——— / P to Q4	Kt to K5 / P takes Kt
11.	Kt to B3 / Kt to R3	Q to R7 / K to B sq.	Q to R5 / Q to B3
12.	P takes P / P takes P	Q takes B / P takes B	P takes P / Q to Kt2
13.	Kt to Q5 / K to B sq.	P to B3 / B to Kt5	P to K6 / Kt to B3
14.	Kt takes KtP / Q takes Kt	QKt to Q2 / Q to Q5	P takes P (ch) / K to K2
15.	B takes P / Q to R5 (ch)	Q to R sq. / B takes Kt	Q to K2 / B to Kt5
16.	P to Kt3 / Q to R8 (ch)	P takes B / R to K sq.	Q to Q3 / QKt to Q2

In all these variations Black maintains his advantage. It will be seen that the chief attack is gained by either attempting to play the Q to R7 *viâ* Q3, or to play Q to R4 after exchange of Rooks; this has to be guarded against. Black may also play K to B sq., followed on Queen playing to R7 by B to Kt2. Another variation of the same attack is—if White on his seventh move plays P takes P, *i.e.*—

7. P takes P / P takes P 8. R takes R / B takes R 9. Q to Q3 / Kt to KR3
10. P to KKt3 / Q to K2 11. Kt to B3 / P to QB3 12. P takes P / P to Kt5
13. Kt to KKt5 / Kt to Q2 14. P to K5 / Kt to B3 15. B to K3 / P to Q4
16. B to Kt3 / Kt to B4 17. Castles / Kt to Kt sq.

Should Black on his fifth move play P to Kt5, White would reply as follows:—

5. P to KR4 / P to Kt5 6. Kt to Kt5 / Kt to KR3 7. P to Q4 / P to KB3

With proper play White ought always to get a good game from this sacrifice. The position can be brought about in various ways, as for example, by

1. P to K4 / P to K4 2. P to KB4 / P takes P 3. Kt to KB3 / P to KKt4
4. P to KR4 / P to Kt5 5. Kt to Kt5 / Kt to R3 6. B to B4 / B to Kt2
7. P to Q4 / P to KB3

and we have the same position which will, as aforesaid, occur also in various other ways. White will continue with 8. B takes P / P takes Kt 9. B takes KtP (P takes P, followed by Q to Q2, likewise yield a strong attack) 9. ——— / B to B3 10. B takes Kt / B takes P (ch) 11. K to Q2 / B to Kt4 (ch) 12. K to Q3 with no disadvantage.

ANSWERS TO CORRESPONDENTS.

⁂ Please address Chess Editor.

H. S. L.—End game received with thanks.

M. D. Dimond.—Experience has proved that in a tournament with a small number of players it is best for every man to play one game with every other competitor, draws to count half to each player. You must beforehand draw every member against each other, to ensure regular play. If you find this task too much, you will find an excellent method for pairing all players beforehand indicated in No. 70 of Knowledge.

E. Anderson.—"Positions in the Chess Openings," by Long, is an excellent book for beginners.

Old Romney, W., M. T. Hooton.—Solutions correct.

Contents of No. 119.

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CONTENTS OF No. 121.

GAMBLING SUPERSTITIONS.

By RICHARD A. PROCTOR.

(*Continued from page 80.*)

BUT it may not seem quite so easy to explain those undoubted runs of luck, by which players "in the vein" (as supposed) have broken gaming-banks, and have enabled those who have followed their fortunes to achieve temporary success. The history of the notorious Garcia, and of others who like him have been for awhile the favourites of fortune, will occur at once to many of my readers, and will appear to afford convincing proof of the theory that the luck of such gamesters has had a real influence on the fortunes of the game. The following narrative gives an accurate and graphic picture of the way in which these "bank-breakers" are followed and believed in, while their success seems to last.

The scene is laid in one of the most celebrated German Kursaals.

"What a sudden influx of people into the room! Now, indeed, we shall see a celebrity. The tall, light-haired young man coming towards us, and attended by such a retinue, is a young Saxon nobleman who made his appearance here a short time ago, and commenced his gambling career by staking very small sums; but, by the most extraordinary luck, he was able to increase his capital to such an extent that he now rarely stakes under the maximum, and almost always wins. They say that when the croupiers see him place his money on the table, they immediately prepare to pay him, without waiting to see which colour has actually won, and that they have offered him a handsome sum down to desist from playing while he remains here. Crowds of people stand outside the Kursaal doors every morning, awaiting his arrival, and when he comes following him into the room, and staking as he stakes. When he ceases playing they accompany him to the door, and shower on him congratulation and thanks for the good fortune he has brought them. See how all the people make way for him at the table, and how deferential are the subdued greetings of his acquaintances! He does not bring much money with him, his luck is too great to require it. He takes some notes out of a case, and places maximums on *black* and *couleur*. A crowd of eager hands are immediately cut-stretched from all parts of the table, heaping up silver and gold and notes on the spaces on which he has staked his money, till there scarcely seems room for another coin, while the other spaces on the table only contain a few florins staked by sceptics who refuse to believe in the Count's luck." He wins; and the narrative proceeds to describe his continued successes, until he rises from the table a winner of about one hundred thousand francs at that sitting.

The success of Garcia was so remarkable at times as to affect the value of the shares in the *Privilegerte Bank* ten or twenty per cent. Nor would it be difficult to cite many instances which seem to supply incontrovertible evidence that there is something more than common chance in the temporary successes of these (so-called) fortunate men.

Indeed, to assert merely that in the nature of things there can be no such thing as luck that can be depended on even for a short time, would probably be quite useless. There is only one way of meeting the infatuation of those who trust in the fates of lucky gamesters. We can show that, granted a sufficient number of trials—and it will be remembered that the number of those who have risked their fortunes at *roulette* and *rouge et noir* is incalculably great—there must *inevitably* be a certain number who appear exceptionally lucky; or rather, that the odds are overwhelmingly against the continuance of play on the scale which prevails at the foreign gambling tables, without the occurrence of several instances of persistent runs of luck.

To remove from the question the perplexities resulting from the nature of the above-named games, let us suppose that the tossing of a coin is to determine the success or failure of the player, and that he will win if he throws "head." Now if a player tossed "head" twenty times running on any occasion it would be regarded as a most remarkable run of luck, and it would not be easy to persuade those who witnessed the occurrence that the thrower was not in some special and definite manner the favourite of Fortune. We may take such exceptional success as corresponding to the good fortune of a "bank-breaker." Yet it is easily shown that with a number of trials which must fall enormously short of the number of cases in which fortune is risked at foreign Kursaals, the throwing of twenty successive "heads" would be practically *ensured*. Suppose every adult person in Britain—say 10,000,000 persons in all—were to toss a coin, each tossing until "tail" was thrown; then it is practically certain that several among them would toss twenty times before "tail" was thrown. Thus: It is certain that about five millions would toss "head" once; of these about one-half, or some two millions and a-half would toss "head" on the second trial; about a million and a quarter would toss "head" on the third trial; about six hundred thousand on the fourth; some three hundred thousand on the fifth; and by proceeding in this way—roughly halving the numbers successively obtained—we find that some eight or nine of the ten million persons would be almost certain to toss "head" twenty times running. It must be remembered that so long as the numbers continue large the probability that *about* half will toss "head" at the next trial amounts almost to certainty. For example, about 140 toss "head" sixteen times running: now, it is utterly unlikely that of these 140, fewer than sixty will toss "head" yet a seventeenth time. But if the above process failed on trial to give even one person who tossed "heads" twenty times running—an utterly improbable event—yet the trial could be made four or five times, with practical certainty that not one or two,

but thirty or forty, persons would achieve the seemingly incredible feat of tossing "head" twenty times running. Nor would all these thirty or forty persons fail to throw even three or four more "heads."

Now, if we consider the immense number of trials made at gambling-tables, and if we further consider the gamblers as in a sense typified by our ten millions of coin-tossers, we shall see that it is not merely probable, but absolutely certain that from time to time there must be marvellous runs of luck at *roulette*, *rouge et noir*, *hazard*, *faro*, and other games of chance. Suppose that at the public gaming-tables on the Continent there sit down each night but one thousand persons in all, that each person makes but ten ventures each night, and that there are but one hundred gambling nights in the year—each supposition falling far below the truth—there are then one million ventures each year. It cannot be regarded as wonderful, then, that among the fifty millions of ventures made (on this supposition) during the last half century, there should be noted some runs of luck which on any single trial would seem incredible. On the contrary, this is so far from being wonderful that it would be far more wonderful if no such runs of luck had occurred. It is probable that if the actual number of ventures, and the circumstances of each, could be ascertained, and if any mathematician could deal with the tremendous array of figures in such sort as to deduce the exact mathematical chance of the occurrence of bank-breaking runs of luck, it would be found that the antecedent odds were many millions to one in favour of the occurrence of a certain number of such events. In the simpler case of our coin-tossers the chance of twenty successive "heads" being tossed can be quite readily calculated. I have made the calculation, and I find that if the ten million persons had each two trials the odds would be more than 10,000 to 1 in favour of the occurrence of twenty successive "heads" once at least; and only a million and a half need have a single trial each, in order to give an even chance of such an occurrence.

(*To be continued.*)

CAUSES OF THE GLACIAL PERIOD.

(*Continued from page* 39.)

HAVING considered in our last article the more probable astronomical causes, we will now proceed to discuss the remaining geographical and other causes which have been alleged as contributing to bring about the great Glacial Period, dealing, in the first instance, with the probable geographical causes.

1. *Distribution of Land.*—The effect of land upon climate is in tropical countries to increase the heat and in arctic ones to increase the cold. During summer also the effect of land is to increase the temperature, and during winter to decrease it. Hence it is, that the land in Arctic regions usually possesses a much more severe climate than the adjoining seas, frozen though these may be. Owing to such being the case, Sir Charles Lyell, the eminent geologist, was led to the belief that the Glacial Period was principally due to an abnormal condition in the distribution of the land at that time. He was of opinion that an excess of land in polar regions (combined with an absence of land in equatorial regions) was the chief cause which led to the Glacial Period. For instance, he considered that both Europe and America might at that time have been united to Arctic regions by large tracts of land now submerged, and that a great extent of land then existed round the North Pole. Now, although such a state of things would, in all probability, have contributed considerably to a glacial climate, there seems to be scarcely evidence sufficient to warrant an assumption that the distribution of land was so very different during the Glacial Period to what it is now. With the exception of the union of England to the Continent during part of that period, and the somewhat further extension of the European continent westwards, proofs are wanting of any very important difference between the geography of that time and this, and the differences of which we have evidence do not justify us in setting them down as the chief cause of the Glacial Period.

2. *Elevation of Land.*—Altitude or elevation of land has also a great effect upon climate, its effect being always to decrease the temperature. Some have supposed, therefore, that during the severity of the Glacial Period the land in the Northern Hemisphere was more elevated above the sea level than at present. But, although there is abundant evidence in England of a submergence during that period—witness the occurrence of Arctic shells on some of the Welsh mountains—there is not sufficient evidence of a general elevation very much exceeding the present state of things. Consequently, this supposition suffers in the same way as the last one discussed; and, although both may have been favourable to the occurrence of a Glacial Period, neither can be said to have been demonstrated as the primary cause of the same. Moreover, we have seen that mild intervals in all probability occurred during the Glacial Period, and the jumping up and down of land on purpose to explain these alternate mild and severe intervals of climate, is a somewhat violent supposition.

3. *Winds.* The southern part of Europe is at present warmed considerably by the south winds blowing from the hot desert of the Sahara. But the Sahara bears signs of having been only (geologically speaking) a recent upheaval of the bed of the sea. Consequently, formerly the south winds would not be charged with the burning heat they have now, and would not possess the power they now have of melting the snow on the mountains, and stopping the advance of the glaciers of Switzerland and Italy. The absence of these warm winds could not fail to contribute to the severity of the climate of Europe.

4. *Currents.*—Of more importance to our climate than winds are ocean currents, especially that most considerable of all ocean currents, the Gulf Stream. Our readers will probably be aware that there is a great difference between the climate of Europe and North America in the same latitudes. For instance, the harbour of St. John, Newfoundland, which is often blocked by ice, not only in winter, but as late as May and June, is actually further south than the principal seaports of England and France which enjoy open harbours all the year round. Boston, in America, possesses only the same mean annual temperature as Dublin, although it is 11° further south; while that part of North America in the same latitude as the British Isles comprises the desolate and ever-frozen regions of Labrador and the Hudson's Bay territory. This great difference between the climate of the east and west shores of the Atlantic is due in a great measure to the warm waters of the Gulf Stream poured upon the shores of England and Ireland, so that it is easy to see what a vast difference would be made in our climate if by any means the Gulf Stream were diverted. Now, the great central valley of the Mississippi is nowhere more than 800 ft. above the sea level, and it has been urged that if this valley were lowered a direct course would be given to the Gulf Stream up the centre of North America; also, that if the Isthmus of Panama were submerged, its course would also be

diverted, and, in either case, that England and Western Europe would thereby be reduced to a condition of climate similar to that of Hudson's Bay, Labrador, and Newfoundland at the present day—in fact, so great would be the results of an alteration in the course of the Gulf Stream upon the climate of Europe, that some eminent writers have considered that that alone would amply account for the Glacial Period. But we must not forget that evidences of such a period exist elsewhere than in Europe, and that even in America marks of glacial action are found 10° further south than in Europe, which fact points to relative conditions of climate in the two continents similar to those now subsisting, and also tends to show that the Glacial Period *in both* was due to general rather than local causes. Moreover, as I have already hinted, a glacial condition of the northern hemisphere caused by the northern winter occurring in aphelion during a period of great eccentricity of the earth's orbit would probably induce various changes in the winds and currents of the globe; so that instead of such changes causing the Glacial Period, as some have contended, the Glacial Period might, on the contrary, have caused them. However this may be, it is certain that such changes, however caused—especially changes in the direction of the Gulf Stream—would, in a very considerable degree, augment the rigour of the glacial climate.

We have now considered the causes, both astronomical and geographical, which most probably led to the Glacial Period; but, as I have before said, it was in all probability a combination of those causes, rather than any one of them, which ultimately brought about such a period in the northern hemisphere. It only remains for us, therefore, in order to render more complete our study of the causes of the Glacial Period to notice briefly two or three other causes which have with less probability been assigned by some.

1. *Changes in the Inclination of the Earth's Axis*, or what amounts to the same thing, in the "*Obliquity of the Ecliptic*," the latter being, of course, dependent upon the former. The axis of the earth at present is inclined to the plane of the orbit or the ecliptic at an angle of 23° 28′; but owing to the attraction of the other planets this inclination of the axis or obliquity of the ecliptic is perpetually varying within certain limits. The effect of this upon climate would be that whenever the angle of obliquity was less than now, the Arctic regions would be more exposed to the long and cold winters in aphelion, and whenever the angle of obliquity was greater the opposite would be the case; so that combined with great eccentricity of the earth's orbit and the other causes I have mentioned, the result of such obliquity would be to increase or mitigate (according to the amount of such obliquity) the severity of climate. As, however, the limits within which variation in the inclination of the earth's axis or obliquity of the ecliptic can take place are very narrow, its effects on climate are probably next to imperceptible.

2. *Supposed Diminution of the Sun's Heat* and *Supposed Passage of the Earth through Cold Regions of Space.* Both these alleged causes are purely conjectural. They involve, moreover, a transparent fallacy. For in either case the temperature of the earth would be lowered generally, and in such a state of things it is difficult to see how ice and glaciers could be formed on land. For we must remember that it is the heat of the sun which draws up the aqueous vapour from the ocean, which vapour afterwards falls in cold climates as snow, and is frozen into glaciers and ice; so that if the sun's heat were in any way diminished, aqueous vapour would not be drawn up as now, and so the very material of which glaciers are formed would be cut off at its source. Thus, although a cold condition of the earth generally might freeze the sea to the bottom, it could never cause a glacial state of things *on land*. Hence we learn the great truth that, paradoxical though it may appear, glaciers and land ice owe their formation ultimately to HEAT, for if there were no heat, or not sufficient heat, the aqueous vapour could not be raised as now from the ocean, and without aqueous vapour there could be no snow, no glaciers, and no land ice. Consequently, we may dismiss these last two alleged causes as altogether worthless, and fall back upon all or some of the causes previously referred to, it being perfectly plain that a diminution of the sun's power, or the passage of the solar system through a space of low temperature, would destroy the glaciers at their source. We therefore see by ultimate analysis of the explanations we have examined that the conditions necessary to produce a Glacial Period are *a short, hot summer and a long, cold winter, with plenty of aqueous vapour caused by the heat, to be converted by the cold into snow and ice.*

ROBERT B. COOK.

PLEASANT HOURS WITH THE MICROSCOPE.

By HENRY J. SLACK, F.G.S., F.R.M.S.

IT is a good plan to keep some wide-mouthed bottles, holding two or three ounces, in a warm, light place to develop the various germs that are sure to be found in the water of a duck-pond, or any other promising source. Larger vessels are likely to yield the greatest variety with hay or other vegetable infusions, but the smaller ones are very handy, and may have the chance of their fertility increased by a few drops of soup or little bits of lean meat, which last favour the development of amœbæ. Often, after several weeks or months, quite unexpected and curious arrivals are found in the bottles, and if a drop looks promising when viewed under the microscope, arrangements should be made to give a whole evening to the study. Now, whether looking through a microscope for hour after hour is fatiguing and eyesight-damaging or not depends upon some points easily attended to.

The usual position of a microscope with a tube slanting a little and the head leaning forward to look down it, is all very well for a short examination of any object, but not at all desirable for continuous work. A better plan is to get a carpenter to make a light stool 2 ft. long and 14 in. wide, standing on four legs, the length of which should be determined by that of the microscope it is intended to use and the height at which the observer sits. My stool is 7 in. high, and when placed on an ordinary table brings a full-sized microscope with its tube in a horizontal position, at a convenient height for the eye of an observer sitting in an ordinary chair. The late Mr. Lobb, who was skilful in exhibiting troublesome objects, always used his microscope in this position, but, so far as I know, it is seldom adopted. When the instrument is in position as described, the substage mirror should be turned out of the way, and the lamp placed so that its flame is exactly opposite the axis of the instrument, and can be seen in the middle of the field on looking through it. If the objects to be watched are large enough for a low power, the light may be softened by placing under the slide a piece of foreign post-paper saturated with spermaceti. For high powers an achromatic condenser is desirable, and one of the smallest central stops is usually the most useful for displaying fine cilia, or delicate whips, as well as for

lighting up without glare the interior of various creatures. If all is arranged properly, the manners and customs of infusoria may be watched for hours without more fatigue than reading a well-printed book. A tenth part of the time spent with the head leaning forward in the usual way is far more exhausting.

On a recent occasion a slight examination of some water showed that it contained a number of small creatures moving with whips, and that a high power was required to show them well. That employed was a fine oil immersion $\frac{1}{12}$th of Leitz,—an excellent and easily-managed glass. Amongst the most conspicuous objects were round and roundish creatures, relations of the beautiful emerald-coloured, ruby-eyed, and fish-shaped *Euglenæ*. They were *Trachelomonads*, some in smooth shells, others in shells with short spines, something like those on the outer case of chestnuts, but blunter. The specific names *cylindrica* and *hispida* correspond with these peculiarities. The contents of the shells were of an exquisite emerald-green colour, with ruby eye-spots—not really eyes. Each shell was like a bottle with a short neck, out of which came a long, thin whip. The optical edges of the shells looked a fine red colour. Of course, they were not real edges at all, but only perspective terminations of the view, and the colour was produced by the thickness of the material through which the light passed. These creatures can at some period of their existence get out of their bottles, squeezing through the neck, but that did not occur in the case before us.

The interest lay in noting the use of the wonderful whip. Some of them used it to keep up a jerking motion without any progression. The whips were then either lashed close round the shell, or thrown forward and then retracted. In most instances, the motion was what would be expected from giving the water a push in one direction, and the little bottle-imp moving in an opposite one. There were, however, some exceptions, and one creature jerked himself a little forward by throwing his whip straight out, and being, as it appeared, more affected by its momentum than by the reaction of the water. The most curious thing occurred with one of the smooth sort, which twirled rapidly round, like a whipping-top, for a quarter of an hour with a velocity that converted its conspicuous dots of colour into circular lines, just as a stick alight at one end makes, when quickly whirled, a ring of fire. As the whips of these creatures look like very fine hairs when magnified a thousand times, this performance with such an instrument is truly wonderful.

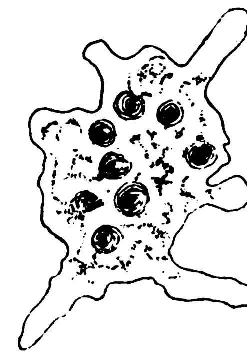
What are these things? They were formerly thought to belong to the plants, but Mr. Saville Kent, in his splendid work,* agrees with those who regard them as animals. His description runs as follows:—

TRACHELOMONADS: Ehrenberg.—Animalcules, monoflagellate, plastic, and changeable in form, enclosed within a free-floating ovate or spheroidal indurated sheath, or lorica; the anterior extremity of the lorica perforated by a minute aperture, through which, in its normal condition, the single flagellum only is protruded; oral aperture terminal, followed by a distinct pharyngeal passage; endoplasm coloured green, with usually a red pigment spot at the anterior extremity; contractile vesicle single, spherical, located near the anterior pigment spot. Mostly inhabiting fresh water.

After the slide, with its water-drop containing the Trachelomonads, had been an hour or two under the microscope, a minute rapidly-form-changing *amœba* made its appearance. Most of these animated lumplets of jelly are sedate in their movements. This was not for two seconds of the same shape, but actively putting out pseudopodia of hair-like tenuity, first from one part, and then from another, the main substance flowing after them. Presently there came out of a mass of slime and *débris* a much larger and less excitable specimen, acting with a deliberation that contrasted curiously with its more lively and versatile companion. The *amœbæ* always throw out their pseudopods as clear protoplasm, and then the granular portions, with their multifarious contents of captured prey, flow, or slobber, after them. Watching their doings shows that they act with *intention*, though no one would add the epithet of "conscious."

The bottle of water contained several individuals of another remarkable infusoria, which will be described in the next paper. Subjoined are figures of a *trachelomonad*, and of the larger of the two *amœbæ*.

Trachelomonad.



Amœba. ×600.

* "Manual of the Infusoria."

HOW TO MAKE USEFUL STAR MAPS.

By RICHARD A. PROCTOR.

(*Continued from page 37.*)

IN my last paper I mentioned that when the meridians and parallels of a map had been drawn in according to the simple plan I described, the stars could be filled in from the star-atlas or from any convenient catalogue. I propose now to dwell a little on the peculiarities which render the map constructed on the conical projection a better representation of the particular part of the heavens it exhibits than the corresponding part of a star-atlas can commonly be.

Of course, in the first place, there is the advantage of scale. We can make a representation of a small part of the heavens cover a sheet as large as that used in the star-atlas for a large portion of the celestial sphere. And if star-atlases were constructed on well-conceived plans this would be the only advantage we could get from making maps of separate constellations. Even then such maps would be very useful, because there would be plenty of room in them to pencil down any notes which the amateur might wish to record about a star, or again to track down the course of a comet from the places published in the daily journals, and independently of that, the construction of such maps is a most useful exercise for the beginner, and will, in the long-run, amply repay him for any time he may give to the work.

But there are other and much more important reasons why the construction of such maps as I have described is not only useful but necessary.

That the celestial sphere can be divided into a convenient number of maps, similar in size and shape, and without

any distortion or change of scale worth noticing, I have shown in my "Library Atlas," and School Star-Atlas." No series of maps before published in the form of an atlas satisfied those simple conditions.

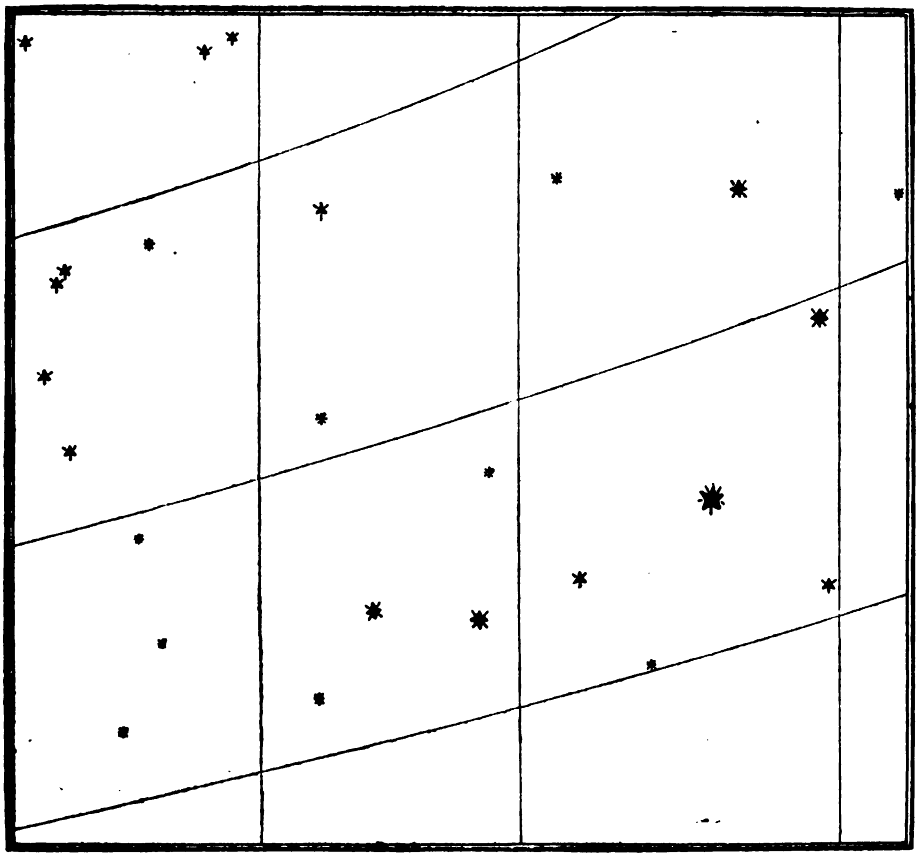


Fig. 5.

Consider, for instance, the large maps published by the Society for the Diffusion of Useful Knowledge, in 1850. These were planned with great skill, to fulfil a condition which many hold to be of the utmost importance for star maps. It was proposed that the heavens should be so exhibited that any three stars which appear to lie in the same straight line on the heavens (or really on what is called a great circle of the sphere), might also lie in a straight line on the maps. The projection which gives this relation is called the gnomonic projection. The heavens were divided into six star-maps, and a simple notion of the plan made use of will be obtained by conceiving the celestial sphere enclosed within a cubical box, and each star marked on a face of this box by drawing a line from the centre of the sphere through the star until it met the corresponding plane face. The maps were made on the scale of a globe two feet in diameter, and they were constructed with an amount of care and a fulness of detail which rendered them, in spite of the inherent defects of the mode of projection adopted, a most valuable series for any one who could interpret their more distorted portions.

Now the stars in Fig. 5 have been pricked off from the S. D. U. K. map. The part of the heavens represented is the well-marked group of stars forming the Northern Crown, which probably formed in very ancient planispheres the uplifted right arm of Boötes—the Herdsman. This part of the heavens does not fall on the most distorted part of the S. D. U. K. map, yet it will be seen at once how largely the shape and position of the spaces inclosed between the meridians and parallels has been distorted. Even in the small space covered by the map, the scale changes perceptibly, as is shown by the rapid increase in the width of the vertical spaces, and by the acuteness of the angles which ought to be right angles.

But the full extent of the distortion can only be recognised by comparing the actual aspect of the star-group, exhibited in Fig. 6, with its distorted presentation in the S. D. U. K. map. In Fig. 6 the scale is precisely the same as in Fig. 5, each map being on the scale of a 2 ft. globe. But Fig. 6 being laid down on the conical projection—that is, according to the simple plan which I have described—there is no appreciable distortion in it; we see all the spaces between the same parallels similar in shape. All the angles are appreciably right angles, and everything, in fact, is clear and intelligible.

Now, on comparing Fig. 6 with Fig. 5, we see immediately that the whole group of stars in Fig. 5 has been distorted. The stars form a sort of spiral in both figures, commencing at the lower left-hand corner, going thence to the upper left-hand corner, next to the upper right-hand corner, and so (by continuing the sweep) into the middle of the map. But whereas in reality the bend at the upper left-hand corner is somewhat acute, the S. D. U. K. map represents it as obtuse; while the almost circular sweep repre-

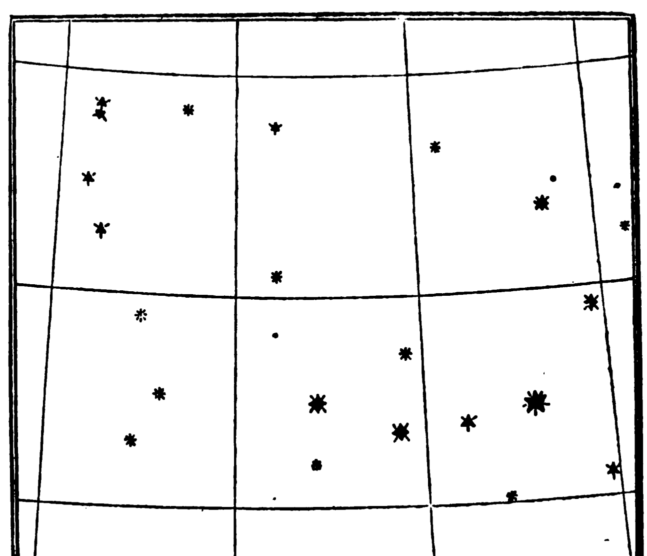


Fig. 6.

sented on the right of Fig. 6 (omitting four small stars), is changed in Fig. 5 to quite another figure.

I do not wish to be understood as in any way finding fault with the S. D. U. K. maps; unfortunately no series of maps can have every good quality we can desire. If we wish to preserve the straight-line pointing, which is the essential property of the S. D. U. K. maps, we must sacrifice some other properties. That I believe that property to be very valuable for popular maps, I have shown by the construction of my own series of gnomonic maps, in which the heavens are divided into twelve instead of six equal portions, and so more than three-fourths of the distortion saved. But in all gnomonic maps, except very small ones, there must be appreciable distortion; and we see at once, therefore, the necessity of supplementing them, so to speak, by the construction of undistorted maps of all the principal star groups.

(To be continued.)

A YEAR'S WEATHER FORECASTS.

By John W. Stainforth.

PART I.—THE WIND.

It has been said that the amount of work done in connection with the science of astronomy varies inversely as the amount of instrumental means possessed by the worker. If this be true of astronomy it is equally true of meteorology, and I think it will be granted that in testing the "Weather Forecasts of the Meteorological Office," and indicating their weak points, there is a field of useful work before those who are not even possessed of what are usually regarded as indispensable adjuncts of all meteorological observation—a barometer and a thermometer. Such is the course I have adopted during the past year, and it is the object of this paper to place before the readers of KNOWLEDGE the results obtained.

Bearing in mind that the Meteorological Office confines its attention, as a rule, to the wind's direction, the wind's force, and the general state of the weather (rainy, fair, foggy, &c.), it will be obvious that in order to ascertain the value of the forecast it was necessary to obtain a trustworthy record of these several atmospheric conditions. I shall, therefore, deal with them under their separate heads.

I. *The Wind's Direction.*—This was ascertained by means of observations of nine vanes which were passed at different periods of every day, and by noting the run of the lower clouds when no vanes were in sight. The result so obtained was checked by means of the very complete Meteorological Reports published by our two Sheffield papers, and the final record entered in my Weather Book. In order to test the forecast, I adopted a plan somewhat similar to that employed by Mr. Jude (Vol. iii., p. 168), and classed the results under four heads.

When the direction of the wind was within the prophesied limits I denoted the fact by A^4; when it exceeded those limits by less than 45°, by A^3; by more than 45°, but less than 90°, by A^2; and when the direction deviated more than 90° from the prophesied limits, by A^1. These letters, together with those referred to afterwards, were entered by the side of each day's record in my book. Like Mr. Jude, I should be inclined to call A^4 correct, A^3 nearer right than wrong, A^2 nearer wrong than right, and A^1 incorrect. In the following table the results of such a system are given for the last year:—

	A^4	A^3	A^2	A^1
Jan.	20	3	1	2
Feb.	18	4	1	0
March	15	7	3	2
Carried forward	53	14	5	4
Brought forward	53	14	5	4
April	12	3	5	5
May	19	3	0	4
June	14	5	2	4
July	22	2	1	1
Aug.	19	4	2	2
Sept.	10	6	3	5
Oct.	20	3	2	1
Nov.	13	6	3	1
Dec.	17	5	0	0
Totals	199	51	23	27

In other words, out of the three hundred forecasts of the wind's direction about two hundred were correct, seventy doubtful, and thirty wrong.

II. *The Wind's Force.*—This is always very vaguely prophesied, being indicated by such adjectives as "light," "moderate," "fresh;" or by such nouns as "airs," "breezes," "gales." In order to subject these to a uniform test I drew up the following table of equivalents, and, looking at the success achieved by the prophecies in this direction, I am inclined to think it is not far from representing the meaning of the office.

"Airs"	=	force 1 ("a light air").
"Light"	=	forces 1 and 2 ("a light breeze").
"Breezes"	=	„ 2 and 3 ("a gentle breeze").
"Moderate"	=	„ 3 and 4 ("a moderate breeze").
"Fresh"	=	force 5 ("a fresh breeze").
"Strong"	=	„ 6 ("a strong breeze").
"A gale"	=	„ 7 ("a moderate gale").

Such terms as "moderate breezes," "strong gales," "calms," &c., were referred to the forces of which they are the synonyms.

When the maximum intensity of the wind (which I estimated and checked by means of the above-mentioned reports) was coincident with the equivalent of the predicted force, as obtained by the above rather arbitrary table, I wrote B^4 on the margin of the day's record. When it varied by one force, I wrote B^3; by two, B^2; and by more than two, B^1. These letters represent the same degree of accuracy as the A's. Here are the results for the year—

	B^4	B^3	B^2	B^1
Jan.	16	4	1	3
Feb.	15	3	2	4
March	10	5	3	5
April	11	6	1	1
May	12	9	1	2
June	12	7	3	2
July	19	2	1	3
Aug.	15	7	3	2
Sept.	18	1	0	6
Oct.	16	3	1	5
Nov.	12	5	1	4
Dec.	17	2	1	5
Totals	173	54	18	42

To put it roughly, out of two hundred and eighty forecasts of the force of the wind, a hundred and seventy were correct, seventy were doubtful, and forty were incorrect—a result which is, considering the arbitrary character of the test, satisfactory in the highest degree.

In testing the predictions of the general state of the weather, rainfall temperature and humidity had all to be taken into account, and as each of these will need separate treatment I will leave their consideration for another paper.

Two of the ventilators on the District Railway, and these the two nearest to the Houses of Parliament, have been removed, and the roads restored to their original condition. One of these two ventilators was opposite to the north door of Westminster Abbey, and the other between the Westminster Palace Hotel and the Royal Aquarium.

NOTES ON BOOKS.

THE PARALLEL ROADS OF GLENROY. By *Jas. Macfadzean* (J. Menzies & Co., Edinburgh). A book containing much interesting matter, and a plausible but not possible theory of the origin of the parallel roads. If Mr. Macfadzean will take the pains to calculate, or if he has not the requisite mathematical knowledge, get some one to calculate for him, how much the position of the earth's axis of rotation could possibly shift within the earth herself, with the fullest conceivable action of such causes as he suggests, he will find that such change of position can in no possible way have sufficed for such work as he wants from it.—BREEDING HORSES FOR USE. By *F. Ram* (Civil Service Printing Co., London). If we consider the wretched breed of horses now existing in the region where Virgil in his famous third Georgic described unmistakably a very fine animal we may infer that the reverse process suggested by Mr. Ram would greatly improve the various races of horses used for different purposes. He proposes practically a system by which only the best of each class should be used for breeding.—MODERN HOUSEHOLD MEDICINE. By *Chas. Rob. Fleury, M.D.* (2nd Edition) (E. Gould & Son, London). A most useful book, conveying just so much information to the non-professional student as he requires, in plain untechnical language, while saying nothing which could tend to encourage amateur practice. The professional medical man, indeed, so far from objecting to the presence of this work on the bookshelves of a house he visits, would in most cases be glad to know that a patient or those around had in such a work the means of recognising the meaning of certain symptoms and the necessity—where it arises—of calling in professional aid in good time. Apart from the medical information conveyed by this work, the very sensible advice and suggestions respecting habits of life, diet, use of stimulants, clothing, mental emotions, gymnastics, and so forth, will be found deserving of the most careful study.—*Practice of Medicine.* By M. CHARTERIS, M.D. (3rd edition (J. & A. Churchill, London). A work intended specially for students; handy and practical. We see here the real value of the technical method of describing scientific matters for scientific students,—it secures brevity and clearness. Non-medical persons interested in scientific matters, and accustomed to the scientific manner of speaking, will find this work interesting and instructive.—THE DISEASES OF CHILDREN, by *Armand Semple*, B.A., M.R.C.P., &c. A work devoted to the diseases of children, and intended chiefly for practitioners and students. But, although in parts largely technical, the work is one which intelligent parents would do well to read. It is full of useful knowledge respecting the habitudes, development, and diseases of children.—QUALITATIVE ANALYSIS, NOTES ON. By *H. J. H. Fenton*, M.A., Demonstrator of Chemistry in the University of Cambridge. (*University Press*, Cambridge). A very useful work, directing attention to the meaning of each process and the nature of each reaction. We attach great importance to the principle underlying Mr. Fenton's method. Chemical analysis is too often regarded as a series of mere mechanical operations, whose scientific meaning is overlooked.—WHEN DID LIFE BEGIN? By *G. Hilton Scribner* (Charles Scribner's Sons, New York.) An interesting little work which will remind many readers of the ideas of the French astronomer Bailly. After carefully considering the evidence Mr. Scribner concludes, and not without good arguments in support of his view, that life began within the arctic zone, "earliest in cooling down to the heat at which life could exist on the earth," and therefore presumably, "first to become fertile, first to bear life, and first to send forth her progeny over the earth."—NAVIGATION. By *John Merrifield*, LL.D., Head Master of the Navigation School, Plymouth (Longmans & Co., London). This is an excellent though small treatise on a subject not only important to the nautical world, but full of interest to students of astronomy. It gives a very clear account of the various methods for finding a ship's position, course, distance traversed, &c., and might be studied in this respect with great advantage by believers in a flat earth. The account is so clear and simple that any one tolerably familiar with elementary geometry, algebra, and trigonometry can follow it. We can imagine no pleasanter companion for such persons on a long sea journey than a handy book of this kind giving a clear and trustworthy account of the various methods used for their safety. We note as a point needing correction the absence of due notice of the effect of refraction in modifying the observations on which the process for taking departures by the dip of the land depends. Refraction is mentioned in such a way as to imply that the observations described only need correction for variations in the refraction, whereas they have not been corrected for refraction at all. The chapter on great circle sailing suggests the use of a gnonomic polar chart for taking great circle tracks; but although if such a chart includes the places at both ends of the journey the course is very simply indicated by connecting these places on the chart by a straight line, a gnomonic chart cannot be made to include more than a fourth of the surface of the earth without enormous increase of scale, nor one-half under any circumstances. A stereographic projection with the north pole as centre and extending to fifty degrees south latitude, would be much more useful. The course is indicated by a simple geometrical construction, thus:—Take the starting-port and its antipodes, the port to be reached and its antipodes; then, with a large bow compass or a string and pencil as described in KNOWLEDGE p. 37, sweep out (this can be done after a few moments' trial for centre) an arc through the two ports and either antipode (it will go through both when it passes through one): that is the great circle course. It is not even necessary to have the features of land and water charted; the longitude and latitude of ports and antipodes suffice to give the track, and therefore the longitude and latitude of every point along the great circle course. PROPHECIES AND SPECULATIONS RESPECTING THE END OF THE WORLD. By *Rev. B. W. Savile* (Houlston & Sons, London). Mr. Savile was once a supporter of what he now fairly calls "the Anglo-Israel craze;" that he is not so now "goes without saying." His pamphlet "Anglo-Israelism and the Great Pyramid" even goes to the extreme length of questioning the claims of Queen Victoria to the throne of David, on which account he has been charged with "apostasy" and a number of other things (a page or so long). In the little work before us he collects a number of the prophecies in which foolish folk have believed. For fun,—to those who are amused to see what idiotic people there are in the world,—the work may be called a splendid shillingsworth.—WEIGHTS AND MEASURES. By *John Morrison* (William Kidd, Dundee). The great trouble about weights and measures at present is that more systems are in vogue than one. Inventing a new system, however excellent in itself, is no remedy for the evil. One of the present systems *may* prevail over the rest; no new system has a chance. Otherwise Mr. Morrison's might be worth attention.—NOTES ON THE NEW PRINCIPIA. By *Newton Crosland*, author of "The New Principia": issued by Mr. Newton Crosland, author of said notes. Mr. Crosland "comes up smiling" after

punishment by the *Saturday Review.* He finds himself "tickled into a grin" every time he thinks of it. The grin is rather too obviously uncomfortable. In his notes on our own remarks Mr. Crosland expresses inability to understand why the centrifugal tendency varies in potency. We never said he *could* understand it. Many do not know why an engine-driver slackens speed in taking a curve, and when told it is to reduce the centrifugal tendency fail to see what connection there can be between velocity along the track and a tendency to leave the track.

THE EVOLUTION OF FLOWERS.

By Grant Allen.

II.—FIRST STEPS.

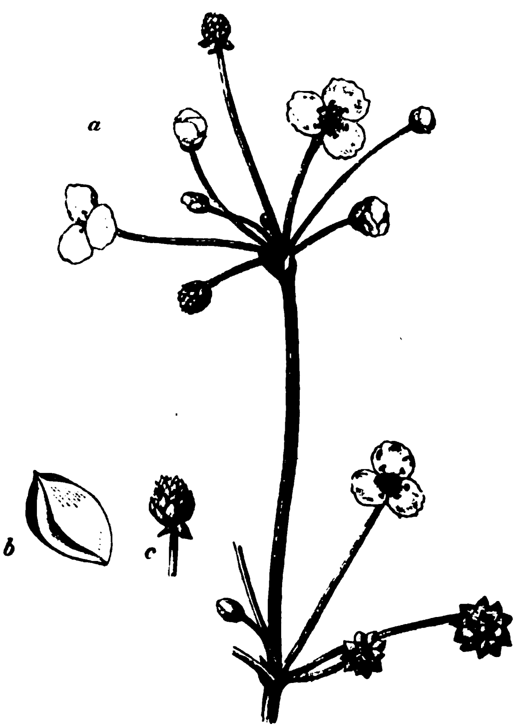
Before we go any further on our upward course in tracking the development of the lily group, I should like to point out the sort of goal towards which we must be gradually tending; and I don't think this could be better done in any way than by taking three typical stages in the evolution of the lilies on the one hand, and of the buttercup family on the other. The special interest of such a comparative view depends upon the fact that while the lilies are monocotyledons, belonging to the great class whose flowers are arranged in rows of threes, the buttercups are dicotyledons, belonging to the great class whose flowers are arranged in rows of fives. Yet in spite of this primordial distinction, we shall see that the two groups progress along exactly similar lines, and that in each we can find almost exact analogues of the grade of development reached at each stage by the other. A tabular statement set side by side will enable us to observe this close parallelism in the clearest and most interesting manner.

Buttercup Group.	Lily Group.
1. The Buttercup has: five distinct greenish sepals; five coloured petals; numerous whorls of stamens; numerous whorls of carpels, all separate, and each containing one seed.	1. The Water-Plantain has: three distinct greenish sepals; three coloured petals; two whorls of stamens; numerous whorls of carpels, all separate, and each containing one seed.
2. The Columbine has: five coloured sepals; five coloured petals; numerous stamens; five carpels, forming a single group, and each containing several seeds.	2. The Tulip has: three coloured sepals: three coloured petals; two whorls of stamens; three carpels, united into a single capsule, and each containing several seeds.
3. The Monkshood has: irregular flowers, with five very peculiar coloured sepals; two to five very peculiar coloured petals; several stamens; and only three united carpels, each containing many seeds.	3. The Orchids have: irregular flowers, with three very peculiar coloured sepals; two very peculiar coloured petals; one or two stamens; and three united carpels, each containing many seeds.

This little comparative table must serve us for the time being as a rough and general guide to the direction that evolution in flowers has generally taken. The why and wherefore of each upward step must be a matter for future explanation piecemeal.

And now let us hark back to our friends the water-plantains, and consider further what are the peculiarities which each of the best-known British kinds presents after its own fashion.

We have in England two other species of Alisma besides the one which formed the text for our first paper. One of these, the lesser Alisma (*A. ranunculoides*), a much rarer plant, is so remarkably like a buttercup that Linnæus gave it its Latin name in commemoration of the close resemblance. And, indeed, the name conveys more meaning than any mere outer likeness of parts could convey: it testifies to the great botanist's underlying appreciation of the fact that the two flowers are in reality exactly analogous to one another. The lesser Alisma holds among the lily group of three-rowed or trinary flowers just the same place that the buttercup holds among the larger group of five-rowed or quinary flowers. In both, the carpels are very numerous, and they are arranged, not in a ring round the centre of the flower (as is the case with the water-plantain), but in a big globular head, which forms a central boss inside the petals and stamens. The meaning of this curious arrangement is probably this: the whorls of carpels have become so numerous and so crowded that they have crushed one another out of shape altogether. In the water-plantain, as a rule, if you count the carpels, you will find there is a rough approximation to an arrangement by whorls of three; either there are eighteen, that is to say, six whorls (the smallest number I have ever noticed), or there are twenty-one, twenty-four, twenty-seven, or thirty,



Alisma ranunculoides.

that is to say, seven, eight, nine, or ten whorls (the largest number I have ever noticed). Often enough one or two carpels have become abortive, and so the number test fails; but, even so, it is always easy to see that the carpels fall into three rough groups, each group representing the carpels of six or seven separate whorls.

But nothing of this sort can be traced in the lesser Alisma. There the carpels are simply all jumbled together into a head, as in the buttercup, and no trace at all is left of the primitive arrangement in rows of three.

Now, this singular resemblance of the lesser Alisma to the buttercup is not a mere casual accident, but is due to the fact that both plants stand at the very bottom of the evolutionary scale in their own division of flowering plants. We cannot say how or why the primitive flowering plants split up into the two great bodies (monocotyledons and dicotyledons) respectively provided in their most regular forms with whorls of three or of five members each; and we have now no links remaining between the two bodies,

though there are a few plants in each which approximate slightly in one or two points to the peculiarities of the other. But we may be pretty sure that the Alismas and the buttercups represent a very early stage of the two bodies shortly after they had begun to diverge from one another in the direction of their own particular peculiarities. Other plants in each great body have undergone wider and ever-wider changes in adaptation to altered circumstances; but the Alismas and the buttercups have continued to preserve the same features unchanged during all the long series of intervening ages. They stand to other flowers in somewhat the same relation that the Australian black-fellows stand to the remainder of humanity, or that the duck-billed ornithorhyncus stands to the fully-developed mammals.

Alisma natans.

As I intend these notes on the lilies to be a sort of explanatory companion to the ordinary botany books, it may be well to say a few words as to some other points in the English Alismas. The lesser Alisma has larger and more solitary flowers than the water-plantain; it trusts more to size and less to number than its commoner ally. Its leaves also, when submerged, often have no blade; there is no more of them developed than the bare leaf-stalk. This is a common peculiarity of submerged plants, which almost always have very long, narrow, or much-divided waving foliage. Leaves of such a sort, I believe, are better adapted for catching and utilising the carbonic acid in the water than the large broad flat blades which succeed best in the open air and sunlight.

Our third English species, the floating Alisma (*A. natans*), found only in a few western counties, displays the peculiarities of a born water-plant in a still more striking degree. It has long lithe stems which trail at great length through the still waters of quiet ponds. It has adapted itself to an aquatic existence far more thoroughly than its two half-marshy neighbours; yet it is very closely allied to the lesser Alisma, from which it only differs in habit and in one or two minute or unimportant particulars. Like a great many other thorough-going water-haunters, the floating Alisma has two kinds of leaves. The one sort, though small, is broad and oval in shape, and lolls lightly on the surface of the water, where it can catch the full sunshine unimpeded and drink in the free carbonic acid at all its pores. The other sort grows below the water, and is reduced to a mere footstalk, sometimes slightly dilated at the end, where the blade or expanded part of the leaf ought to be. Evidently, the development of the leaves here depends entirely upon the question whether they can reach up to the free air or not; the root leaves being always submerged and narrow, while the leaves that spring from the upper joints are floating and oval in outline. Exactly the same sort of thing happens again with the water-crowfoot (*Ranunculus aquatilis*) which is a water-haunting buttercup, exactly analogous to the floating Alisma; only in that case, while the upper floating leaves are broad and five-lobed, the lower waving ones are reduced to numerous long, narrow, much subdivided segments. Observe in every case that the floating leaves are analogous to the broad round foliage of the water-lily, and the submerged ones to the hair-like waving foliage of water-milfoil, and other common freshwater weeds. Almost all aquatic plants have leaves belonging to one or other of these types, or else both to one and the other in different parts. Botanical readers will remember that the pondweeds (*Potamogeton*) offer many excellent examples of all three conditions. It is interesting to observe, however, in the case of the Alismas, that the lesser Alisma preserves for us an intermediate stage in the establishment of this peculiar aquatic habit.

The carpels of the lesser Alisma have four or five little ribs each; those of the floating Alisma have from twelve to fifteen. Can anybody say why this should be so? It is one of the numerous small points of difference one always finds even between closely-allied species, points which are generally far more difficult to account for than more conspicuous matters of habit or structure, in obvious adaptation to the circumstances of the plant.

The flowers of both these kinds are nearly identical. As in most other of our English water-plants, they are whitish in colour, a peculiarity well observed also in the water-crowfoot, which (with its variety the ivy-leaved crowfoot) is our only white British buttercup. I suppose white is a favourite colour with waterside insects; certainly it is a very conspicuous one among the lush green leaves of most aquatic plants. Both in water-plantain and water-crowfoot, the petals are yellow at the bottom, which shows that yellow was their original colour; for petals always acquire new tints from outside inward. The water-plantains secrete honey at the root of the stamens, in little round drops, which can be easily seen with a small pocket lens. All the English species are fertilised by flies, especially flies of the family of Syrphidæ, the very same insects that visit and fertilise the water-crowfoot. This is a very important fact as helping to explain the close similarity in colour between these various water-side weeds. It is also interesting to note that when floods are out, the flowers of water-crowfoot and of floating Alisma both alike remain closed under water, and fertilise themselves, the closed petals compelling the stamens to shed their pollen on the stigmas of their own blossoms.

Gossip.

Among nearly a hundred more letters about our proposed change of size and price, only two have been unfavourable, —and these kindly. We note in answer to several correspondents that the size of the page will remain unchanged. At present, we only remark of the change, which will begin with the first number of March, that apart from added matter, it will enable us to keep promises which we made in the hope that they could be fulfilled without change. It is all very well to promise serial papers on such and such subjects which would just fill our present space; but when new subjects are constantly arising which though not needing serial treatment yet themselves form a continuous series, the promised articles are crowded out. A part, therefore, of our increased space will be occupied by those papers on "How to get Strong," "Light Sifting," and so forth, which we have been long anxious to resume or begin, but for which we have had insufficient space. Another portion of the increased space will be taken up in lengthening our weekly supply of matter relating to special subjects. For the rest we are promised papers on Geological, Optical, Mechanical, and other matters of general interest, for which we have long been anxious to find room. From the tone of nearly all the letters which have reached us, we deem it hardly necessary to say that from the beginning we have sailed as near to the wind as we dared (rather nearer at times than we deemed altogether safe), in the way of providing for our readers as much of what a correspondent calls "readable matter" as possible. We shall continue on this course as long as we may.

Dogberry never said anything finer than a man about whom recently we have seen a good deal (one can hardly tell why) in the papers. He was rebuking, deservedly as it seems, a certain rude man, and had expressed a very proper feeling about the fellow's bad manners. "Will you repeat that before those gentlemen there?" said the man. "Yes, I will," was the odd reply, "before *any one who does not know how to behave.*" A strangely chosen audience! He must have afforded a fund of amusement to those about him. So probably did the older Dogberry, though once he seemed tedious.

I have to cry "Peccavi." A correspondent, "A. McD.," some time since rather roundly made fun of a statement made in all good faith by Mr. J. W. Robertson. The latter, in letter 1,101, defended himself pleasantly, and received my apology for a letter which, however, was not intended, I felt sure, offensively. Mr. "A. McD." then wrote a courteous and pleasant letter apologetic both to Mr. Robertson and myself. This I *thought* I had forwarded to the printers with a little note of my own explaining that no apology to me was necessary, and mentioning how my own experience enabled me to understand that while no offence was intended, the sense of the humorous aspect of the matter had been too strong for him. Unfortunately, I had *not* it appears sent the letter. At least they knew nothing of it at the office. As it is far more likely the mistake is mine than theirs, I hereby express both to Mr. Robertson and to Mr. A. McD. my regret that my carelessness should so long have delayed the pleasant ending of this little matter.

Among the inaccuracies which Professor Ruskin attributes to scientific people is one which singularly illustrates the contempt with which he has allowed scientific teaching to pass unnoticed by him, and the scorn which he has then felt for what he has not cared to examine. The ignorant scientific person, in this particular case Prof. Tyndall, uses the words "vibration" and "undulation" as synonymous. Then Professor Ruskin defines vibrations and undulations, about as inexactly and even incorrectly as is possible. But the point to be noticed is not the incorrectness of an art-critic person, (to follow his method) but the circumstance that neither Professor Tyndall nor any one else, except Professor Ruskin, has ever even thought of the possibility of confounding "vibrations" the (proximate) cause with "undulations" the effect.

But Professor Ruskin has not contented himselt with criticism of the descriptions and explanations given by scientific men; he has attempted scientific description and explanation himself. We find that he suspects the prismatic clouds to consist of comminuted water or of ice while the non-prismatic clouds consist of cloud-vapour. What is cloud-vapour? It cannot be aqueous vapour, for we know that to be as transparent as dry air. What then is it?

But he makes up for all such mistakes, by his sound precepts about troubled passions and lip-insolence Even though the precept be not illustrated in his case by practice it is well worth attention.

I find I was mistaken about my election to the St. George's Chess Club. It appears that a remark of mine ("I wish I could be a member," my words probably were), really expressing my regret that multitudinous engagements prevented me from seeking election into our leading chess club, was misunderstood to signify a wish that I might be elected. So my election was in reality an act of kindness and courtesy on the part of those who arranged it. It will be imagined how regretfully I find that I had misapprehended the matter. Yet my mistake was a very natural one. Not a word or a line had ever been addressed to me by any member of the club on the subject of election (except some three years ago by Herr Gümpel to whom I mentioned that though it would be most pleasant to me, I could not join, because I scarcely ever had an hour free for chess play) until the time when I received from the secretary the news of my election,—my name having been proposed by a gentlemen with whom I have not exchanged a dozen sentences in my life. I had an idea how the matter might have arisen; but letters received from that gentleman and from the secretary seemed to negative that idea,—really (as it now appears) correct. I can only express my warm thanks to those who procured for me the honour of election to so deservedly distinguished a body, and my regret that for want of a few words of explanation I was led—in the long run—to entirely misunderstand the matter.

My remark about the place where the club's rooms are has been misapprehended. It by no means signified that the club is little known,—the idea is indeed absurd—the club is well known on the Continent and in America as well as through the length and breadth of England. Not to know exactly where its rooms are argued only myself unknown in its purlieus, which is exactly what I meant to express.

A Spanish correspondent addresses Mr. Sala (see "Echoes of the Week" in the *Illustrated London News*) as to the meaning of the word "jerry mander" used in "el ultimo discurso de Don Josef Chamberlain a Birmingham." The Castilian has looked in all manner of English dic-

tionaries for the word without success; and Mr. Sala has at his instigation looked into others, with no better results. "Nor is it in Professor Scheele de Vere's 'Americanisms,'" he writes. I have not the latter work, and I am away from my books of reference. But I think I can explain why neither Mr. Sala nor the inquiring Spaniard have found the word in question. There is in fact no such word under the "J's" in any correct dictionary. The word is Gerrymandering, and the origin is in this wise. When political disputes were in fierce progress throughout the United States (come, I think no one will dispute that time reference) a plan was devised for so dividing up Massachusetts as to ensure a large majority of elect favourable to the side which was at the moment in power—the Democratic side. This was managed by so arranging that two or three unfavourable representatives should be elected by overwhelming majorities, so that a great number of the voters on that side were used up in securing those few elections; while on the other side representatives were to be elected by bare (but just safe majorities). Thus supposing 100,000 Democratic electors and 120,000 Republican, and the regions where these last were mostly gathered to be well known; then by suitably arranging the district, it would be possible to distribute voters for ten representatives thus:—(1), 18,000 R., 4,000 D.; (2), 19,000 R., 3,000 D.; (3), 17,500 R., 4,500 D.; (4), 9,700 R., 13,500 D.; (5), 9,300 R., 12,500 D.; (6), 9,500 R., 12,700 D.; (7), 9,100 R., 12,300 D.; (8), 9,300 R., 12,500 D.; (9), 9,400 R., 12,600 D.; (10), 9,200 R., 12,400 D. With such arrangement, or something akin to it, three Republicans only would be elected, as against seven Democrats, though the district really contained a large majority of Republican voters. With somewhat more daring in the application of the method two Republicans or even only one might be returned from the district, as against eight or nine Democrats. But naturally when a district had been specially divided up for this purpose a map indicating the boundaries of the various voting districts had a strange appearance. A map of the sort prepared on a plan which was supposed (wrongly, it is said) to have been favoured by Governor Gerry, had so strange an appearance that an artist by a few touches made it resemble a picture of a salamander. "A Gerrymander, we may call it," said the editor of a Republican paper in whose office the map hung. The name has remained in America to this day. Now that Mr. Chamberlain has used it in the old country we may expect the word to be often heard, especially as the thing exists here though not perhaps in quite so extravagant a plan as was illustrated by the original Gerrymander map.

In a treatise on the Bath Waters, by Joseph Hume Spry, Surgeon, we come across a passage from an old work called "Leland's Itinerary," (vol. 2). "Or ever I cam to the Bridge of Bath that is over Avon, I cam down by a rokky hille, fulle of fair springes of water; and on this rokky hille is sette a long streate, as a suburbe to the Cyte of Bath; and in this streate is a chapelle of St. Mary Magdalen. Ther is a great gate with a stone arche at the entre of the bridge. The bridge hath V fair stone arches. Betwixt the bridge and the south gate of Bath I markid fair medowes on eche hand, but especially on the lift hand, and they ly by south-west on the town. The Cyte of Bath is seete booth yn a fruteful and pleasant botom, the which is environed on every side with greate hilles, out of the which cum many springes of pure water, that be conveyed by dyverse way to serve the Cyte; inasmuch, that leade beyng made ther at hand, many houses yn the towne have pipes of leade to convey water from place to place.

"Let Knowledge grow from more to more."—Alfred Tennyson.

Only a small proportion of Letters received can possibly be inserted. Correspondents must not be offended, therefore, should their letters not appear.

All Editorial communications should be addressed to the Editor of Knowledge; *all Business communications to the* Publishers, *at the Office, 74, Great Queen-street, W.C.* If this is not attended to delays arise for which the Editor is not responsible.

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THE AFTER-GLOW.

[1119]—I am very much indebted to Mr. Howarth, for his communication (letter 1,105, p. 76) in reply to what I have previously said in these columns concerning the recent wonderful after-glows; and I may avail myself also of this opportunity of thanking Mr. Clapham and Major Hawkins-Fisher for very interesting private letters which they have addressed to me on the same subject. From Major Fisher's observation of the Christmas sunset, it must have been one of almost unparalleled splendour, and I can quite conceive, from his description, that the fiery glare of the after-glow must have penetrated the thick fog which covered our Sussex sky, and so imparted that ruddy colour to it which appeared so strange and unnatural. Mr. Clapham was observing with Mr. Kershaw (letter 1095, p. 4) at the time when this phenomena was apparent in full daylight. In his case it would seem to have assumed rather a different form to that which it presented to Mr. Howarth; inasmuch as he describes it as a kind of rosy glow or halo, with its inner edge about 5° from the sun, and its outer boundary some 20° to 25° outwards. Ice spicules would account for its appearance between 20° and 30° from the sun, but its approximation within 5° puzzles me. It is a matter of history that the sky was ablaze when Titian died. Have any of the readers of Knowledge access to records of voyages at that date? because early travellers always noted such phenomena as volcanic eruptions carefully.

William Noble.

Forest Lodge, Maresfield, Uckfield, *Feb.* 13, 1884.

SKY-GLOW (EXTRACT FROM LETTER).

[1120]—"The light in the sky is not only at sunset but at sunrise; in fact, is more brilliant in the morning. About an hour before sunrise it appears in the east. A few months ago, when staying at Patrysfontein, after it was almost dark, this light would appear, and so bright I could take my work or book and have nearly an hour's light."

Graaff-Reinet, Cape Colony, *Jan.* 1.

GRACE'S SPECTROSCOPE.

[1121]—Since my reference to Grace's spectroscope in letter No. 1052, Vol. IV., Mr. Browning has kindly given me an opportunity of trying the new form of this instrument, and I have no hesitation in saying that it is eminently adapted for the purposes of a hygro-spectroscope. The dispersion being the same as in "The Rainband Spectroscope," observers will find no impediment to the comparison of results in the two different forms.

F. W. Cory, F.R. Nut. Soc.

FEAR OF THE UNFAMILIAR.

[1122]—After I had recently closed and sent a letter containing some observations relative to Mr. Grant Allen's view of instinctive fear in certain animals with regard to others, I remembered a curious circumstance which happened to me in Delhi, in November, 1881. I had gone to the Chandni Chowk (or large bazar there) to make some purchases, accompanied by my dhurzie (or native tailor). All my commissions were executed; just as this native (a Mahomedan from the Punjaub) was assisting me into the carriage, a

native boy, about eight years of age, passed us; he was a perfect albino. I shall never forget the expression of terror depicted on my servant's face; he stood as if petrified for the moment. This was to him evidently a new sensation. He turned a ghastly colour, till I reassured him by explaining what this child was, and that there was nothing to fear. May not the effect produced on certain animals by the sight of others which are new or strange to them, as in the case of monkeys in the Zoo who have never seen a snake, be somewhat similar in its nature to this? My Mahomedan tailor was decidedly intelligent, clever above the average in his vocation, and a most faithful servant. May his shadow never be less! COSMOPOLITAN.

STRANGE RESUSCITATION.

[1123]—A trawling vessel, the *Tempus Fugit*, of Lowestoft, arrived at that port on Wednesday, Jan. 9, about 7 a.m., having made the last catch of fish about 8 a.m. the previous morning. At 8 a.m. on the 9th ult. these fish were landed in Lowestoft Market, twenty-four hours having elapsed since they were caught. I picked up a fine specimen of plaice ("platessa") which was frozen quite rigid. It accidentally dropped from my hand and fell with a smart blow on the vessel's deck, when it immediately commenced jumping about quite as briskly as a fish just out of the water, and continued doing so for upwards of an hour.

This phenomenon is well known by most trawl fishermen, and is by no means an isolated case.

As I cannot understand how a fish that has been frozen for twenty-four hours, and then, by receiving a sharp blow, will exhibit—at least as far as its movements are concerned—all the appearance of a freshly-caught fish, I shall be pleased if any of the readers of KNOWLEDGE can throw any light on this subject.

JOHN HAME.

TRICYCLING UP HILL.

[1124]—Mr. Browning seems to meet with the most extraordinary accidents while riding tricycles. I have ridden one for eighteen months (a "Salvo"), and have never met with the slightest mishap. What can Mr. Browning mean when he says, "The machine should not have been turned or allowed to turn when riding up a stiff hill"? Why, any tricycle-rider will know that the easiest way to ride up a steep hill is to keep zigzagging or turning about from one side of the road to the other, in the same manner that one will see the driver of a horse that has a heavy load to pull up a steep hill—in fact, the horse will adopt the plan of his own accord! What sort of a machine must it be that will not work in this way going up hill? SUBSCRIBER.

EARLY EMOTIONS.

[1125]—I have observed a blind kitten, not three days old, spit, after the manner of its kind, at a dog to whom it was shown. Such an act seems to involve sensations and emotions of considerable complexity, and I must leave the analysis of them to others; but I may mention that there were no signs of anger or excitement on the part of the dog, who was rather frightened than otherwise. The object of the introduction was to explain to him that the kitten was a recognised member of the household, and was not to be molested.

T. S. L.

P.S.—On no other occasion have I ever seen so young a kitten act thus.

LETTERS RECEIVED.

H. B. LINDSAY. The closing poem to my lecture on the "Life of Worlds" is Richter's dream, as translated by De Quincey. It closes my "Universe of Stars." Goethe's lines, by which I usually close my lecture on the Moon, beginning "See all things with each other blending," will be found in Anster's translation, publisher's name I have forgotten. I do not know the lines beginning "Roll on thou sun, for ever roll!" and should be much obliged if you could send them: albeit the first line reminds me rather grotesquely of Gilbert's address to the earth, beginning "Roll on thou inky ball, roll on! what though," &c.; and ending, "Never *you* mind, roll on! (*It rolls on.*)"—SENEX. Thanks: I agree with you muchly. In passing, let me express my sympathy with your remark (proverbial quite), "a thousand mosquitoes cannot kill a man; but, my word, they can worry him, *some*." You would still better know how applicable it has been if you could see arrayed the multitudinous suggestions made me in the past, and even lately. Why so much about the stars, cooking, flowers, insects, the microscope, the telescope, happiness, Chess, Whist, and now chiefly the tricycle. Then if one of these or other subjects gets less than usual, why are you dropping out flowers, insects, the telescope, happiness, &c. The writers themselves on these various subjects have been quite good and patient with me, when I have had to limit their space,—with one recent and rather "loud" exception. But many are ready to raise an uproar for them.—H. DALZIEL. Thanks. That is the way to view matters. On the last point; I also think a man is about the best judge of what he can do; also of what he cannot. Yet he may have to learn from others what his work is worth. It is not by the words but by the actions of others that a man is taught what his capacities and shortcomings may be. I tried a certain line of writing eighteen years ago, which I thought I could manage fairly well: no one *told* me I could not, neither did I ask any one to tell me; but I was shown in a very practical way that what I was trying was not in my line.—T. CANAWAY. Yet another "Yes." Thanks. We certainly do not mean to alter the page.—HARRY WILSON. And another. The evidence and reasoning about matters rather voluminous. Chiefly thus. In a given part of the sky, carefully watched, a certain number seen. For decrease of size a certain increase in number. But some telescopic. Hence inference as to actual number for whole sky, and thence for whole earth.—C. J. CASWELL. Another in favour of the change. I do not know how my old friend F.R.A.S. came to overlook the occultation of Venus.—X. Another. As to Index, quite agree with you. It will come out in future with the second or third number following completion of volume.—H. POOLE. Thanks. Your letter marked for insertion.—R. B. COOK. Simply crowded out. With the new arrangement there will be no such trouble. The absence of initials arose from the intention to insert.—G. A. BROWN. Thanks. Every correspondent thus far answered here has approved the change of size,—except one who sent a letter for insertion, and had no occasion to refer to the matter.—H. ROMEIKE. Many thanks. That pleasant and kindly notice had not reached me. If it had been unpleasant I think it would. But I am inclined to modify the opinion I had formed. I believe many would be benefited by an arrangement bringing such notices to their attention.—W. J. TROUTON. The results are almost identical if almost be taken with a tolerably wide meaning. "We can't quite almost generally tell," as an American hymn remarks. But your relations will not do. My old acquaintances (the real distances) are far better. It is worthy of notice, however, how singularly the planetary relations you deal with and others can be fitted together with a little ingenuity. See Prof. P. Smyth on the Pyramid for other instances.

Our Paradox Column.

LIGHTNING.

A MOST remarkable attack upon the universally received theory of lightning discharges has been made by Colonel the Hon. Arthur Parnell, late R.E., who on the 21st ult. read a paper on "The Action of Lightning Strokes in Regard to the Metals and Chimneys of Buildings," before the members of the Royal Institute of British Architects.

The paper was the result of four years' research into the nature and action of what he termed "thunderbolts," instead of lightning. He had 506 cases selected out of 1,145 records of lightning strokes, ranging from Jan. 24, 1665, to Nov. 23, 1883. In all these cases metals were present in the buildings struck, or chimneys were in the immediate vicinity. The cases quoted were, however, wanting in point, as even when a lightning conductor or protector was present, he failed to give any particulars as to its condition. In deducing his practical conclusions, he stated that he desired especially to guard against advising the members of the Institute taking *any measures at all* to safeguard buildings from thunderbolts.

He laid it down as a general principle that *the use of metal in any form* inside and outside buildings should be minimised. He condemned as wholly useless all metallic external appliances such as—

I. Lightning conductors.

II. Vanes, weathercocks, finials, crosses, balls, and spindles.

III. Bells and clocks in towers, and in elevated parts of buildings.

IV. Iron ridge castings.

V. Metallic balconies (as also metal chimney-pots, flashings, hips, and other lead work on roofs; eaves-gutters, rain-water pipes, wire-guards to large windows, iron window-bars, &c.).

Inside of buildings he condemned as dangerous the employment of metal, and

VI. Large pier-glasses.

VII. Gildings (together with organs, pianos, safes, iron bedsteads, gas-pipes, and water-pipes).

For substitutes he would use earthenware, terra-cotta, Portland cement, glass, asphalte, &c.

He considered gas-holders, oil-tanks, railway stations, and temporary erections of corrugated iron, as special cases, which he would meet by connecting the iron framework to the ground at one or more places, and provide the most elevated points of such metalwork with short spikes, very sharply pointed, so as to convert the whole mass into what he calls "an *electric tap*, ready to eject the whole charge that may accumulate in the ground near its base, in virtue of the physical property of metal points to throw off and scatter electricity."

Chimneys should be built without metal, but since they were a necessity he suggested:—1. That the metal work on the *lowest floor* should be connected to the soil below by means of two iron bands, one at each side, securely fastened to the foot of the grate's front, and passing down through the hearthstone below into about a foot of soil. 2. To fix securely and closely to the top bar of the grate two iron spikes, three inches long, at an angle of 45°. The points to be steeled and always kept sharp.

In concluding his remarks the gallant Colonel contradicted his previous advice by urging architects to make a study of the physics of lightning strokes as a part of their professional business.

Enough knowledge of lightning strokes to enable an architect to safeguard a building could be obtained from nature alone:—"*It did not belong to the electrical engineer, whose province was artificial electricity.*" The defence of life and property from lightning seemed to rest on the labours of three classes of scientific men:—1. The meteorologists, who observed the phenomena of thunderstorms. 2. The physicists, who reasoned on the facts, and eventually deduced therefrom theories and laws. 3. Architects practically to apply the reasonings of the physicists.

As may be imagined, the reading of such a paper called forth frequent roars of laughter, which was slightly reflected in the comments of the various speakers who attempted to criticise the idea set forth. Thus Prof. Symons, secretary of the recent Lightning-rod Conference, said his breath had been fairly taken away by the statements he had listened to. He could fancy he had been carried backward just a century. Prof. Hughes confessed he had learnt a great deal that night—to wit, that lightning and electricity were different in their action; whilst another interesting point was that there were no lightning strokes now, but there were a number of "thunderbolts." He desired very much to see one and get hold of it. To talk of buildings being destroyed by lightning because they were provided with lightning-conductors was like talking of ships being wrecked by their rudders, or of steam boilers being burst by their safety-valves.

To us, the chief surprise is that a society like that of the British Architects could have afforded to devote an evening to the ventilation of such nonsense as is contained in the paper. We presume the great majority of our readers are better acquainted with the subject, and can enter into the feeling of amusement infused amongst those present who could appreciate the fun—otherwise we should have long since devoted an article to the subject.

We hear that Messrs. Wyman & Sons, of Great Queen-street, London, have a little work in the press, entitled "John Bull's Neighbour in her True Light: being an Answer to some recent French Criticisms." By "A Brutal Saxon." Rumour says that the book is likely to be as eagerly read as "John Bull and his Island," as the author, who has lived for many years in France, draws some startling pictures of French life; and his authoritative descriptions of "Horrible Paris" can hardly fail to cause a sensation on this side of the Channel.

Long-distance Telephony.—Some very successful experiments in communicating over long distances by telephone have been made by the officials of the electric telegraph department at Port Elizabeth. The first experiment, says *The Colonies and India*, was made between Port Elizabeth and Graham's Town, when the sound was freely transmitted along the line—a lively conversation being carried on. Songs sung were also distinctly heard at the other end. The Fort Beaufort line was then connected, and an extension made *viâ* Humansdorp, giving a total length of communication of over 400 miles, through which the sound travelled with astonishing clearness. It is probable that an attempt will shortly be made to communicate with Cape Town by the telephone.

Our Whist Column.

By "Five of Clubs."

The following game was played recently at the Editor's table: it shows how a lost game may be sometimes saved if the holders of winning hands are thrown off their guard by the seeming certainty of their game.

The Hands.

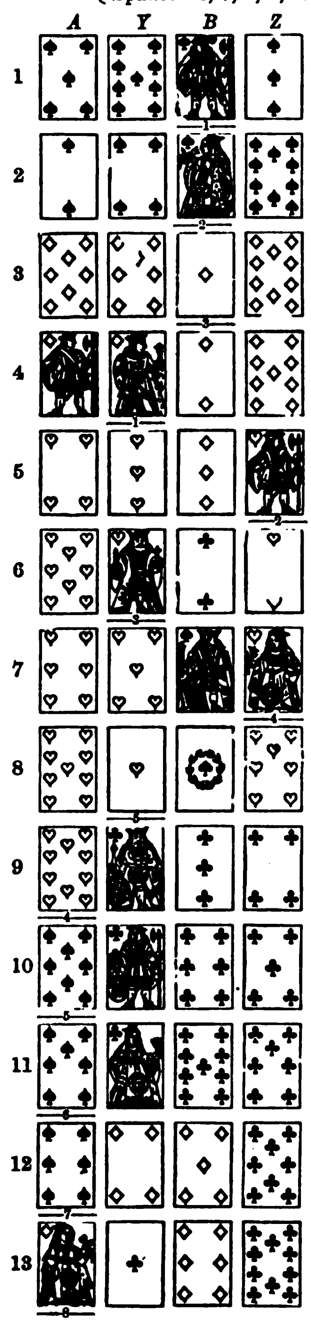
B { *Hearts*—None. Spades—A, K, Q, Kn. Clubs—9, 6, 3, 2. Diamonds—A, 6, 5, 3, 2. }

Y { *Hearts*—A, K, 5, 3. Clubs—A, K, Q, Kn. Diamonds—K, 7, 4. Spades—9, 4. }

A B, 3. B — Dealer. Z — Trump. H. 7. — Y — Y Z, 1. — Leader. A

Z { 2, 7, Kn, Q—*Hearts*. 4, 5, 7, 8, 10—Clubs. 3, 10—Spades. 9, 10—Diamonds. }

A { *Hearts*—10, 9, 8, 6, 4. Spades—8, 7, 6, 5, 2. Clubs—None. Diamonds—Q, Kn, 8. }



NOTES AND INFERENCES.

1. *A* leads the penultimate of his long weak suit rather than trumps. The lead is unsound. *Y* begins a signal.

2. *B* leads the lowest of the head sequence to show *A* he holds the others. *Y* completes the signal. Neither *Y* nor *Z* has any more Spades.

3. *B* cannot go on with Spades since that would be leaving *Z* to ruff and *Y* to discard. He therefore opens his long suit. *Z* begins "the echo of the signal."

4. *Z* completes the echo. The Queen lies either with *A* or *Z*.

5. *B* discards from his long suit, strength in trumps being with the enemy.

6. All the honours are with *Y-Z*, who therefore win unless *A-B* can make two by cards. As *Y-Z* are sure of four tricks in trumps and have already won a trick in a plain suit, the case looks almost hopeless, since one more trick will win the game for *Y-Z*. *B* sees that the sole chance is that *A* may hold the long trump and be able with it to bring in his long spades. He does not begin *at once* however to get out of the way. If he had allowed the seventh trick to pass before beginning to discard his big Spades the game would have been past saving. But he had a reason for waiting to the seventh trick.

7. *Y* should at once have led Clubs. There is only one chance for *A-B*, viz., that *A* holding the winning Diamond should remain with the long trump. If he does, then as he has the three long Spades, he can have no Clubs, and will bring in his long suit by ruffing Clubs after the other trumps are out. But if he is forced at trick 7 (and he cannot refuse the force) he is powerless to save the game.

8. *Y* having blundered, *Z* follows suit. They thought the game so sure that no care was wanted. "A mere 'walk over,'" said *Z*, as he led; and so it was, only the walking was done by the other side. The discard of the Spade King at trick 7 should have shown *Y-Z* their danger. That was why *B* delayed the significant discards.

9, 10, 11, 12, 13. *A B* walk over.

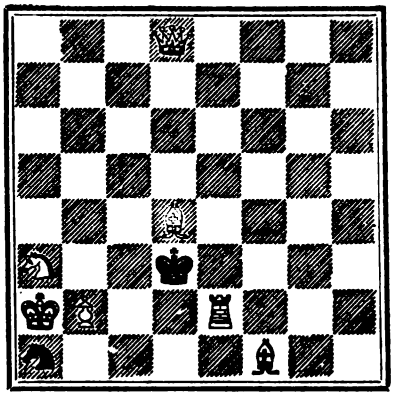
Our Chess Column.

By Mephisto.

SELECTED PROBLEM.

By G. J. Slater.

Black.



White.

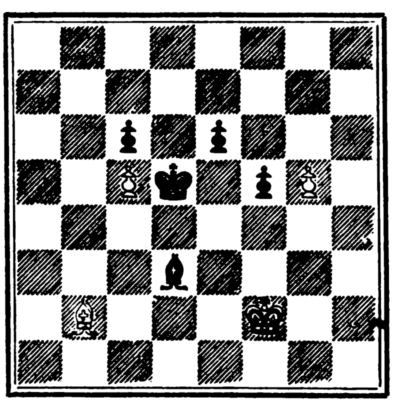
White to play and self-mate in seven moves.

1. P to Kt4	1. Kt moves
2. B to R sq (ch)	2. Kt to Q5
3. Kt to Kt sq	3. K to B5
4. R to QKt2 (ch)	4. Kt to K7
5. Q to Q6	5. K to Kt4
6. Q to K6	6. K to R5
7. Kt checks	7. Kt takes Kt, mate

The following position and ending, occurring in a match game played on Saturday evening, the 9th inst., between Mr. Hoon (playing for the North London Club), and Mr. H. Selfe Leonard (playing for Greenwich), are illustrative alike of the mischief of "trying to win a drawn game," of the importance at times of reckoning with minutiæ of arrangement, seemingly insignificant, and, still more, of the occasional rashness of pawn capture :—

Mr. Hoon.

Black.



White.

Mr. Leonard.

K to K3	B to QKt8 (*a*)
K to KB4	K takes P (*b*)
B to K5, and wins (*c*)	

NOTES.

(*a*) Not very obvious, but this choice of sq for the B proves curiously unhappy.

(*b*) The sacrifice here by Black of his King's Pawn, instead of his greedy capture of the White Pawn would still have secured the draw, but he missed his danger.

(*c*) For, curiously enough, the sole remaining White Pawn is too much for the three remaining Black ones, as was the last of the Horatii in Roman story more than the equal of the three maimed Curatii. Though in the middle of the board, he can be caught by nothing before Queening, so effectively is Black's Bishop *hors de combat*, thanks to his own Pawns. It would have been otherwise had he gone, for instance, to B2, whence in three moves he could have commanded KKt sq.

SOLUTION.

Problem No. 114, by F. J. D., p. 92.

1. B to Kt8	K to K3	Or,	K to B4
2. K to K3	K to B4	Kt takes P	Any
3. Kt to Q4, mate		Kt mates	

ANSWERS TO CORRESPONDENTS.

*** *Please address Chess Editor.*

Parson.—1. *Chess Monthly* and *British Chess Magazine*. 2. They are mostly open to everybody. Write to the Hon. Sec. of the particular club you wish to join. The St. George's is the foremost.

Ch. E. Bell.—If 1. Q takes Kt, B to QB3. 2. Q to K6, B takes B. 3. Q to QB4 (ch), K to K4, and there is no mate.

Henry Bristow.—Solution correct.

Contents of No. 120.

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CONTENTS OF No. 122.

GHOSTS AND GOBLINS.

By RICHARD A. PROCTOR.

(*Continued from page* 95.)

AMONG the most perplexing circumstances in the common belief about ghosts are the accepted ideas about ghostly habilaments. For instance, why should so many ghosts be clothed in white? If the answer is that grave-clothes are white, we may inquire what a ghost wants with grave-clothes? It might as well refuse to appear without a coffin. And then, many ghosts have appeared in their habit as they lived. If we inquire what is the real conception in the ghost-seer's mind as to the nature of the vision, we find a difficulty in understanding what idea is formed by the real believer in ghosts respecting the vestments in which spirits make their appearance. This is an old difficulty. In fact it has probably occurred to every one who has thought over a ghost story. So soon as we come to the description of the ghost's vestments there is always a hitch in the story. For my own part, I must have been a very small child indeed, when I first pondered over the question—Who made the ghost's clothes?

Of course there is no difficulty in the case of those who believe only in ghostly apparitions as phantoms of the brain. Here a distinction must be drawn. I am not speaking of those who regard such apparitions as either due to a diseased action of the brain or to the power of fancy in forming from real objects, indistinctly seen, the picture of a departed friend; but of those who look on visions of the dead as produced by supernatural impressions on the brain. Those who think that at the will of the dead a vision may be caused to appear, can of course understand that this vision would either be clothed in the garb which had been worn during life, or in grave-clothes, or in such other dress as suited the circumstances under which the vision appeared. But this view is not ordinarily adopted by those who regard apparitions as supernatural phenomena. They commonly regard the phantom as something really existent in the place where it is apparently seen. The dead person is *there* in some form; some essential entity representing him has the power to transport itself from the place of the departed into the presence of the living. This ordinary idea of ghostly visions is aptly rendered in Hamlet's address to the ghost. He does not speak *of* it as a vision, but *to* it as something real, although not understood:—

> Be thou a spirit of health or goblin damn'd,
> Bring with thee airs from heaven or blasts from hell,
> Be thy intents wicked or charitable,
> Thou comest in such questionable* shape,
> That I will speak to thee: I'll call thee Hamlet:
> King, father, royal Dane: O, answer me!
> Let me not burst in ignorance; but tell
> Why thy canonized bones, hearsed in death,
> Have burst their cerements; why the sepulchre,
> Wherein we saw thee quietly inurn'd
> Hath oped his ponderous and marble jaws,
> To cast thee up again.

Nor does the poet shrink from investing the ghost with the garb of life. This had been already shown in the first scene. "Such," says Horatio, "was the very armour he had on, when he the ambitious Norway combatted." And now Hamlet asks—

> What may this mean,
> That thou, dead corse, again in complete steel,
> Revisit'st thus the glimpses of the moon,
> Making night hideous; and we fools of nature
> So horridly to shake our disposition
> With thoughts beyond the reaches of our souls?
> Say, why is this? Wherefore? What should we do?

Again, it is curious how thoroughly the conventional idea of a ghost or goblin is associated with the thought of a shrouded face. It may be that this is partly due to the circumstance that while the imagination may quite commonly present to us the idea of a vision in all points complete except in the face, it can be but rarely that real objects are mistaken for the actual features of a deceased friend. Be this as it may, the ghost has been pictured with concealed face from time immemorial. So Flaxman draws the ghost encountered by Ulysses in Hades, and no really fearful ghost has shown its face since the days when fear came upon Eliphaz the Temanite, "and trembling which made all his bones to shake; when a spirit passed before his face and the hair of his flesh stood up; and the spirit stood still; but he *could not discern the form thereof.*"

It is curious that children, when they try to frighten each other by "making ghosts," cover their heads. There is another singular trick they have—they make horns to their heads with their forefingers. Why should horns be regarded as peculiarly horrible? The idea can scarcely be referred to the times of our savage ancestors, for the creatures they had chiefly to fear were certainly not the horned animals. Yet the conventional devil is horned, and moreover, "divideth the hoof," and is therefore a ruminating animal.† Did our savage ancestors keep their children in order by frightening them with stories about their horned cattle? It is likely, at least, that among the most portentous forms known to those children must have been the oxen and goats which formed a principal feature of their surroundings.

It must be admitted that there is something particularly hideous in a long horned face. I remember an instance where the sudden appearance of such a face, or what I took to be such, caused me a degree of discomfort certainly not justified by the occasion. Singularly enough,

* Mistakenly understood generally to signify "doubtful." What is meant is obviously "a shape as of one to whom questions can be addressed."

† The conventional dragon is a Pterodactylian reptile. Ruskin will have it that Turner's picture of the Dragon guarding the Hesperidan apples was a mental evolution of a Saurian reptile; but Turner himself said he got the idea of his dragon at a pantomime at Drury Lane. *Utrum horum mavis accipe.* It is a wide range from the greensand to the greenroom.

the event belonged to the period of my life to which I have already referred; and I may as well note that at no time, either before or since, have I even for a moment (and against the will of the mind) mistaken commonplace objects for either "spirit of health" or "goblin damn'd."

During the last weeks of the long vacation already mentioned I went alone to Blackpool, in Lancashire. There I took lodgings in a house facing the sea. My sitting-room was on the ground-floor. On a warm autumn night I was reading with the window open; but the blind was down, and was waving gently to and fro in the wind. It happened that I was reading a book on demonology; moreover, I had been startled earlier in the evening by prolonged shrieks from an upper room in the house where my landlady's sister, who was very ill, had had an hysterical fit. I had just read to the end of a long and particularly horrible narrative, when I was disturbed by the beating of the curtain—the wind having risen somewhat—and I got up to close the window. As I turned round for the purpose, the curtain rose gently and disclosed a startling object. A fearful face was there, black, long, and hideous, and surmounted by two monstrous horns. Its eyes, large and bright, gleamed horribly, and a mouth garnished with immense teeth grinned at me. Then the curtain slowly descended. But I knew the horrible thing was there. I waited, by no means comfortably, while the curtain fluttered about, showing parts of the black monster. At last it rose again so as to disclose the whole face. But the face had lost its horror for me. For *the horns were gone.* Instead of the two nearly upright horns which before had shown black and frightful against the light background of sea and sky, there were two sloped ears as unmistakably asinine as I felt myself at the moment. When I went to the window (which before I felt unable to approach) I saw that several stray donkeys were wandering through the front gardens of the row of houses to which my lodgings belonged. It is possible that the inquisitive gentleman who had looked in at my window was attracted by the flapping curtain, which he may have taken for something edible. "If so," I remarked to myself, "two of your kind have been deceived to-night."

(To be continued.)

THE MORALITY OF HAPPINESS.

By Thomas Foster.

CARE FOR SELF AS A DUTY.

(Continued from page 96.)

BUT there is another aspect of this part of our subject which requires careful attention. We have already touched on the effects which would follow if all the members of society in their zeal for the interests of others disregarded the requirements of their own health and well-being, and overlooked the effects of unwise neglect of self on the interests of their descendants and therefore of the society of which their descendants would form part. Nor in considering this aspect of the subject, have we been dealing with imaginary evils, seeing that many of the defects of the body social at the present day can be clearly traced to such misdirected, though well-meaning, efforts on the part of the better sort in past ages.

But when we consider the mixed nature of all communities, the mischief of ill-regulated disinterestedness as compared with far-seeing consideration of the interests of family, race, and nation, becomes more obviously a matter of practical moment.

If *all* men sought the good of others before their own it is obvious that a confusion of interests would arise,—other but not less unsatisfactory, perhaps, than that which exists in a society where, let their doctrines be what they may, the greater number seek their own welfare first. If, on the other hand, *all* men were moved by far-seeing considerations and a well-regulated care for the interests of others, no special care would be needed, and few rules would have to be laid down, to ensure the progress and happiness of the community. But, as a matter of fact, neither one nor the other state of things exists. The body social as at present existing may be classified, as regards care for others and self-seeking, into the following principal divisions :—

A. First there are those who in precept, and as far as they can in practice also, think of others before themselves, who repay injuries by benefits, answer reviling by blessing, and adopt as their rule the principle that those who injure and hate them are those whom they should chiefly love and towards whose well-being their efforts should be chiefly directed. This class is very small; it is always losing members, but is probably increased by fresh accessions about as fast as it is diminished by those who leave it.

B. Secondly, there are those who, having for their chief aim the well-being of those around them and of mankind generally, yet recognise as necessary even for the advancement of that object, a due regard for the well-being—the health, strength, cheerfulness, and even the material prosperity—of self.* This class like the first is small; but steadily increases in every advancing community.

C. Thirdly, come those who in all societies, at present, form far the greater part of the community,—those, viz., who think chiefly of their own interests or their families', yet, though not specially careful to increase the happiness of others, are not selfishly intent on their own well-being only.

D. Fourthly, there are those who think solely of themselves, or if they look beyond themselves care only for their nearest kinsfolk, consciously disregarding the interests of others, and seeking only in the struggle for life the advancement of themselves or their families.

E. Lastly, there are those who in their struggle to advance self are prepared to prey on others if need be, in other words wilfully to do mischief to others for their own advantage.

* One or two correspondents whose letters have been handed to me, seem still unable to dissociate the idea of self-regard from the idea of selfishness, and imagine the man who duly cares for his own well-being (as the only effective way of fitting himself to be useful to others) to be necessarily one who really has at heart only his own comfort. It might be shown that the man who selfishly seeks his own comfort really goes the worst possible way to secure his own happiness. But apart from this, such a man is not the man of whom I am speaking. I am inquiring what the man should do who really wishes to increase the happiness of those around him most effectively; and I show how his care for their happiness involves, if he is wise, a due regard for his own happiness and well-being also—and even primarily, because his existence and his fitness to do good necessarily came before the good he may be able to do.

One correspondent asks whether a man who could save life at his own peril, ought not, according to the views I have indicated, to consider whether his life might not be of greater value to the community than the life which he could save by sacrificing or endangering his own? I may remark in passing that the man who most freely acknowledged, as a matter of pure reasoning, that in such a case he ought to weigh his own life's worth against the worth of that other life, would probably be the first to risk his life for others; while the man who made cheap parade of his readiness to sacrifice his life would probably be the readiest to slink away at the moment of danger. We are not considering, however, what men should do under sudden impulse of danger affecting others—and especially the weak and tender. If we were we might point out that in such cases there is much more at issue than the mere value of the lives at

In this classification we consider only the actual conduct of the various orders, not their expressed opinions. Were these to be taken into account the classification would remain nominally unchanged, but the numbers belonging to the different classes would be very much altered. Most of the members of the body social in civilised and especially in Christian countries would be assigned in that case to Class A,—though every one knows that in reality this class is a very small one indeed. Class B would be scarcely changed in number, because while members of that class are ready to maintain that the views on which their conduct depends are in their opinion sound and just, these views are not such as the members of other classes are anxious to simulate. They are not popular views — like the self-sacrificing ones which so many pretend to hold but by no means really act upon.

It is tolerably obvious that the wellbeing of society as a whole requires that classes D and E shall not be unduly large compared with the whole number of the community. Whatever tends to diminish their number, and especially the number of class E must tend to increase the wellbeing, that is the happiness, of the social body. Class C, which always constitutes the main body, merges by insensible gradations into class D, and class D into class E. Comparatively slight changes, influences relatively unimportant, suffice to transfer large numbers from the indifferent class C to the self-seeking class D, and similarly slight changes may suffice to transfer many from the simply self-seeking class D to the noxious class E. The lines of distinction between the first three classes are more marked. Members of the first class are more apt, at present, to pass into the third class than into the second, though little, it should seem, is needed to make these (the self-forgetting, enemy-loving members of the community) pass into the section combining due care of self with anxious desire to increase the happiness and well-being of the social body. That any members of the second class should pass either into the first, whence most of them came, or into the third whose indifference to the welfare of others is unpleasing to them, or into the fourth whose selfishness is abhorrent to them, is unlikely; for which reason, this class should logically have occupied the first place, seeing that the class we have set first really merges both into the second and into the third, which should therefore be set on different sides of it. We had a reason however, which many will understand, for not depriving class A of the position it holds theoretically,—though practically the class has no such standing, and is especially contemned by class C, the noisiest in pretending to accept its principles.

Since, then, the welfare of the body social depends mainly on the relative smallness of classes D and E, the selfish and the noxious, it follows that an important if not the chief duty to society, for all who really and reasoningly desire the well-being and progress of the community, is so to regulate their conduct as to cause these classes to become relatively smaller and smaller. Conduct which can be shown to encourage the development of these classes, to make selfish ways pleasanter and noxious ways safer, is injurious to the body social, and is therefore *wrong;* while on the contrary conduct which tends to increase relatively the number of those who are considerate of the welfare of others, is beneficial to the community, tends to increase the happiness of the greater number, and is therefore right. If therefore it can be shown that the principle adopted by class A, however self-sacrificing, must tend to work far wider mischief in encouraging the development of selfishness and wrong-doing, than it can possibly effect in the way of good (the good being local and casual, the evil systematic and wide-spread), then will it become clear that the principle adopted by class B, which equally seeks the good of others but entirely avoids the risk of encouraging the selfish and the evil-disposed, is that which can alone lead to permanent improvement and happiness in the social body.

This, as we shall now proceed to show, is unquestionably the case.

(*To be continued.*)

stake. If I saw a child weak-minded, crippled, of small worth as a member of the body social, in danger from which I could save it at the risk or even the certainty of losing my own life (which I might judge of more value to the community), I trust that whether I had to act on impulse or after reflection, I should act, *not* as weighing the value of that life against my own, but rather as considering what would be the evil influence of cowardice and meanness in a community. If I had time to reason I might reason that whatever value my life might have, must go but a small way to counterbalance the effect of evil example.

In many cases, however, men are bound first to think of the value of their life: they do so even in cases where eventually they know that their life must be sacrificed. The captain of an endangered ship, for instance, cares for his own life more than for the life of any on board, *while* his skill and experience are necessary to save life; and his actions in detail might under conceivable circumstances seem suggestive of mean care for his own life, when he knows at the very time that, after he has seen off the last boat—perhaps before many minutes are past—he and his best officers must go down with the ship.

It is singular and significant, however, that cavils such as I have here touched upon, come without a single exception in letters otherwise so worded as to show inexperience, deficiency of reasoning power, or that turn of mind, unfairly regarded as specially belonging to the weaker sex, which does not reason at all but simply repeats parrot-like, and with constant reference to the last word, the maxims (often quite misunderstood) learned by rote in childhood.

THE CHEMISTRY OF COOKERY.

XXIX.

By W. Mattieu Williams.

NEXT to the enveloping tissue, the most abundant constituent of the vegetables we use as food is starch. Laundry associations may render the Latin name "*fecula*," or "*farina*," more agreeable when applied to food. We feed very largely on starch, and take it in a multitude of forms. Excluding water, it constitutes above three-fourths of our "staff of life;" a still larger proportion of rice, which is the staff of Oriental life, and nearly the whole of arrowroot, sago, and tapioca, which may be described as composed of starch and water. Peas, beans, and every kind of seed and grain contain it in preponderating proportions; potatoes the same, and even those vegetables which we eat bodily, all contain within their cells considerable quantities of starch.

Take a small piece of dough, made in the usual manner by moistening wheat flour, put it in a piece of muslin and work it with the fingers under water. The water becomes milky, and the milkiness is seen to be produced by minute granules that sink to the bottom when the agitation of the water ceases. These are starch granules. They may be obtained by similar treatment of other kinds of flour. Viewed under a microscope they are seen to be ovoid particles with peculiar concentric markings that I must not tarry to describe. The form and size of these granules vary according to the plant from which they are derived, but the chemical composition is in all cases the same, excepting, perhaps, that the amount of water associated with the actual starch varies, producing some small differences of density or other physical variations.

Taking arrowroot as an example. To the chemist arrowroot is starch in as pure a form as can be found in nature, and he applies this description to all kinds of arrowroot;

but, looking at the "price current" in the *Grocer* of the current week (Feb. 16), I find, under the first item, which is "Arrowroot," the following:—"Bermuda per lb., 1s. to to 2s;" "St. Vincent and Natal, 2¼d. to 8½d.;" and this is a fair example of the usual differences of price of this commodity. Nine farthings to 96 farthings is a wide range, and should express a wide difference of quality. I have on several occasions, at long intervals apart, obtained samples of the highest-priced Bermuda, and even "Missionary" arrowroot, supposed to be perfect, brought home by immaculate missionaries themselves, and therefore worth three-and-sixpence per pound, and have compared this with the twopenny or threepenny "St. Vincent and Natal." I find that the only difference is that on boiling in a given quantity of water the Bermuda produces a somewhat stiffer jelly, the which additional tenacity is easily obtainable by using a little more twopenny (or I will say fourpenny, to allow a good profit on retailing) to the same quantity of water. Putting it commercially, the Natal, as retailed at fourpence per lb., and the Bermuda at its usual retail price of three shillings, I may safely say that nine ounces of Natal, costing twopence farthing is equal to eight ounces of Bermuda, costing eighteenpence. Both are starch, and starch is neither more nor less than starch, unless it be that the best Bermuda at three shillings per lb. is starch *plus* humbug.

The ultimate chemical composition of starch is the same as that of cellulose—carbon and the elements of water, and in the same proportions; but the difference of chemical and physical properties indicates some difference in the arrangement of these elements. It would be quite out of place here to discuss the theories of molecular constitution which such differences have suggested, especially as they are all rather cloudy. The percentage is—carbon 44·4, oxygen 49·4, and hydrogen 6·2. The difference between starch and cellulose that most closely affects my present subject, that of digestibility, is considerable. The ordinary food-forms of starch, such as arrowroot, tapioca, rice, &c., are among the most easily digestible kinds of food, while cellulose is peculiarly difficult of digestion; in its crude and compact forms, it is quite indigestible by human digestive apparatus.

Neither of them are capable of sustaining life alone; they contain none of the nitrogenous material required for building up muscle, nerve, and other animal tissue. They may be converted into fat, and may supply fuel for maintaining animal heat, and may supply some of the energies demanded for organic work.

Serious consequences have resulted from ignorance of this, as shown in the practice of feeding invalids on arrowroot. The popular notion that anything which thickens to a jelly when cooked must be proportionally nutritious is very fallacious, and many a victim has died of starvation by the reliance of nurses on this theory, and consequently feeding an emaciated invalid on mere starch in the form of arrowroot, &c. The selling of a fancy variety at ten times its proper value has greatly aided this delusion, so many believing that whatever is dear must be good. I remember when oysters were retailed in London at fourpence per dozen. They were not then supposed to be exceptionally nutritious and prescribed to invalids, as they have been lately, since their price has risen to threepence each.

The change which takes place in the cookery of starch may, I think, be described as simple hydration, or union with water; not that definite chemical combination that may be expressed in terms of chemical equivalents, but a sort of hydration of which we have so many other examples, where something unites with water in any quantity, the union being accompanied with an evolution of some amount of heat. Striking illustrations of this are presented on placing a piece of hydrated soda or potash in water, or mixing sulphuric acid, already combined chemically with an equivalent of water, with more water. Here we have aqueous adhesion and considerable evolution of heat, without the definitive quantitative chemical combination demanded by atomic theories.

In the experiment above described for separating the starch from wheat flour, the starch thus liberated sinks to the bottom of the water and remains there undissolved. The same occurs if arrowroot be thrown into water. This insolubility is not entirely due to the intervention of the envelope of the granules, as may be shown by crushing the granules, *while dry*, and then dropping them into water. Such a mixture of starch and cold water remains unchanged for a long time—Miller says "an indefinite time."

When heated to a little above 140° Fah., an absorption of water takes place through the enveloping membrane of the granule, the grains swell up, and the mixture becomes pasty or viscous. If this paste be largely diluted with water the swollen granules still remain as separate bodies and slowly sink, though a considerable exosmosis of the true starch has occurred, as shown by the thickening of the water. It appears that in their original state the enveloping membrane is much folded, the folds probably forming the curious marking of concentric rings, which constitutes the characteristic microscopic structure of starch granules, and that when cooked, at the temperature named, the very delicate membrane becomes fully distended by the increased bulk of the hydrated and diluted starch.

A very little mechanical violence, mere stirring, now breaks up these distended granules, and we obtain the starch paste so well known to the laundress, and to all who have seen cooked arrowroot. If this paste be dried by evaporation it does not regain its former insolubility, but readily dissolves in hot or cold water. This is what I should describe as cooked starch.

Starch may be roasted as well as boiled, but with very different effects. The changes that then occur are much more decided and very interesting. I will describe them in my next.

WILD BEES.

BY S. A. BUTLER, B.A., B.Sc.

(*Continued from page 100.*)

SOMETIMES there may be found on the bodies of bees a curious little orange-coloured six-footed creature about one-tenth of an inch long. This is the young larva of a great, fat, lazy beetle, the Oil Beetle, so called from a nasty habit it has of causing drops of an oily fluid to exude from its joints whenever it is handled. The perfect beetle may frequently be seen lazily scrambling along over the grass in our fields. It is a bluish-black, flabby creature, with an abdomen of proportions quite aldermanic. It is not easy to understand what the larvæ of these beetles can want on a bee's back; but in some instances, at any rate, on being carried to the nest, they fall to on the larva of their host, and enrich themselves at its expense.

The humble-bees, or dumbledors, which constitute the genus *Bombus*, are subject to the parasitism of certain two-winged flies. The grubs of one of these live actually inside the bodies of the bees. Another is called *Volucella bombylans*; it is a beautiful creature, presenting a superficial resemblance to the bees in whose nests it lives; but the most curious fact about it is that its colour varies with the species on which it is parasitic. When

it lives with a yellow-banded white-tailed bee, it is itself yellow-banded and white-tailed; but when the host has a red tail and no yellow band the fly also is furnished with a like colouration. The fly-grubs devour the bee-grubs, and sometimes almost clear out the nest. But the *Bombi* are subject to more insidious parasitism than even that of *Volucella ;* certain *bees*, almost the exact counterparts of the *Bombi*, but differing in having no brush of hairs on their legs for the collection of pollen, live with them like the wasp-like cuckoo bees before-mentioned, and their grubs feed upon the store of nutriment laid up by the industrious hosts for their own progeny. Notwithstanding their "sponging" habits, these parasites live quite amicably with their hosts; who may, indeed, be unable to distinguish them from the legitimate owners of the nests.

Wild bees are either solitary or social. The solitary species live each in its own nest, while those that are social form larger or smaller communities, living in a single nest. Solitary bees may be, and often are, gregarious—that is, many burrows are found very near together, in the same bank, for example, but these are all separate abodes, and the inhabitants are all independent of one another. Amongst the solitary species there is, as I have already implied, a great diversity of habits. The burrows of the Plastering bees are adorned with layer upon layer of a

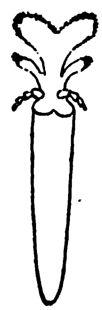


Fig. 2 *bis*.—Tongue of Plastering Bee.

most delicate membrane, something like gold-beater's skin, but much thinner; this is secreted by the insect itself, which uses its curiously-shaped tongue (Fig. 2) as a kind of trowel to plaster the secretion over the sides and end of its burrow. The Leaf-cutters (Fig. 7) line their nests with fragments of leaves, which they cut from various shrubs and trees. The cylindrical burrow having been prepared in sand,

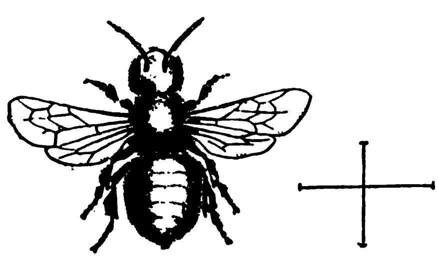


Fig. 7.—Megachile centuncularis. A Leaf-cutter Bee.

earth, or wood, as the case may be, the little labourer flies away to its chosen shrub—say a rosebush. It alights upon a leaf, and, fixing itself upon the edge, holds it with three legs on each side, and then with its mandibles begins to snip out an oval or semicircular cutting, biting its way backwards and holding on all the time to the piece, which is thus gradually detached. When the last bite is given, and the insect is about to fall with its prize, it spreads its wings and flies away in a "bee-line" to its home, where the leaf-cutting is duly deposited in a suitable position. Straight back again to the same plant the industrious creature flies, and in the same way makes another cutting, slightly altering the shape according to the requirements of the burrow. This is placed so as to lap slightly over the piece first placed in position, and then another and another is added, until the whole cell is made snug and comfortable for the reception of the egg and the food the young grub is to eat; then the cell is closed up with a number of circular leaf-cuttings, all obtained in the same way as before.

Like the Leaf-cutters, the Mason Bees are stoutly-built insects; their habits are very various, so much so, indeed, that they have been said to manifest greater diversity of instinct than any other group of bees. There is not that constancy in the choice of a situation for the construction of the nest that we find in most of the other groups. For example, the commonest species, *Osmia rufa*, exhibits a wonderful power of adapting itself to circumstances; in hilly country, or at the seaside, it often burrows in the sunny side of cliffs and in sandy banks, but in a cultivated district, especially when the soil is clayey, it will seek out some old willow tree and make its burrow in the decaying stump. Much more difficult situations are, however, not unfrequently chosen, *e.g.* the mortar of old walls. Nor does the insect always excavate its own burrow; it sometimes avails itself of some cleft or crevice already made; thus, its nests have been found in the lock of an out-house door and in a cavity in a flint used in the rockwork of a garden; but, most extraordinary of all, a nest was once formed in the tube of a fife that had been left in a garden arbour. When the fife was found, no less than fourteen cells had been made by the industrious little creature, and the fifteenth had been commenced; the bee had entered the fife at the lower end, and commenced its cells a little below the blow-hole. Some species form their nests in old snail shells, arranging the cells in different ways, as necessitated by the varying diameter of the whorls of the shell. Others, again, choose as a nidus the hollow straw-tubes in thatch, or a bramble stick from which the pith has been extracted. The cells are made of little particles of earth, stone, or other materials, agglutinated together by a gummy substance secreted by the insect itself. Both Leaf-cutters and Masons have the under surface of the abdomen densely clothed with pubescense, and with this brush of hairs they collect the pollen required for the support of their young.

The economy of the social bees is exceedingly interesting; the great buzzing, humble bees that are seen in the warm days of spring rifling the sallow blossoms, or calmly cutting their way through the air with self-satisfied hum, are the females, which have remained in a torpid state through the winter months, but have been revivified and brought forth from their retreats by the enlivening rays of the sun, as it daily mounts higher and higher in the heavens. The troubles of maternity are before them; and after they have made selection of a convenient cavity for the nest, a store of pollen has to be provided to meet the wants of the expected offspring. In addition to this, the parent bee constructs a number of receptacles called honey-pots, which she fills with a coarse kind of honey, the use of which is not certainly known. Then a few eggs are laid, which soon hatch, and the little creatures speedily pass through their metamorphoses; these are the neuters, or workers, and upon them now devolves the labour of enlarging the nest according to the needs of the community, as well as of providing for the wants of their future companions. It is

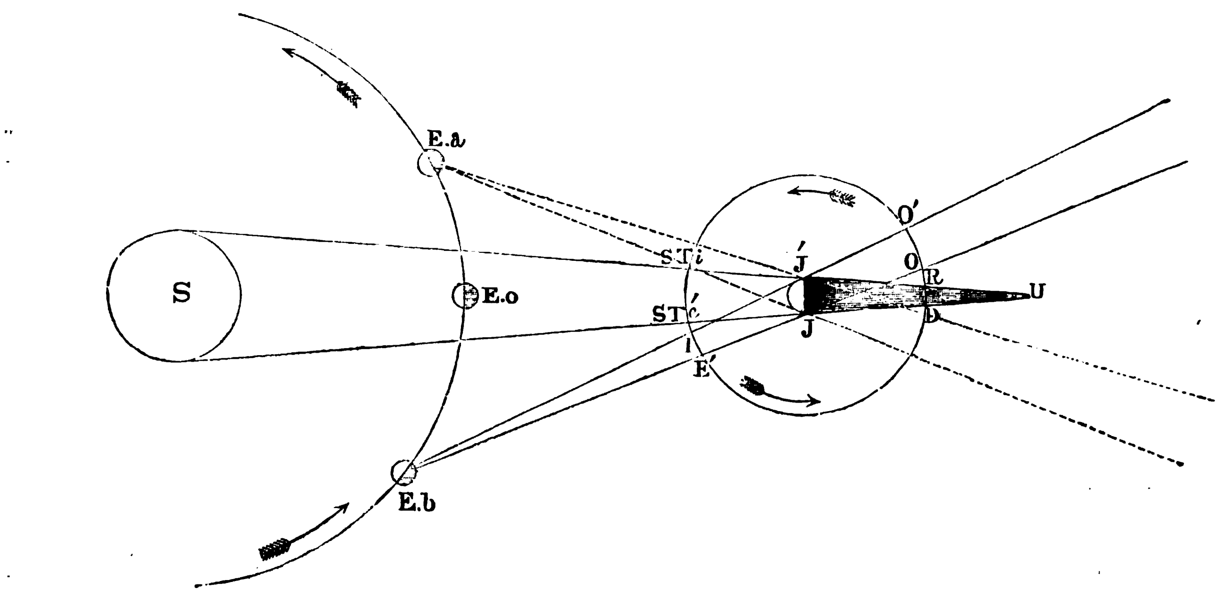
not till the season is considerably advanced that young females appear, and these are followed after a time by the males. The females are often very much larger than either the males or the workers, and sometimes the latter are exceedingly diminutive, when compared with the mother of them all. Some of the Bombi construct their nests on the surface of the ground, and others at some depth underneath, and it is a remarkable fact that this difference of habit is accompanied by a difference of temperament in the insects, the underground builders being much more pugnacious and vindictive that those that build at the surface. The nests of the latter consist of piles of little bits of grass, moss, &c., collected with great assiduity, and arranged with considerable care and skill by the little artificers. Occasionally they will adapt birds' nests to their requirements, apparently turning out the real owner, sometimes even after its eggs have been laid. The number of bees constituting one community of course varies with the season, being greatest in the autumn, when all the sexes are found. At this time the nest of one of the carder bees has been found to contain about 120 specimens, of which about half were workers. In no case is the population anything like so dense as with the hive-bee. It may be as well to mention that honey-comb is not manufactured by any of our wild bees.

JUPITER IN A THREE-INCH TELESCOPE.

By a Fellow of the Royal Astronomical Society.

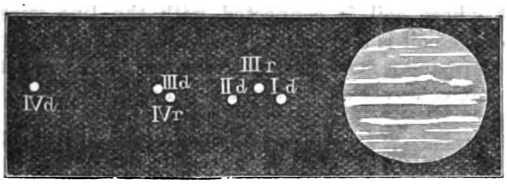
(Continued from page 102.)

BEAUTIFUL and remarkable as are the unstable details which diversify Jupiter's face, they are, in one sense, almost monotonous as compared with the perpetually-changing phenomena of the four moons which circle round him. Of these satellites the first, second, and fourth appear as stars of the seventh magnitude, and the third as a sixth magnitude star. When a satellite crosses Jupiter's face, it is said to "transit" him; its entry on to his disc being called its ingress, and the instant of its leaving his opposite limb its egress. When it passes actually behind the planet it is said to be "occulted;" and when it plunges into his hadow, to be "eclipsed." We are unwilling to introduce anything into these papers not strictly within the scope embraced by their title; but in order to render what we are about to describe intelligible, it will be necessary to enter into certain elementary explanations of the conditions under which we view Jovian phenomena from our terrestrial standpoint. Leaving, then, our telescope for a few minutes, let S in our figure be the sun, E*b*, E*o*, E*a* the earth travelling round him in the direction of the curved arrow; J, Jupiter also going round the sun in the same direction, but so much more slowly that—for our present purpose—we may regard him as standing still. Then, evidently, Jupiter will cast the conical shadow JUJ' out behind him into space. Let us call D, R, ST*i*, ST'*e*, the orbit of one of his outer satellites, and conceive it to coincide with the plane of the ecliptic. From E*b* draw the lines E*b*J, E*b*J' meeting the path of the satellite at I and E'. Now, imagine the earth at E*b*, *i.e.*, before Jupiter comes into opposition (say about the end of last November). Then, when a satellite is at the point I in its orbit on the line E*b*J', it is seen to enter on to Jupiter's eastern limb, and, when it arrives at E', to leave his western limb. This,

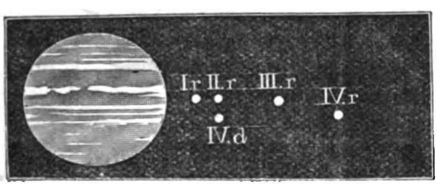


then, is a Transit of the Satellite. A glance at the figure will show that (independently altogether of the earth's position) when this same satellite passes between the points ST*i* and ST'*e* its shadow must be projected on to Jupiter's face, just as the shadow of our own moon is projected on the earth in an eclipse of the sun, and it is seen to cross the planet as a round, black spot. Furthermore, when the satellite plunges into the planet's shadow at D, it disappears in eclipse, to reappear (under the conditions we are supposing) at R. It will, however, be noted that this reappearance from eclipse is only to be followed by occultation behind the body of Jupiter when the satellite reaches O, the satellite finally reappearing at Jupiter's opposite limb when it reaches O'. What we have said, be it remarked, applies only in its entirety to the two outer satellites. The inner ones, which describe smaller circles round the planet, disappear in eclipse, to reappear *from occultation*, as they emerge from the actual shadow behind the body of the planet. It will be further remarked that while the shadow of the satellite enters on to Jupiter's face when the satellite reaches the point ST*i*; the satellite itself does not follow it on to the limb of the planet, to a terrestrial observer, until it arrives at the point I in its orbit. Thus, to sum up, before opposition,

the shadows precede the satellite casting them, in their transits; the inner satellites suffer eclipse and re-appear from occultation, and the outer satellites may both disappear and re-appear from eclipse on the western side of the planet to be subsequently occulted by it. When Jupiter is actually in opposition (E*o* in our figure), evidently the satellites will be actually superposed on their shadows as they cross the disc of the planet; and, as the whole of the shadow cone is hidden behind him, occultations only, and no eclipses, can take place. After opposition, as at present (a condition of things represented at E*a* above), the sequence of phenomena is obviously reversed, the satellites precede their shadows over Jupiter's face; the inner satellites are occulted by the planet, and re-appear from eclipse; and the outer satellites may disappear in, and re-appear from, occultation to be subsequently eclipsed. The student will now be prepared to understand that when our sketch of Jupiter was made on the night of Jan. 24, the planet having passed opposition a few days previously, Satellite I., which was about to leave his disc, after crossing it in transit, was slightly in advance of its shadow. In fact, the shadow did not leave his limb for seven minutes after the satellite had quitted it. Near quadrature an outer satellite may have left the planet's face for an hour or two before its shadow even enters on to it! The annexed two small diagrams represent, approximately to scale, the points of disappearance in and re-appearance from eclipse of the four satellites as seen in an inverting telescope during the months of December, 1883, and February, 1884. After what we have said, they ought to be perfectly intelligible.



Eclipses of Jupiter's Satellites (Dec., 1883).



Eclipses of Jupiter's Satellites (Feb., 1884).

It only remains, in conclusion, to refer to certain curious phenomena, for which the observer should always be on the alert. In the case of occultations, to begin with, the satellites have been seen apparently projected on the planet's disc; although it seems probable that they were rather seen *through* Jupiter's limb. A star occulted by Jupiter has been seen, in a very large telescope, to fade away in a manner which affords strong confirmation of this idea. When a satellite begins its transit, it may be traced fairly on to the planet as a brilliant spot; but it generally disappears after getting some distance within the limb, its re-appearance happening as it is about to pass off on the other side of the planet. We have said that a satellite "generally" disappears when well within the planet's limb; but very remarkable exceptions indeed to this rule have been witnessed.

We have ourselves seen Satellite III. quite as dark in appearance as its own shadow when transiting Jupiter, and the same effect has been noticed with IV., and even, more rarely, with II. Again, the shadows, although normally like ink-spots, have been seen of curiously diversified colours. Those of Satellites I. and II. have been noted as grey. Noble saw the shadow of II. a chocolate brown in October, 1880, and attempted to account for this phenomenon by the supposition that the sun's light must have been shut off from a part of Jupiter's surface glowing with a dull red heat! But, as we remarked in connection with the phenomena of the belts, our object here is not to give a mere list of prior observations, but rather to direct the beginner in his own. Under any circumstances we would fain hope that we have said enough to stimulate him to pursue the study of so interesting a system as that of which we are treating, and to impress him with something of the charm and pleasure of the investigation of the leading characteristics of the Jovian system, even in a three-inch telescope.

MADNESS AND MARRIAGE.*

THE *Medico-Legal Journal* number for December which has just reached us contains much interesting and suggestive matter in relation to the insane. The following passage is worth careful consideration :—

"MARRIAGE OF THE INSANE.—Dr. C. H. Hughes, of St. Louis, says: that marriage of all insane persons at certain ages should be interdicted by law, and the victims also of such diseases as entail insanity or epilepsia, should also be forbidden to enter into matrimony before the sterile time. In behalf of the rights of the insane, who would wish to have a maimed offspring, if under the dominion of their right reason, it should be lawful for proper persons to forbid such disastrous banns, and the duty of the State to prevent them? It is a terrible thing for the State to tacitly consent to such deterioration of the race as is caused by such marriages; and duty to humanity, sane and insane, demands repressive legislation. No "pestilence that walked in darkness, or destruction that has wasted at noon-day," ever called more loudly for State intervention against their spread, than the distinctive heritage of the neuropathic diathesis" [which we may call nerve-taint, for convenience] "calls for the concern of the State. Its evil influences are all about us, even more disastrous than any plague or pestilence, afflicting the humblest citizen as well as the highest, and their posterity.

[We may remark in passing that there would seem to be a comic compositor among those who attend to the *Medico-Legal Journal*. In one place "Folie Circulaire" is altered into "Folio circulaire," in another we learn that "a proper regard for the rights of the insane before the law should secure for them rulings by courts in accordance with the nature of their *melody*."]

As water may be said to be the chief source of wealth in Australia, the following telegram from the officer in charge of borings at Bourke, in New South Wales, is, says the *Engineer*, important :—"We have struck a further supply of fresh water in Bore B, No. 3, at a depth of 192 ft., in a layer of granite pebbles and boulders. This water is quite distinct from that struck at 122 ft. We have not yet gauged the quantity, but the water has risen 10 ft. over the surface in pipes. We are of opinion that we are only coming on the main water-bearing strata, and will have a greater supply when the bore gets deeper into the gravel bed."

* Published by the *Medico-Legal Journal* Association, 128, Broadway, New York.

WHO INVENTED THE TELEPHONE?

By W. Slingo.

THIS question is one which has lately caused considerable excitement in electrical circles, and is one upon which large financial interests depend. Although the financial side of the question is not one which concerns us to any great extent (nor for the matter of that is the claim for priority of invention *in the abstract*), nevertheless, the issue is so important in its bearings upon our social system, that the impulse to call attention to very recent revelations is irresistible. Professor S. P. Thompson has struggled very hard to prove that Reis invented, twenty years since, a telephonic system embracing the features of the Bell and Edison patents. Supposing, however, that the evidence in favour of Reis lacks convincing power, such a charge against the claims of a later worker are and have been declared by an American Court, against whose decision there is said to be no appeal, to be untenable.

The *Electrical Review* for the 16th inst. contains a communication from the States, in which a long account of the productions of the new claimant, Daniel Drawbaugh, are given. It appears that The People's Telephone Company (New York), in defending an action commenced in April, 1881, for infringement by the American Bell Telephone Company, assert Daniel Drawbaugh to be the

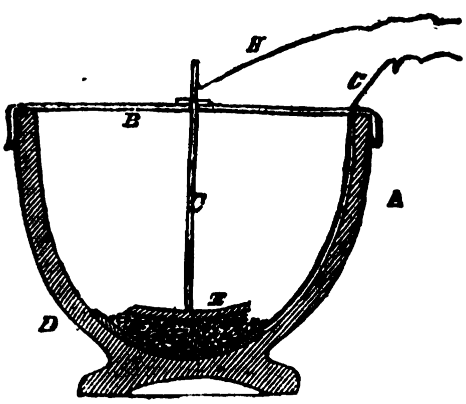


Fig. 1.

first and original inventor, and that he is entitled to the broadest possible patent for the telephone and the art of telephony. One hundred and forty-five witnesses have "identified certain telephonic instruments which they had seen and heard talk in Drawbaugh's shop from 1867 to 1876" (the year in which Bell's first patent was taken out), and two experts have testified "that the Drawbaugh talking machines were electric speaking telephones, embodying and containing all the telephone inventions patented by Bell, and that they were practical working instruments, and as good articulating telephones as they had ever heard." Space cannot be spared to describe the whole of the apparatus made by Drawbaugh, but the more important instruments call for notice.

It is probably generally known amongst the readers of Knowledge that the telephone system of Bell and Edison consists essentially of a transmitter and a receiver—the former consisting of pieces of more or less loose carbon introduced into a battery circuit, and operating on the same principle as that of the Hughes' microphone. A varying current is thus sent along the line, and on arriving at the receiving station passes through a coil of wire enveloping the pole of a magnet, which pole is thereby strengthened or weakened according to the direction and strength of the current. A thin disc of iron rigidly fixed at its edges or circumference faces the magnet-pole. The magnet's strength suffering a change according to the intensity and direction of the current, the attraction of the disc varies proportionately, and so vibrating in response to every movement of the sounding-board and loose carbon at the sending end of the line, speech and other sounds are transmitted. The first piece of Drawbaugh's apparatus, antedating Edison ten years, is illustrated in Fig. 1 (a facsimile of Drawbaugh's sketch). It consists of a china teacup, A, with a flexible membrane, B, stretched over the top. A metal rod, C, extends from the membrane to a metal plate, E, resting on a mass of powdered material, F (several substances were tried, but charcoal and other forms of carbon were eventually adopted as giving the best result), under which is another metal plate, D, connected to the wire, G. The current from the battery enters at G, passes through D, F, E, C, to the line wire, H. Drawbaugh's

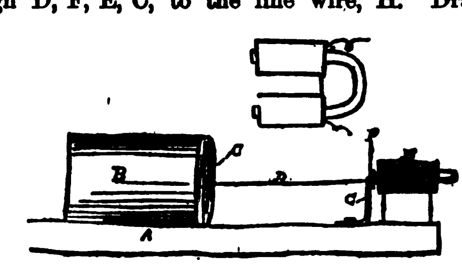


Fig. 2.

early receiver is represented in Fig. 2, in which B is an old mustard can fastened to a board, A. E is an electro-magnet whose coil is connected with the line wire. In front of the coil is an iron armature supported on a spring, G, and carrying a string, D, attached at its other end to a membrane, C, stretched across the end of the can, B.

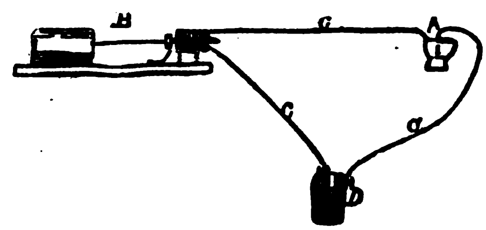


Fig. 3.

The whole arrangement is represented in the facsimile sketch Fig. 3, in which D is the battery and A the cup containing the carbon, which, by offering a varying resistance to the passage of the current, varies the strength of that current. B is the receiver at the distant end of the line.

Several modifications were made, including the application of a wooden mouthpiece to the transmitter (an exact antetypye of those now in use) and the suppression of the string in the receiver, the armature plate being attached direct to the membrane.

In 1870, Drawbaugh had found out that the telephone could be made to operate without a battery by substituting permanent magnets for electro magnets. An iron diaphragm was adopted, and a telephone (which could transmit and receive speech equally well) was made in which every feature of the Bell instrument was introduced. The instrument is represented in Fig. 4, which almost explains itself. G is the magnet which carries a coil of wire with a soft iron core on each pole, opposite the cores being the iron diaphragm. D is the mouthpiece. It will be seen that the only difference between this and Bell's instrument is

that both poles of the magnet are employed. This instrument was, however, somewhat "large and cumbrous," but within a year smaller forms were made, the working parts being placed in a cylindrical box 5 in. in diameter; and in

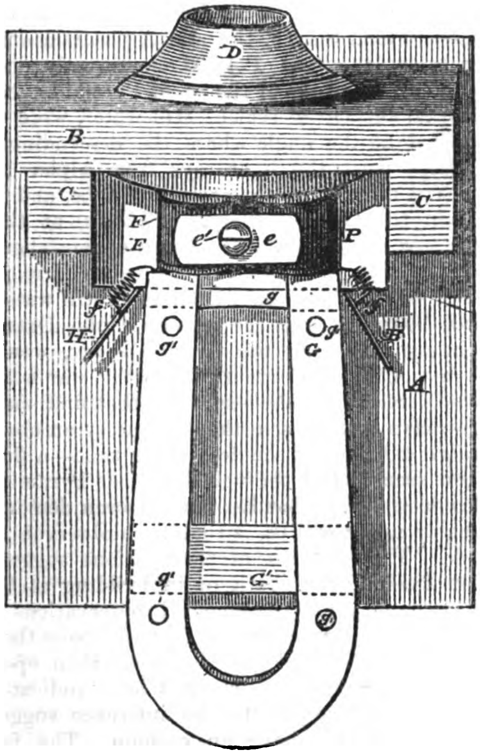


Fig. 4.

January, 1875, an instrument was made in which, by using a curled magnet, the size was reduced to 3 in. in diameter by $1\frac{1}{2}$ in. thick.

Fig. 5 illustrates the progress made up to the beginning of 1876 with carbon transmitters. Here the plate diaphragm is used, and, instead of loose carbon powder, blocks

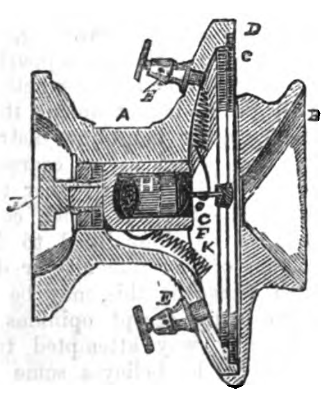


Fig. 5.

of hard gas carbon, held normally in contact by spring pressure, are employed. This is said to be an exceedingly sensitive transmitting instrument, which was, however, further improved during the same year.

Thus it may be seen that Bell and Edison were anticipated by several years. The patents granted to them in America have, if the information referred to above be correct, been declared void, and the telephone made free to all. The result upon the share market comes but little within our province, although it is worth mentioning that the shares of the Bell Company fell in one hour from 180 to 130 dols. What the effect in England will be is not yet clear, as it will have to be proved that Drawbaugh's discoveries were known here prior to 1876. For the sake of the community it is to be hoped they were.

Reviews.

HEATH'S FERN PORTFOLIO.

THE first of what promises to be a splendid series of fern pictures presents the Royal Fern (*Osmunda regalis*). As this fern attains under favourable conditions a height of twelve feet, it is obviously impossible to present within the compass of a folio page a facsimile of a complete frond. But the upper part of a fertile frond here presented is of the natural size and an absolute facsimile of the subject from which it is taken. The picture is very beautiful as well as correct. Mr. Heath sends us for insertion as a paragraph a statement which (freed from the common but somewhat undignified jargon used in such cases) is to this effect:—He has forwarded to the Queen a copy of the *Osmunda regalis*, and received her thanks, through General Ponsonby. We do not understand Mr. Heath to say that this communication comments upon the accuracy and beauty of his fern-picture, either favourably or unfavourably, so that we are somewhat at a loss to know precisely *why* he wishes the statement made here. But we very willingly accede to his request.

Gossip.

NEXT week KNOWLEDGE will enter on a new career, bearing the same price thenceforward as the *Athenæum*, the *Academy*, and our old friend *Punch*. Enlarged to 32 pages, of which 22 pages on the average (alternating probably for convenience of binding between 20 and 24 pages) will be reading matter, KNOWLEDGE will be able far more effectively to fulfil the purpose for which it was originally intended. Now indeed that the change is decided upon, I may state that the question upon which readers were invited to express an opinion was whether KNOWLEDGE should continue to exist or not; for I had been so dissatisfied with the way in which, owing to limited space, the subjects dealt with here had to be broken up, or else the style and character of the paper altered, that I had almost decided to give up an effort which was leading to such (to me) unsatisfactory results. The communications of several hundreds of correspondents show that this feeling was fully justified. They also, and no doubt the bulk of the readers (for we may fairly assume that the correspondents represented the whole constituency of KNOWLEDGE) had felt that more space was wanted for the fulfilment of the paper's purpose. I can only regret that I had not sooner known the general feeling of readers.

WE do not propose to make KNOWLEDGE hereafter more miscellaneous than it has been, but to treat the various subjects dealt with more fully and more continuously. Notices of books will appear more frequently than heretofore. Messrs. Slack, Grant Allen, M. Williams, E. Clodd, T. Foster, S. Butler, W. Slingo, and *the* Fellow of the Royal Astronomical Society (who has so long under that *nom de plume* instructed and delighted the readers

of the *English Mechanic*), will (we hope and believe) continue their serial or occasional contributions (as the case may be). The long-promised papers on Light Sifting (or Spectroscopic Analysis) will be commenced, and those on How to get Strong resumed and continued systematically till completed. The Editor will join as of yore in the work. We shall add some papers on optical and mechanical matters; and we hope fortnightly to give passages, with notes, illustrative of the work of "English Men of Letters." Our Mathematical column will alternately deal with Euclidean and Cartesian Geometry (a year's supply of each has accumulated). Whist, a subject which we find interests a great number of specially scientific readers, will be continued regularly, and usually a hand with notes illustrating the strategy of the game will appear weekly. In Chess we are anxious to return more to the method which we proposed to follow at the outset. Problems of five, six, or seven moves are not much cared for, nor are sui-mates very instructive to those players for whom we chiefly wish to cater, namely, those by whom Chess is regarded as a recreation rather than as a study. Even the examination of the leading openings, though in my opinion essential to the real enjoyment of the game, should in such a magazine as this be limited to the lines interesting to amateurs. We have suggested these considerations to the skilful player who deals with chequered fate under a fear-inspiring name. Possibly the Chess Column may hereafter be described as conducted by Mephisto (primarily) and the Editor.

Correspondence will weekly occupy a page or two more, perhaps, on the average than of yore. But we must remind some correspondents who have expressed regret because their communications have not appeared, that an editor is apt to be very roundly denounced if he allows the correspondence columns to become too numerous.

As regards Editorial Gossip, many kindly correspondents express a feeling that this should form a feature as of old in Knowledge. It is very pleasant to me that it should be so. In fact I may say that I fancy I should very soon have tired of Knowledge if I had not felt something like personal friendship for all its readers, even for those among them who hold opinions such as Knowledge strives to controvert.

We have received, too late for insertion in the present number, a letter from the Edinburgh Reviewer of Mr. Spencer's Philosophy, which shall appear next week.

We have received a note from Mr. Newton Crosland (for which we hope to find space next week) in which he expresses the mistaken idea that in our remarks last week on his pamphlet we wished to ridicule his belief. Nothing could be farther from our thoughts. We said he could not understand how, according to Newtonian astronomy, the variations of the centrifugal tendency is explained. There is no ridicule here, any more than there is self-ridicule in his remark (see his letter this week) that he cannot follow Newton's mathematical demonstration. His second letter, intended to set him right, really sets us right, as will be seen when it appears.

A correspondent of more vehemence than *vim* jeers at a supposed statement of mine that the rays of Alpha Draconis, the Pole star some 5,300 years ago, would at that time have been reflected by a horizontal mirror in latitude 30° north to the star Alpha Centauri, *quam proxime*, at the hour when the former Alpha was due north (below the Pole), the latter Alpha due south. I have made no statement of the kind. Three statements I have made in my little work on the Great Pyramid, to the following effect. *First*, at the time specified, as well as at the later date mentioned by Sir John Herschel, and adopted by Professor Piazzi Smyth, the star Alpha Draconis when northing below the true Pole shone directly down the descending entrance tube of the Great Pyramid. *Secondly*, if a horizontal surface, as that of still water, were at that time provided at the angle where the descending and ascending tubes meet, the light rays from Alpha Draconis would be sent directly up the ascending tube, and into its prolongation the Great Gallery. *Thirdly*, at the same epoch, the star Alpha Centauri when southing shone down the Great Gallery into the ascending tube. I might have added that, *fourthly*, the same surface of still water which at another hour had sent the rays of Alpha Draconis up the ascending tube into the Great Gallery, would have sent the rays of Alpha Centauri, when the star was southing, up the descending tube—so that that first magnitude star might have been seen in the daytime by an observer at the northern entrance of the descending tube—a first magnitude star seen in fact through the Great Pyramid! All this is demonstrable. What has not yet been demonstrated, and probably never will be, is that such observations were ever made. The neatness of the relations suggests that the Great Pyramid was built or in building at the time specified, and used for astronomical observations in that way. All I have said however is that if it were then being built, and the tubes and galleries were then open, they would have subserved the purposes I have indicated. On this point I am certain; as to the inference suggested by this certainty, I only have an opinion. The facts are curious any way, whether they indicate a set purpose or a merely accidental coincidence.

There are adverse opinions which tend, if reasonably considered, to strengthen rather than to weaken a man's faith in the views which have commended themselves to him as sound,—just as there are favourable opinions which seriously shake his confidence. When an adverse opinion is rudely or coarsely advanced, or is expressed in ill-chosen words, or supported by absurd reasoning, we naturally think such opposition akin rather to support. We feel at least a sense of relief that *this* man,—whether he be rude, or ill-trained, or illogical,—is not among those who share our opinions. On the other hand how distressing it is to hear our favoured views illogically or coarsely advocated! One begins at once to feel doubt whether there must not be something wrong about views which could commend themselves to a mind so ill-qualified to form a sound opinion on a question of any difficulty or delicacy. Yet relief, even in such a case as this, may be found in the recollection that so many accept opinions from others, without having in any way attempted to form them. When one who believes he believes some opinion to be sound is unfortunately moved to give a reason for holding or thinking he holds that opinion, he is bound to give bad reasons even for opinions which may be perfectly sound. We must not rashly then begin to doubt opinions merely because they are ill supported by persons manifestly unwise.

Mr. Thos. Foster, writing to the *Daily News* from Bangor, N.W., on the 22nd, calls attention to the common mistake of spelling Anglesey *Anglesea*. As he justly remarks one might as reasonably write Jersea or Guernsea. He remarks also that the name Llewelyn is very commonly

misspelt *Llewellyn*, which to a Welshman is absurd. We may add that the spelling *Welch*, *Welchman*, *Scotch* (for Scot or Scottish), *Scotchman*, are equally objectionable.

Australian Belief.—All the tribes believe that the earth is flat, and that the sky is propped up on poles. Beyond the sky is the gum-tree country, the home of spirits and ghosts. Every man has within him a *Yambo*, or spirit, which can leave his body and wander even to the gum-tree country and talk with the spirits there, or converse with the wandering ghosts of other sleepers. The state of departed souls and their doings after leaving the human body fill a great part of Australian mythology. The dead are buried doubled up, the body lying on the side, and the usual deposit is made of the personal effects of the deceased. Mr. Howitt, in the *Journal of the Anthropological Institute.*

The Indian Population in the United States.—From a census compiled by Mr. Sherman Day in the *Overland Monthly* it appears that the Indian population in the United States numbers about 340,000. Most of them belong to the Territories. In the Indian Territory there are 79,000; in New Mexico, 36,500; in Dakota, 32,487; in Alaska, 31,000; in Montana, 20,400. Of the States California has an Indian population of 17,925; Michigan, 17,044; Wisconsin, 10,917; Nebraska, 4,174. In the Territories there are 247,312 Indians, of whom 193,631 are Agency Indians, in a total population of 1,012,091 (including Agency Indians). In the States there are 91,786, of whom 41,890 are Agency Indians, in a total population of 49,418,560.

A Negative Thermometer.—The ordinary mercurial thermometer is, as is well-known, based on the dilatation of bodies by the action of heat, and on the difference of dilatation between mercury and glass. A new thermometer, in which the mercury column sinks with a rise of temperature, has, however, been introduced by M. D. Latschinoff, who has based his instrument on the discovery of Kohlrausch that the co-efficient of dilatation of ebonite is greater than that of mercury. Latschinoff has made the reservoir of his thermometer of ebonite, and the result is that the level of the mercury falls in it when the temperature rises, and, on the contrary, rises when the temperature falls. A rise of 20 deg. Cent. lowers the mercury 25 millimètres. The result is curious; but ebonite is so untried a substance for thermometry, that we may be permitted to doubt if it will give accurate and constant indications with lapse of time.—*Engineering.*

Quaint old Fuller speaking of the map-makers of his day says that "Though the greater part of their land is undiscovered, yet rather than they will have their maps naked and bald, they periwig them with false hair, and fill up the vacuum (especially towards the north) with imaginary plains of *Ung* and *Gog*, and the plains of *Bargu*." "So true is it," he adds, "what one saith wittily in the comedy, 'that Phantastes, the servant of Geographus, travelled farther beyond the Arctic circle than did ever his master.'" Although modern geographers are less imaginative, yet there are still a few regions, especially towards the poles, respecting which they must be content to say little, unless they will permit "Phantastes, the servant of Geographus" to range farther than his master has yet been able to travel. This is a case, however, in which, in the absence of exact information, it may be possible to theorise with some hope of instruction, since, though our seamen have not yet been able to push their way to the north pole they have discovered much which tends to give us an insight into the relations presented in the as yet unexamined portions of the Arctic and Antarctic seas.

THE FACE OF THE SKY.

From February 29 to March 14.

By F.R.A.S.

The sun continues to exhibit pretty numerous spots, faculæ, and other indications of disturbance, which should hence be looked for whenever he is visible. After sunset, the Zodiacal light may be traced as a blunt cone of faint hazy light over that part of the horizon below which he has sunk, and having its axis roughly coincident with the Ecliptic. Map III. of "The Stars in their Seasons" shows the aspect of the night sky. Mercury is a morning star, but is in a bad position for the observer. Venus is a brilliant and conspicuous object in the western evening sky, and does not set until between 9 and 10 o'clock at night. She is still gibbous in the telescope. She will be occulted by the moon a few minutes after 3 o'clock this afternoon (29th), rather more than half an hour after she has passed the meridian. At this time she will be very nearly 45° above the horizon. She will remain behind the moon for a little more than an hour and a quarter. Mars is visible all night long in the constellation Cancer (Zodiacal Map, p. 70), but his angular diameter is dwindling, and he requires a pretty powerful telescope to show much detail on his surface. Jupiter is still visible all night long, and is the chief gem of the nocturnal sky. He is, however, best seen between the hours of 7 and 11 p.m. He is sensibly stationary at a point on the confines of Gemini and Cancer (Zodiacal Map, p. 40). During the next fourteen days numerous phenomena of his satellites will occur to interest the student. Forty-three minutes after midnight to-night Satellite I. will be occulted. On March 1, the transit of Satellite I. will begin at 10h. 2m. p.m.; as will that of its shadow at 10h. 58m. The satellite will pass off at 12h. 22m. p.m.; the shadow between 1 and 2 o'clock the next morning. On the evening of the 2nd, at 7h. 10m., Satellite I. will be occulted, to re-appear from eclipse at 10h. 24m. 14s. Meanwhile, Satellite III. will re-appear from eclipse at 7h. 51m. 54s. p.m. On the 3rd Satellite I. will leave Jupiter's face at 6h. 49m. p.m., followed by its shadow at 7h. 47m. Satellite IV. will be eclipsed on the 4th at 9h. 48m. 41s. p.m. This is a phenomenon which should be carefully watched. On the 6th Satellite II. will be occulted at 8h. 36m. p.m., to reappear from eclipse between 1h. and 2m. a.m. on the 7th. On the 8th the egress of the shadow of Satellite II. will happen at 7h. 42m. p.m., and at 11h. 51m. Satellite I. will begin its transit. The shadow which it casts will not enter on to the planet's limb until 53 minutes after midnight. On the 9th Satellite III. will reappear from occultation at 7h. 41m. p.m.; only, however, to plunge in eclipse into the shadow of the planet at 8h. 28m. 44s. Satellite I. will be occulted at 8h. 59m.; and at 11h. 52m. 3s. Satellite III. will reappear from eclipse; as will Satellite I. 19m. 25s. after midnight. On the 10th the ingress of the shadow of Satellite I. on to Jupiter's disc occurs at 7h. 22m. p.m., the satellite casting it leaving his opposite limb at 8h. 38m. The shadow passes off at 9h. 42m. On the 11th Satellite I. reappears from eclipse at 6h. 48m. 10s. p.m. On the 12th Satellite IV. begins its transit at 7h. 41m. p.m., and passes off the planet's opposite limb 8 minutes after midnight. This transit, too, should be carefully watched; if possible, from beginning to end. Lastly, Satellite II. will disappear in occultation at 11 o'clock on the night of the 13th. Saturn should be looked for as soon after dark as possible, as he is steadily approaching the west. Saturn is travelling slowly towards ϵ Tauri ("The Stars in their Seasons," Map I.). Uranus may be well seen after 10 or 11 o'clock at night, now, between β and η Virginis, which last-named star he is approaching ("The Stars in their Seasons," Map V.). His pale-blue disc in the telescope will readily distinguish him from surrounding stars. Neptune has disappeared for our present purpose, until he becomes a morning star in the late autumn. The moon's age at noon to-day is 2·7 days; and quite evidently 16·7 days at the same hour on March 14. Occultations of stars by the moon are tolerably numerous during the next fourteen days. Beginning with March 3, δ^1 Tauri, a 4th magnitude star, will disappear at the moon's dark limb at 10h. 44m. p.m., at an angle of 156° from her vertex, and will reappear at her bright limb at 11h. 35m. p.m., at a vertical angle of 287°. On the same night, δ^2 Tauri, a star of the 6th magnitude, will disappear at her dark limb at 11h. 10m., at a vertical angle of 134°; reappearing at her bright limb 5 minutes after midnight, at an angle from her vertex of 308°. On the 4th, the 6th magnitude star, 115 Tauri, will disappear at the moon's dark limb at 12h. 51m. p.m., at an angle of 85° from her vertex, to reappear at her bright limb at 1h. 31m. the next morning, at an angle from her vertex of 344°. On the 6th, λ Geminorum, a star of the 3½ magnitude, will disappear at the moon's dark limb at 10h. 10m. p.m., at a vertical angle of 134°. The star will reappear at the bright limb of the moon at an angle of 262° from

her vertex at 11h. 11m. p.m. On the 8th, κ Cancri, a 5th magnitude star, will disappear at the dark limb of the moon at 10h. 18m. p.m., at an angle of 53° from her vertex, reappearing at her bright limb at 11h. 29m. p.m. at a vertical angle of 292°. On the 10th, 34 Sextantis, a 6th magnitude star, will disappear at the moon's dark limb at 6h. 13m. p.m. at an angle of 5° from her vertex. It will reappear at 7h. 8m. p.m. at her bright limb at an angle of 243° from her vertex. Lastly, on the 11th, υ Leonis, a star of the 4½th magnitude, will disappear at the moon's bright limb at 9h. 22m. p.m. at a vertical angle of 70°, emerging from behind the dark limb at an angle of 193° from the vertex of the moon at 10h. 25m. We have said that the emersion will take place at the dark limb; but from the moon being full only an hour or two previously, the effect will be that of emerging from an illuminated limb. The moon is in Pisces to-day; but at 7 o'clock to-morrow morning she leaves that constellation for a little corner of Cetus, which she takes about four hours to cross before entering Aries. She takes until 10h. p.m. on March 2nd to traverse Aries and enter Taurus. She continues in Taurus until 10h. a.m. on the 5th, when she passes into the extreme northern part of Orion; which, twelve hours later, she quits for Gemini. Her passage across Gemini occupies until 2 o'clock in the afternoon of the 7th, at which hour she passes into Cancer. She is travelling through Cancer until 5h. a.m. on the 9th, when she enters Leo. At 5h. a.m. on the 10th she descends into Sextans, to re-emerge at 11 o'clock the same night into Leo. She finally quits Leo for Virgo at 1h. a.m. on the 12th, and is still in the confines of that great constellation when our notes terminate.

THE average expenditure on all highways in England is rather under £18 per mile; but of those managed by highway boards the cost amounts to fully £18½. Comparing one county with another, the figures show much greater discrepancy than can be easily explained. In Devonshire the boards pay less per mile for repairs than anywhere else, excepting only in Hampshire. In these two counties the cost is as low as £9 per mile; whereas in Shropshire, which comes next on the list, it is £11, in Cornwall and Dorset £11½, and in Somersetshire £17. No other county pays much less than £20 a mile, and in Berkshire, Durham, Northampton, and Lancashire the expense is about £30. Least economical of all is Surrey, which pays £37 a mile. The cost of road metal probably explains much of this, but the *St. James's Gazette* thinks that "the expenditure seems to bear no regular proportion whatever either to the geological condition of the district, the facility of procuring materials and labour, or to any circumstance which might be expected to raise or lower the cost of road-making."

IN an article on gutta-percha in the *Journal of the Society of Arts*, Mr. James Collins says:—"Dr. Oxley calculated that to supply the 6,918 piculs—1 picul equal 133⅓ lb.—exported from Singapore, from Jan. 1, 1845 to 1847, 69,180 trees were sacrificed; and, according to the *Sarawak Gazette*, 3,000,000 trees were required to supply the 90,000 piculs exported from this district during 1854 to 1874. These are only two instances, the first showing the trade in its infancy; and the second, that of a limited and comparatively small producing locality. In fact, the gutta-percha tree has only been saved from utter annihilation because trees under the age of twelve years do not repay the trouble of cutting down. Still, it is clear that the growth of young trees of the best varieties has not kept pace with the destruction, but are becoming much scarcer, so that recourse now, more than ever, has to be had to the products of very inferior varieties. At the present time there is a great difficulty in obtaining sufficient supplies of the best varieties, especially for telegraphic purposes. The Indian Government, acting on the advice of the late Mr. Howard, F.R.S., Mr. Markham, Dr. Spruce, and others, have taken up the india-rubber question. The Colonial Government should now take up the question of gutta-percha."

"Let Knowledge grow from more to more."—ALFRED TENNYSON.

Only a small proportion of Letters received can possibly be inserted. Correspondents must not be offended, therefore, should their letters not appear.

All Editorial communications should be addressed to the EDITOR OF KNOWLEDGE; *all Business communications to the* PUBLISHERS, *at the Office, 74, Great Queen-street, W.C.* IF THIS IS NOT ATTENDED TO DELAYS ARISE FOR WHICH THE EDITOR IS NOT RESPONSIBLE.

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NO COMMUNICATIONS ARE ANSWERED BY POST, EVEN THOUGH STAMPED AND DIRECTED ENVELOPE BE ENCLOSED.

IRELAND'S REAL ENEMIES.

[1126]—In communication 1113, page 90, allusion is doubtless made to me. Though the writer is correct in his statements (barring what he says about my "fortune") yet they may make your readers form too gloomy a picture of this sufficiently wretched country. I am not unpopular with my poor tenants in general, and they would not harm me, but I have incurred the enmity of certain rich and dishonest Land Leaguers by defeating them, and by my open opposition to illegal combination. I believe nothing but their cowardice prevents them from proceeding to extremities. But cowards are just the persons to take advantage of an opponent, say for instance when occupied in an observatory; hence it was considered prudent to abstain from star-gazing at present. If you wish it, you are quite at liberty to publish my name; it is declared war between the rebels and your humble servant,

EDWARD M. RICHARDS.

THE "NEW PRINCIPIA."

[1127]—Why will you so persistently continue to misunderstand and misrepresent my Astronomical opinions? You say "Mr. N. C. thinks (I fancy) the law is wrong; for he asks somewhere what keeps the velocities up." I beg leave to reply that I do *not* think Newton's first law of motion wrong; *i.e.*, if I rightly understand a law to be a certain manifestation of phenomena under certain conditions. What I distinctly disputed was Newton's explanation of the forces which *caused* the motions of the Universe, which explanation I considered inadequate and fallacious.

I have endeavoured to show in the "New Principia" that gravitation is not, and that polarity is, a full and sufficient elucidation of all the phenomena of the movements of the planets, &c.; that Newton's facts do not support his theories, and that the phenomena of motion, which no one questions, could not be sustained if his hypothesis of their cause were accepted as conclusive. I contend that "the velocities are kept up" by polarity—*i.e.*, magnetic attraction and repulsion.

I do not quarrel with Newton's *mathematical* demonstrations. I am not competent to do so, but I step behind them, and maintain that the data upon which he constructs his system are, to the last degree, untenable and unsatisfactory. If you will study my views you will find that they are not so shallow as you imagine them to be.

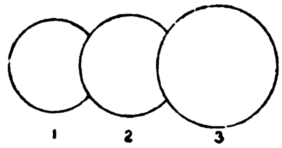
I must ask you to insert this communication as a concession to truth and justice. NEWTON CROSLAND.

[Mr. Crosland must not misapprehend the exceptions I have taken against the reasoning by which he conceives that Newtonian Astronomy is overthrown and the principle of polarity established. It is evident that Mr. Crosland fully believes in the strength of his position, and he is therefore quite right in asserting and re-asserting his belief that Newton's reasoning is invalid. But that his belief is altogether erroneous is so clear to every one who is competent to follow Newton's mathematical demonstration that it is impossible to speak otherwise of his views than as purely paradoxical. Every movement in the solar system, every change of velocity and corresponding change of centrifugal tendency accords *exactly* with the law of gravity. How those movements began no one is competent to say, but taking them as they exist at any moment, their future character and their observed past character for the whole time over

which astronomy extends its sway are in precise accordance with the theory that they are one and all governed by the law of universal attraction.—R. P.]

FALSE SUNS.

[1128]—I had hoped that some more competent observer than myself would have noticed and described the interesting phenomenon—mock suns I presume—which I and others of my family witnessed at sunset last Wednesday, February 6.



The subjoined diagram will serve to show the apparent positions and relative sizes of the three suns when first seen, at about twelve minutes to five o'clock, from a window which commands an almost uninterrupted view to the horizon. I suppose No. 3 was the true sun, though all were of about the same blood-red colour and equally well defined.

On the horizon, and very slightly overlapping the lower edges of the three disks, was a heavy bank of clouds, behind which they rapidly disappeared—it may have been about three minutes from the time of my first observing them; but No. 1 had then become very indistinct, Nos. 2 and 3 remaining well defined till the last.

About midway between the sun and zenith was another heavy stratum of cumuli, the intervening space being of a clear amber red, with some few light cirri floating about. I saw no halo. There was some *slight* mist in the distance. Thos. H. Amyot.

LETTERS RECEIVED.

Boyd Moss. The half-yearly volume will not be thicker than Vols. I. and II.—G. Clavering Mesnard. Quite agree with you that your solution of the Cribbage problem, (the same which two other correspondents have given) besides giving a larger total is more ship-shape than the other.—S. de C. Thompson. Your former letter marked for insertion. Thanks both for that and later letter.—J. Rowe. Know of none suitable for lectures. Oddly enough your suggestion already in part adopted, pictures actually engraved, but kept out for want of space.—D. C. Many thanks for your kindly and suggestive letter. Pecuniary considerations have to be taken carefully into account. The case is akin to that considered by Mr. Foster. Existence is essential even to the best intentions. Without going so far as dear old Johnson who said that "no one but a fool would write except for money," I note that no one but a fool would so conduct literary work as to impoverish himself and those for whom it is his duty to provide. A man of large means who gives time and labour gratuitously to help in instructing or benefiting the world deserves our approval and esteem; but one who should do the same, without sufficient means, and thereby eventually deprive himself of the power of doing the good he had intended, would be regarded as unwise to say the least of it.—Innocent Amateur. Rather hard to advise. My own experience does not help; for when I wanted such a telescope as you describe, I neither bought one of the cheaper sort, nor a smaller but more expensive instrument from a leading optician, such as Dollond, Cooke, or Wray,—but first bought a good object-glass, and three eyepieces,—then having noted the focal length of the O-G, I made my own tube, of card and black calico. Afterwards, but to my eventual regret, I had this light but strong tubing replaced by a wooden hexagonal one, the only satisfaction I derived from which arose from my having planned its proportions myself and also the arrangement for the slow movements. This instrument is, or was till lately, at Marlborough College.—A Lady Mathematician. The "we" who dealt so contumeliously with that bright luminary of yours, "were" the Editor; but by an odd coincidence the printer's devil *had* something to do with the matter, for he put that answer under the wrong heading. H. S. is *not* food for babes; but then "that fellow, F.," (as you politely call him) is not a babe, nor are "we." As your mathematical studies progress you will find it easy to see that at the time and in the latitude specified the light of Alpha Draconis would be reflected in the way we indicated. At present, it might be a useful exercise to try to prove the reverse. What you have to do is to show that the difference between the north polar distances of Alpha Centauri and Alpha Draconis was *not* at that time equal to twice the colatitude of the great pyramid. Lastly, you should note that the study of mathematics ought to have an effect rather soothing than the reverse. To illustrate this let me tell you an experience. I was once suffering torture as intense—about—as I could bear without fainting; I knew it would last several hours; and I was rather troubled at the thought; it occurred to me my beloved mathematics might help me. I was writing at the time my treatise on the geometry of cycloids, and had just reached that interesting part where the ratio of the arc of a prolate cycloid to an elliptical arc has to be geometrically demonstrated. It occurred to me that if I worked out a demonstration without the aid of pen, ink, and paper (which in my position at the time I could not use), I might bear my anguish better. The result was marvellous. After a few minutes during which the pain seemed increased, my mind went bodily off into the prolate-cycloidal country (bodily off because my body seemed forgotten). Before I returned thence I had completed mentally the solutions for about thirty pages of the sweet and light little treatise I have named. So I say of mathematics, "When pain and anguish wring the brow" (or even were it the big toe, for that matter), "a ministering angel thou." What comfort then ought not you to find in your department, which I assume to be "Lady Mathematics." Yet hitherto the case seems otherwise with you.—A Learner. Do not quite understand your difficulty,—that is why waves resulting from vibrations in the direction of wave motion should be harder to understand than waves resulting from vibrations transverse to that direction.—E. C. R. Doubt whether any other cause but relative dryness makes the action of the east wind so unpleasant.—R. Hilton. Under what conditions have you used your telescope? Do not rashly condemn it or dispose of it as inferior. The maker has a high and well deserved repute.—Polyglott. A page on patent law clearly showing what an inventor has to do and avoid, what to expect, what to hope, and what to fear, would, I think, be of great interest to our readers.—Jupiter. 1. Not if you properly protect your eyes. 2. No influence whatever has ever been detected. 3. Jupiter's moons would serve to make the theory of the tides very difficult for future inhabitants of that planet. Would prefer to leave the problem to them. 4. Mars has two moons; but if he had none the sun would make tides. And even if he made none, Mars might may be, do without them.—J. Murray.—M. B. Stidston.—P. D. T. Myers.—E. M. Faithful.—W. B. Graham.—J. Russell.—R. Phillips.—W. Wilkinson.—London Clay.—A. M. Chandiss.—H. Mackly.—Capstan.—J. P. T. Ashton.—Expectant.—T. H. Amgot.—Ein Deutche. I think you *are* too "youthful" to express such opinions.—G. E. Anti-Friction.—J. Pearce.—F.R.C.S.—G. T. Harrap.—Edwin Wooton. Should be glad to know what space such papers would require. The subject is important and interesting.—M. Bell. Many thanks.—Merres.—T. H. Amyot. Thanks.—W. Mackly. Thanks.—W. A. T.—M. T. H.—J. Macdonald. Thanks, you give us the kind of opinion we have sought to obtain. You are mistaken about the odd advertisement leaf; no binder of any experience would bind it in.—F. Loburtz.—J. W. Martin.—Senex.—J. Herbert. Thanks.—G. G. Hardringham. Thanks; suggestions noted.—J. P. T. Ashton. Messrs. Longmans. Title of Book, "Constellation Seasons."—F. Hawkey.—J. G. Smith.—S. Shrive.—E. Lee.—J. Farrar.—R. P. Anderson. Have no doubt your papers would prove most interesting; and think it likely the introduction of such matter would be most profitable for Knowledge. But it would not be fair to readers, who have a right to consider that the narrative element is not in our programme.—J. Farrar. Unfortunately cannot work out sums here.—Anti-Friction. The difference really depends not on the extent of surface in contact, but on the difference between rolling friction and sliding friction. Block the bearings, and the smallness of the surface in contact will be of no avail to reduce frictional resistance. Thanks for closing passage of letter.—London Clay. Do not think the B. A. Cat. can now be bought. My copy was borrowed. Almanack papers more suitable at longer intervals, as scattered short tables inconvenient. But, I say!—no star maps for a month? That's a large order. How long ago is it since the first and second numbers of February appeared? Surely not a month. It would not be fair just now to publish statistics on the point you mention.—C. Rousselet.—A. F. Science regards air as visible if in sufficient quantity.—A Reader. Thanks.—J. Harrison.—C. Southall. Thanks for your very kind and encouraging letter.—J. Barnes.—J. Rogers. We meant one only among last week's letters, up to the time of opening that particular letter. You do not make Nos. 2 and 3 who object to the change from 2d. to 3d.: more nearly fifty-two and fifty-three. But certainly thirty times as many have voted for as against the change, and the greater number of them have so written as to make us very gravely regret that the change was not made earlier, since they point out that Knowledge has had defects which the change alone could have corrected. The probability is as you say, that hundreds oppose the change, in fact that follows as almost a certainty from the process of dividing *all* our

readers into those for and against in the proportion indicated by those who have expressed an opinion. It follows as certainly that thousands are in favour of the change. The conclusion is that we may lose several hundreds of readers. But we have confidence that in the long run as many will be brought to us through the canvassing of those whose wishes we have considered in the matter. Some write, by the way, as if a change adding only one-fourth to the expenses for paper and printing, but one-third to the price, must necessarily involve a gain to the proprietors. They overlook the circumstance that the returns from advertisements (always an important part of the proceeds) are not affected at all by the change, or if affected might be expected to be at first somewhat diminished. The change will certainly at the outset involve considerable loss. But we believe it is absolutely essential to the adequate carrying out of the plan and purpose of the paper; and finding it approved by about twenty-nine-thirtieths of our readers, we feel it to be but just to make the change.—W. G. WOOLLCOMBE. Anent Tangent Galvanometer, see KNOWLEDGE No. 58. Will measure resistance of wire for you. Supposing a certain length of wire to offer 10 ohms, another specimen of *exacty similar* wire 25 times as long will offer approximately 250 ohms. Vols. II. and III. of KNOWLEDGE contain, it is thought, more information of the kind you require than any work.—EXPECTANT. See latter part of preceding answer. In the articles on electrical measurement and batteries (Vols. III. and IV.), calculations are made arithmetically, and algebra is systematically avoided. On this account the articles have, we hear, been used as the bases of lessons to unmathematical classes.—J. H. WOOD. Your remarks are for the most part fully answered elsewhere. You ask if KNOWLEDGE would be half the size of *Nature* (as it should) if with 32 pp. we had 14 or 15 of advertisements. There is no chance of our filling 14 pp. of a 32 pp. number with advertisements; but supposing it happened we could still stand favourable comparison with *Nature*. Take our number for February 22nd, then (remembering that when our price is raised to 3d. the readable matter will be correspondingly increased) the comparison with *Nature* for February 21 would run as follows:—In KNOWLEDGE there were 13½ pp. of matter, 9 original, 3 borrowed, 1½ correspondence; *Nature* should give at the same rate for 6d. 40½ pp. of matter, 27 original, 9 borrowed, 4½ correspondence; but the page of *Nature* contains only five-sixths of ours, so that of such smaller pages there should be about 48 of matter, 31 original, 12 borrowed, and 5 correspondence. As a matter of fact the number contains 24 pp. of matter, consisting of two original reviews (of "Languages of Africa," and the "Theory of Determinants") and other apparently original matter, in all 7 pp., 4½ pp. correspondence; the rest chiefly made up of notes, borrowed reports, the titles of articles in scientific serials, with occasional very short summary of contents; and reports of scientific societies and academies, in the extremest technical language. The provision made by *Nature* for each 2d. of its price is found to be 6⅔ pp. (of our size) as against 13½ in KNOWLEDGE, 2 pp. original matter as against 9 in KNOWLEDGE, and 4⅔ borrowed, correspondence, and reports as against almost exactly the same quantity of the same sort in KNOWLEDGE. Halving *Nature's* supply we have 10 pp. in all, of which 3½ are original, and 6½ borrowed, correspondence, and reports. We can safely promise never to give less than 20 pp. in all, of which at least 10 will always be original. Will not twice the quantity of total matter and thrice the quantity of original matter, satisfactorily answer your requirements?

Our Chess Column.

BY MEPHISTO.

IN order to gain additional experience, we have lately been engaged in some practical play, choosing Gambits whenever we had the first move. Our opponent—a reader of KNOWLEDGE, and blessed with a good memory—trumped us with our own cards, or rather variations, the result being a bad game in the opening. We then resolved to try the effect of simply varying the moves—mostly always a good expedient against inexperienced play. The result proved highly satisfactory, although it caused our opponent to remark on the artfulness of keeping the best variations up the sleeve.

The following is the game which follows the line of play in variation (*b*), p. 78:—

1. $\frac{\text{P to K4}}{\text{P to K4}}$ 2. $\frac{\text{P to KB4}}{\text{P takes P}}$ 3. $\frac{\text{Kt to KB3}}{\text{P to KKt4}}$ 4. $\frac{\text{B to B4}}{\text{B to Kt2}}$
5. $\frac{\text{P to B3}}{\text{P to Kt5}}$ (given as best) 6. $\frac{\text{Castles}}{\text{P takes Kt}}$ 7. $\frac{\text{Q takes P}}{\text{Kt to KR3}}$

8. Q to R5. This was the novelty. In the analysis we gave 8. P to Q4. The object of this move was to be able to take the Pawn with the Rook, and, if possible, to play R to R4 after P to Q4 had been played, thus bringing three pieces to bear upon the awkwardly placed Knight, as the following position will show:—

AMATEUR.

BLACK.

WHITE.

MEPHISTO.

Black here replied with 8. Q to B3, with the laudable intention of ousting the enterprising White Queen from her advanced position. We of course played 9. P to Q4, to which Black replied with 9. Q to Kt3, compelling 10. Q to R4. Here Black wanted to play too well; the Queen not wisely played 10. Q to Kt5, to which we were enabled to reply with 11. $\frac{\text{B takes P (ch)}}{\text{K takes B}}$ 12. $\frac{\text{R takes P (ch)}}{}$ with a won game.

Of course K takes B was a blunder, but in any case White had a fine game, as, per example, if instead of K takes B, 11. K to B sq. 12. $\frac{\text{Q takes Q}}{\text{Kt takes Q}}$ 13. $\frac{\text{B to Kt3.}}{}$ Now B to R3 would not be good on account of B takes P, nor Kt to B3, on account of P to K5. Black's best seems to be 13. $\frac{}{\text{P to Q3}}$ 14. $\frac{\text{R takes P (ch)}}{\text{K to K sq.}}$ 15. $\frac{\text{R to B7}}{\text{B to R3}}$ 16. $\frac{\text{Kt to R3}}{}$ with a good attack, for if now Black plays B to Q2 to save the Pawn and prevent Kt to Kt5, a likely continuation would be 17. $\frac{\text{P to KR3}}{\text{B takes B}}$ 18. $\frac{\text{R takes B}}{\text{Kt to KR3}}$ 19. $\frac{\text{R to Kt7}}{\text{K to B sq.}}$ 20. R to Kt5, and White must win by bringing his QR into play *viâ* KB sq. to B6, which Black could not prevent.

MR. R. A. PROCTOR'S COURSE OF LECTURES.

1. LIFE OF WORLDS.
2. THE SUN.
3. THE MOON.
4. THE PLANETS.
5. COMETS.
6. THE STAR DEPTHS.

See Advt. Pages for full Syllabus.

The following arrangements are complete: the numbers in brackets referring to above list.

TAUNTON (Victoria Rooms), March 5, 6 (1, 2).
SURBITON, Feb. 25, March 3.
BIRKENHEAD, March 10.
ALTRINCHAM, March 11 (5).
CHESTER, March 12, 13 (1, 2).
HAVERSTOCK-HILL, March 14 (2).
BLACKHEATH, March 17, 18.
REIGATE, March 19 (2).
HITCHIN, March 20 (5).
UXBRIDGE, March 21 (1).
LONDON (Brixton Hall) March 28, April 1, 4 (1, 2, 3).
„ (Memorial Hall), March 24, 27, 31, April 3 (1, 2, 3, 4).
BIRMINGHAM (Town Hall), April 18, 23, 25, 28; May 2 (1, 2, 3, 5, 6).
LEAMINGTON (Royal Music Hall), Four Afternoons, April 17, 19, 24, 26 (1, 3, 5, 6); Two Evenings, April 17, 24 (2, 4).
OXFORD (Town Hall), May 12, 13, 19, 20.

NOTE.—*All communications respecting Lectures should be addressed to Mr. John Stuart, Royal Concert Hall, St. Leonards.*

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CONTENTS OF No. 123.

HOW TO GET STRONG.

[The preceding parts of this series appeared in Nos. 32, 33, 35, 36, 41, 44, 49, 53, 58, 65, 66, 90, 91, 92, 94, 95, 97, and 104.]

AS the series of papers under this heading has been considerably interrupted, through increasing pressure on the Editor's available space, it seems desirable, before resuming it, to give a brief *résumé* of preceding portions, noting in what numbers they are to be found, and pointing out were lie the threads which we now intend to take up and write into the remaining parts of the series, so that when finished each part of our subject may be found to have been fully dealt with, however irregular the treatment of the series may seem to have hitherto been.

In No. 32, June 9, 1882, we pointed out how necessary it is that all men engaged in the struggle of life should maintain the vigour of their limbs and muscles and internal organs, but how brief is the time which busy men can usually afford for the purpose. But we showed that men do not need to be athletes to have their bodies in due working order, prepared to resist any such sudden strain as may at any time tax its energies, and find out a weak spot if any such there be.

In No. 33, for June 16, 1882, we began the detailed investigation of those exercises which are calculated to retain the vigour of particular muscles and organs. We started with the chest, dwelling on the value for chest development of sparring (not necessarily or even desirably with a living and breathing opponent), and of systematic inhalation and exhalation to the full extent of the lungs,—giving an illustrative instance of the great improvement in chest compass which such exercises can impart.

In No. 35, for June 23, 1882, we dwelt further on the importance of systematic exercise in inhalation and exhalation, and in answer to numerous correspondents, touched on the bad effects which any single form of exercise, however good in itself, must inevitably have on symmetry and general activity, if pursued without due admixture of other exercises. We touched there on a circumstance which any one who chooses to inquire into may readily test during the approaching season, the unsymmetrical figures of rowing men who are rowing men only, cricketers and lawn-tennis players who care for no other exercises, runners, tricyclists, and so forth who practise only their own favourite exercise, "only that and nothing more."

In No. 36, for July 7, we dwelt further on exercises for expanding the chest, dwelling in particular on rope climbing (for the young and light) and bell-ringing, or rather the kind of pull that is involved in bell-ringing. In No. 41 we added a form of exercise especially intended to expand and deepen the chest.

In No. 44, for Sept. 1, we turned our attention from the enlargement of the chest itself to the fortifying of the muscles outside the chest. We dwelt on the importance of distinguishing the matter of mere muscular development around the chest, a desirable result, from the essentially vital points,—1st the development of the frame enclosing the breathing organs, and 2ndly the development of these breathing organs themselves. We had rightly made the expansion and deepening of the chest itself and the training of the organs it contains to more complete fulfilment of their vitalising work, take precedence of the development of the pectoral and dorsal muscles. In fact, we pointed out that many exercises which increase these muscles and give to the chest (at least when examined by inexperienced eyes) an appearance of splendid development, positively injure the chest and act as a drain on the vitality instead of increasing it.

However, strength in the pectoral muscles is a good thing in itself, though not so necessary for men as it is for birds. We described several exercises which serve to strengthen them greatly, as (for the young and light) work on the parallel bars, work in thrusting downwards from the shoulders towards the knees, and slow work in slowly lowering dumb-bells held with the arms extended horizontally on either side and as slowly raising them again,—exercise requiring but a few seconds to do its work.

In No. 49, for October 6, 1882, still dealing with chest muscles but now considering the dorsal ones we dwelt on those exercises for the upper back and shoulders which are usually taken rather in excess than insufficiently in this country. Rowing in particular which is supposed by many to expand the chest and strengthen the upper arm muscles, in reality strengthens most the shoulder muscles, (next the leg muscles, and last the lower arm muscles,—not strengthening the upper arm muscles at all). It is well to notice this because it has been proved by measurement that if rowing is not corrected by other forms of exercise it absolutely tends to contract the chest.

In No. 53, for November 10, 1882, we gave an illustration of the ill-developed chest (especially as seen in the case of a rowing man (Fig. 1, p. 386, Vol. II.), and the symmetrical form given to the chest by ancient Greek sculptors working on the models supplied them in the gymnasium, where well-selected exercises gave well-proportioned figures to nearly every healthy Greek.

Next in No. 58, for December 8, 1882, we dealt with the muscles of the waist, wofully neglected in this country and therefore more liable to strain and accident generally than any muscles of the body. We showed how walking exercise should be taken, and what particular style of walk is good for weak waist muscles. In running also these muscles are exercised if the shoulders are well thrown back, the body upright, the strides long and energetic. We gave also some indoor exercises, requiring no apparatus, by which the waist muscles may be very efficiently strengthened.

In No. 65, for Jan. 26, 1883, we considered the mischievous effects of stays on the muscles of the waist, which in many cases lose all their strength and may be regarded as in part atrophied under constant corset pressure. In the

following number we dealt further with this important, one may say vital question, considering that though few men are so foolish as to ruin their abdominal muscles this way, all who are "born of women," which includes a considerable proportion of the human race, may inherit weakness thus occasioned.

In No. 90, for July 20, 1883, the series of papers on "How to Get Strong," which had been for a long time discontinued, was resumed. But we did not take up the subject where we had left it off. We had only dealt, thus far, with the chest and waist, and still had the important muscles of the arms and legs, besides the muscles of the hands, feet, and neck to deal with, before proceeding to consider the exercise of the body as a whole, and such questions as diet, regimen, training, and so forth. In No. 90, however, we commenced a long digression on the subject of reducing fat. This was continued in Nos. 91, 92, 94, and 95.

Lastly, in Nos. 97 and 104, we dealt with the question of such exercise as is suitable in advanced and middle life, a subject to which, as it is of extreme importance we shall refer later on, and also, *pro re natâ*, in treating of specific exercises for developing strength and lissomeness in particular muscles.

Here our survey of the past numbers ends. We resume the series at the point whence we digressed in No. 90. We had then dealt with the development of the muscles of the chest, (pectoral and dorsal), and waist. We have still some further remarks to make on the important and much neglected muscles of the waist and abdomen. Then we shall consider the muscles of the legs, before proceeding to those of the arms, because the muscular development of the legs has been more neglected than that of the arms; hundreds who by the use of dumb-bells, Indian clubs, and so forth strengthen their arms effectively enough, leave their legs with no exercise but walking and dancing, though at the very time that they are swinging clubs or bells they might be giving their lower extremities excellent and interesting occupation, making them at once stronger, more lissome, and more symmetrical. Besides which, owing to the circumstance that the legs have to support the weight of the body, exercises without apparatus are more easily devised for the legs than for the arms.

(To be continued.)

GAMBLING SUPERSTITIONS.

By Richard A. Proctor.

(Continued from page 108.)

But we may learn a further lesson from our illustrative tossers. We have seen that granted only a sufficient number of trials, runs of luck are practically certain to occur; but we may also infer that no run of luck can be *trusted* to continue. The very principle which has led us to the conclusion that several of our tossers would throw twenty "heads" successively, leads also to the conclusion that one who has tossed "heads" twelve or thirteen times, or any other considerable number of times in succession, is not more (or less) likely to toss a "head" on the next trial than at the beginning. *About half*, we said, in discussing the fortunes of the tossers, would toss "head" at the next trial—in other words, *about half* would fail to toss "head." The chances for and against these lucky tossers are equal at the next trial, precisely as the chances for and against the least lucky of the ten million tossers would be equal at any single tossing.

Yet, it may be urged, experience shows that luck continues; for many have won by following the lead of lucky players. Now I might, at the outset, point out that this belief in the continuance of luck is suggested by an idea directly contradictory to that on which is based the theory of the maturity of the chances. If the oftener an event has occurred, the more unlikely is its occurrence at the next trial—the common belief—then contrary to the common belief, the oftener a player has won (that is, the longer has been his run of luck), the more unlikely is he to win at the next venture. We cannot separate the two theories, and assume that the theory of the maturity of the chances relates to the play, and the theory of runs of luck to the player. The success of the player at any trial is as distinctly an event—a chance event—as the turning up of ace or deuce at the cast of a die.

What then are we to say of the experience of those who have won money by following a lucky player? Let us revert to our coin-tossers. Let us suppose that the progress of the venture in a given county is made known to a set of betting men in that county; and that when it becomes known that a person has tossed "head" twelve times running, the betting men hasten to back the luck of that person. Further, suppose this to happen in every county in England. Now we have seen that these persons are no more likely to toss a thirteenth "head," than they are to fail. About half will succeed, and about half will fail. Thus about half their backers will win and about half will lose. But the successes of the winners will be widely announced; while the mischances of the losers will be concealed. This will happen—the like notoriously does happen—for two reasons. First, gamblers pay little attention to the misfortunes of their fellows: the professed gambler is utterly selfish, and moreover he hates the sight of misfortune, because it unpleasantly reminds him of his own risks. Secondly, losing gamblers do not like their losses to be noised abroad; they object to having their luck suspected by others, and they are even disposed to blind themselves to their own ill-fortune as far as possible. Thus, the inevitable success of about one half of our coin-tossers would be accompanied inevitably by the success of those who "backed their luck," and the successes of such backers would be bruited abroad and be quoted as examples; while the failure of those who had backed the other half (whose luck was about to fail them), would be comparatively unnoticed. Unquestionably the like holds in the case of public gambling-tables. If any doubt this, let them inquire what has been heard of those who continued to back Garcia and other "bank-breakers." We know that Garcia and the rest of these lucky gamblers have been ruined; they had risen too high and were followed too constantly for their fall to remain unnoticed. But what has been heard of those unfortunates who backed Garcia after his last successful venture, and before the change in his luck had been made manifest? We hear nothing of them, though a thousand stories are told of those who made money while Garcia and the rest were "in luck."

In passing, we may add to these considerations the circumstance that it is the interest of gaming-bankers to conceal the misfortunes of the unlucky, and to announce and exaggerate the success of the fortunate.

I by no means question, be it understood, the possibility that money may be gained quite safely by gambling. Granting, first, odds such as the "banks" have in their favour; secondly, a sufficient capital to prevent premature collapse; and thirdly, a sufficient number of customers, success is absolutely certain in the long run. The capital of the gambling public doubtless exceeds collectively the capital of the gambling-banks; but it is not used collec-

tively: the fortunes of the gambling public are devoured successively; the sticks which would be irresistible when combined are broken one by one. We leave our readers to judge whether this circumstance should encourage gambling or the reverse.

It is also easy to understand why in the betting on horse-racing in this country and others, success ordinarily attends the professional bettor, rather than the amateur,—or, in the slang of the subject, why "the ring" gets the advantage of "the gentlemen?" Apart from his access to secret sources of information, the professional bettor nearly always "lays the odds," that is, bets against individual horses; while the amateur "takes the odds," or backs the horse he fancies. Now, if the odds represented the strict value of the horse's chance, it would be as safe in the long run to "take" as to "lay" the odds. But no professional bettor lays fair odds save by mistake. Nor is it difficult to get the amateur to take unfair odds. For "backing" is seemingly a safe course. The "backer" risks a small sum to gain a large one, and if the fair large sum is a little reduced, he still conceives that he is not risking much. Yet (to take an example), if the true odds are nine to one against a horse, and the amateur sportsman consents to take eight to one in hundreds, then, though he risks but a single hundred against the chance of winning eight, he has been as truly swindled out of ten pounds as though his pocket had been picked of that sum. This is easily shown. The total sum staked is nine hundred pounds, and at the odds of nine to one, the stakes should have been respectively ninety pounds and eight hundred and ten pounds. Our amateur should, therefore, only have risked ninety pounds for his fair chance of the total sum staked. But he has been persuaded to risk one hundred pounds for that chance. He has therefore been swindled out of ten pounds. And in the long run, if he laid several hundreds of wagers to the same amount, and on the same plan, he would inevitably lose on the average about ten pounds per venture.

In conclusion, I may thus present the position of the gambler who is not ready to secure Fortune as his ally by trickery:—If he meets gamblers who are not equally honest, he is not trying his luck against theirs, but, at the best (as De Morgan puts it) only a part of his against more than the whole of theirs: if he meets players as honest as himself he must, nevertheless, as Lord Holland said to Selwyn, "be—in earnest and without irony—*en verité le serviteur très humble des évènements*, in truth the very humble servant of events."

THE EVOLUTION OF FLOWERS.

By GRANT ALLEN.

III.—INTEGRATION BEGINS.

BESIDES the true Alismas with which we have hitherto dealt, there are a few other Alisma-like plants in Britain, each of which helps us on a little way toward the development of the true lilies; and as it is better, where possible, not to travel beyond the limits of fairly well-known or easily obtainable flowers, we may as well take these English species as the representatives of the various intermediate stages.

In a very few spots among the South-eastern counties there grows a rare water-side weed of wet ditches or pools, known to botanists as *Damasonium stellatum* or *Actinocarpus stellatus*. It is a south European plant, which only just reaches our shores where they lie nearest to the Continent, and has never been able to spread itself further north or west, against the adverse climate and the stout competition of our more northerly waterside weeds. Damasonium at first sight presents a great many points of resemblance to the water-plantain; it has three small green sepals, three much larger white petals, six loose stamens, and a group of carpels in the bossy centre. Being fertilised by the same sort of flies as water-plantain, it has even the yellow spot at the base of the petals as in Alisma, which marks the way to the honey, and points back to the original yellowness of the whole petal. But when we come to look more closely into the flower, we see that it possesses two distinct symptoms of advance in organisation, each of which is very important as leading onward and upward in the direction of the true lilies. In the water-plantain we saw that the carpels were very numerous, sometimes as many as thirty in a single flower: but in Damasonium they are always six in number, that is to say, they are reduced to two whorls of three carpels each. The significance of this change is best seen if we put together four typical groups in ascending order of evolution, thus—

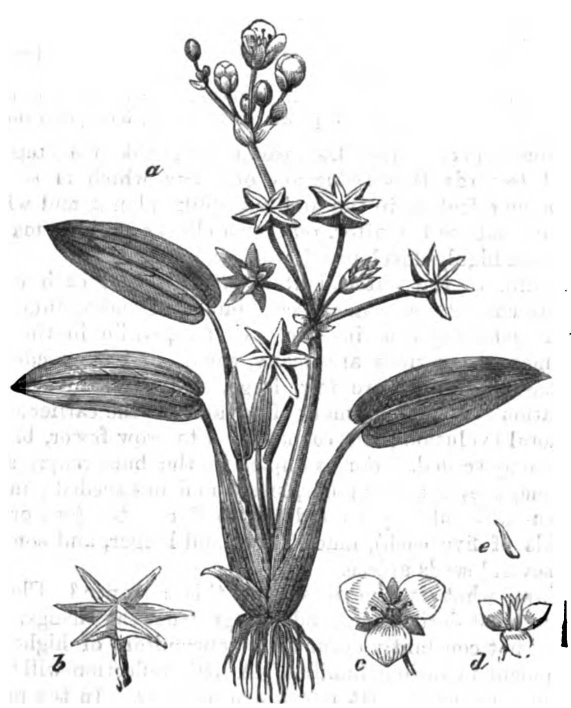
Water-plantain...3 sepals; 3 petals; 6 stamens (=twice 3); many carpels.
Damasonium......3 sepals; 3 petals; 6 stamens; 6 carpels (=twice 3).
Lily (tulip) ......3 sepals; 3 petals; 6 stamens; 3 carpels (united).
Iris..................3 sepals; 3 petals; 3 stamens; 3 carpels (united).

Thus it is clear that Damasonium has taken a step forward towards that reduction of parts which is so conspicuous a feature in the higher lily-like plants, and which, as we shall see hereafter, reaches a climax in those marvellous and highly-developed flowers, the orchids.

Again, in the water-plantain, we saw that each carpel contained only a single seed; but in Damasonium each carpel generally contains two. In fact, while in the true Alismas the carpels are many, small, and one-seeded, in Damasonium they are few, large, and two-seeded. This variation is a common mark of advance in the earlier stages of floral evolution; the carpels tend to grow fewer, bigger, and many-seeded. For example, in the buttercups, as in Alisma, they are numerous, small, and one-seeded; in the marsh-marigold they are reduced to five or ten (one or two whorls of five each), much larger and longer, and containing several seeds apiece.

Now, what is the reason of this advance? Clearly, plants must derive some advantage from the change, or it would not constantly occur as a concomitant of higher development in various families. A little reflection will serve to show us what that advantage really is. In the primitive plants with one-seeded carpels, each seed has to be separately fertilised by a distinct act of impregnation; for every stigma on to which an insect brushes pollen only one seed in the end gets set. This, of course, necessitates the production of a large number of carpels, and compels the plant to ensure, as far as possible, the separate impregnation of every one among them. Hence it becomes an advantage for the plant to produce fewer and larger carpels, each containing two or more ovules; because, in that case, each single act of impregnation suffices to fertilise two or more seeds. Accordingly, we find that as plants rise in the scale of evolution, they usually at first lessen the number of their separate carpels, while increasing the number of seeds in each. Later on, as we shall observe hereafter, the number of seeds also begins to decrease; but that is only when, by improved methods of fertilisation, protection, and dispersion, and increased richness of the seeds themselves, the plant is able to dispense with the necessity for producing an immense number of seeds from each flower. Efficiency, in such cases, serves the species in place of quantity.

Another interesting peculiarity of Damasonium consists in the fact that all six of its carpels are not quite free and separate from one another throughout all its life, as is the case with those of the Alismas and the buttercups: they are joined together at the bottom on the inner side to the stem or axis of the flower. This is the first beginning of that tendency which finally produces the compact, three-celled ovary of the lilies, where the three carpels have coalesced altogether, though their original distinctness is still marked by the walls of separation which divide the three cells from one another. Among the buttercup group, the monkshood is in this respect the exact analogue of Damasonium, for its three carpels are also usually joined together slightly at the base. If the lilies, however, had been developed from an ancestor at precisely the same stage as Damasonium, they would, of course, have had six cells to the ovary, instead of three, as is actually the case. The carpels of Damasonium taper to a sharp point, and are arranged radially like a conventional star, whence it gets its second name—*stellatum*, or starlike, and its other title of Actino-

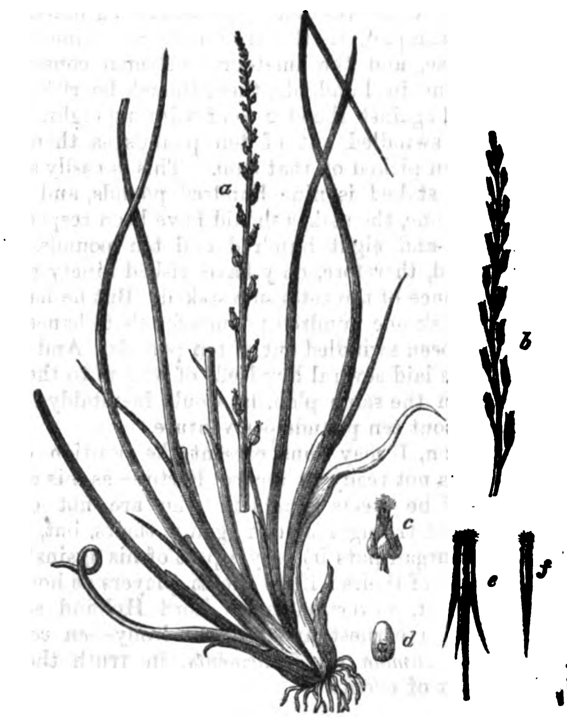


Damasonium stellatum.

carpus, or "ray-fruit." I believe this arrangement of the carpels has reference chiefly to the fertilisation, so as to allow the insect's head to touch each stigma in succession easily and certainly; but it also doubtless aids in dispersing the seed equally in all directions.

It is very seldom that we can trace the marks of evolution continuously along a single line. We must rather pick out here and there separate indications of its general tendency, each surviving step or link being oftener a mere analogy (or independent similar development) than an actual survival of the various stages in the pedigree of the higher kinds. This is very strikingly seen in the Alisma-like plants, where several species preserve different levels of development, not in all parts alike, but one part in one species and one part in another. For example, take our two British arrowgrasses (*Triglochin maritimum* and *T. palustre*). In one respect, both these plants approach closer to the lily type than even Damasonium, for they have their carpels united during the flowering stage around the axis, though they separate from it into distinct pieces when ripe. Yet in another respect they are still as primitive as Alisma, because they contain only one seed in each carpel. Let us look for a moment at these two curious and common, but inconspicuous, little plants.

The arrowgrasses are degenerate small Alisma-like weeds, growing among tall marsh plants and grasses, and compelled by their habitat to decline into the practice of wind-fertilisation. Hence, like all other wind-fertilised plants, they have no bright-coloured petals. All six perianth pieces (that is to say, sepals and petals alike) are simply green, and they are very small or almost scale-like. Colour is not here needed to attract insects, and so the plant dispenses with it altogether. There are six stamens, hanging rather looser than in Alisma, so as to shed the pollen to the wind, though much confined by the scales of the perianth; and the carpels have each a feathery stigma, protruding from the flower, so as easily to catch the pollen dropped by the stamens. This feathery state of the stigma is very common among wind-fertilised plants, as it exactly suits their habit



Triglochin palustre.

of life. In fact, the arrowgrasses are Alismas which by degeneration have very nearly reached the same state as the true grasses; only, these latter are degenerate lilies, starting from a higher level in the evolutionary order. The head or spike of flowers in arrowgrass reminds one very closely of the common plantains (*Plantago*), which are degraded relatives of the veronicas, with closely similar habits.

Our two kinds of arrowgrass differ between themselves in the matter of carpels. The marine kind (*Triglochin maritimum*), which grows in salt marshes, is clearly the more primitive in type of the pair, for it has six carpels, like Damasonium, reduction having gone here only so far as to leave two whorls. In the flower there is thus a six-celled ovary, in the ripe fruit the cells divide as six distinct carpels. The fresh-water species (*T. palustre*), common everywhere on the border of streams, has only three, the reduction here having left but a single whorl. Thus this species, when in flower, closely resembles the true lilies, and is only distinguished from them when in fruit by the

fact that its carpels separate from one another as they ripen, so bearing witness to their primitive distinctness in the original ancestor. Still, a great many things conspire to show us that this Triglochin is not really a predecessor of the true lilies, but only an analogue, that is to say, a plant which has independently developed to some extent in the same direction. The Flowering Rush (*Butomus*), which we shall examine hereafter, though less like the lilies in its ovary, stands really nearer to them in genealogical order, as we shall see by-and-bye; but I must defer the consideration of that plant, as well as of the pretty arrowhead, to our next paper. We shall then have finished with the British representatives of these earliest Alisma-like monocotyledons.

AMONG THE INDIANS OF GUIANA.

(SECOND NOTICE.)

BY EDWARD CLODD.

NOT only do the physical characteristics and languages of the Indians of Guiana mark them as members of the Red Race, but their family system also, this being identical with the totem system of the North American Indians, which was explained in a recent number of this journal.* In the origin of the names of a number of families, of which Mr. Im Thurn gives a list, the belief in descent from the eponymous animal, bird, or plant is evidenced. Their marriage system is exogamous, *i.e.* the women are taken from other tribes, unions within the tribe being, except at missions, a rare event. The practice of reckoning descent through the mother prevails, and intermarriage with relations on her side is forbidden.

The ancient custom described by Herodotus, and familiar to us through the discoveries in the Swiss lakes and elsewhere, of living in houses raised upon piles, prevails among some of the Guiana Indians. Like the early Neolithic immigrants, they are not only hunters and fishers, but tillers of the ground, the harder fieldwork falling to women in the cultivation of the cassava plant, from the root of which their bread is made. The Indian has his spasms of work and long spells of rest, nor, where wants are simple and, on the whole, easily supplied, need he act otherwise.

> To be, contents his natural desire,
> He asks no angel's wing, no seraph's fire.

And in reading the account of his daily life one is inclined to agree with Professor Huxley that it is preferable to the squalid and sunless existence of the unpitied "wastrels" in the slums of great cities.

The curious and, to us, unmeaning custom of "la couvade" prevails in Guiana. When a birth happens, the father has to take to his hammock, and is tended just as if *he* had suffered the pains of child-birth. He has to abstain from food for a time, and for a still longer time from killing and eating large animals, lest harm come thereby to the child. The Rev. Mr. Brett, in his Travels in Guiana, describes how the "man in robust health and excellent condition, without a single ailment, was lying in his hammock in the most provoking manner, carefully and respectfully attended by women, while the mother of the new-born infant was cooking, none apparently regarding her." This odd custom probably has its origin in that barbaric feeling of a common sympathy between man and everything else which is the offspring of belief in their connection, and with which is therefore related the fear of harm through bad spirits indwelling in animals. Only such crude philosophy can seemingly explain an otherwise unintelligible practice which, if it were local, we should dismiss as eccentric, but which is traced from east to west, and the tradition of which, as Mr. Black points out in his work on "Folk Medicine,"* survives among the Basques, as also in France, and in Ireland. "The husband does not, indeed pretend to suffer the pains of labour, but the nurses boast that they possess the power of transferring the sufferings to him or to any other person they please. In earlier times she brought the pretended pains, for her appearance was tantamount to a declaration that his confinement and restricted living must commence. Now, the nurse threatens a real transfer, and not understanding why the husband should be the only sufferer, she boasts of being able to give the pains to any man, particularly, my informant says, to old bachelors."

The father is supreme in the family; but, in the settlement, authority rests with the man who has proved himself the ablest warrior or hunter, he, in his turn, standing in awe of the peaiman or medicine-man, minister to ills both of body and mind. Mr. Im Thurn gives an amusing account of the treatment to which he submitted himself at the hands of the peaiman when suffering from headache and fever. Space does not permit us to extract it, and it must suffice to say that one of the remedies consisted in indescribable yells and growls, which the peaiman kept up for six hours without ceasing! But the treatment, being external only, bears comparison with that adopted by a medicine-woman among the Piutes in the case of a patient suffering from an attack of grizzly bear, her compound consisting of mixed weeds, chewing-tobacco, the heads of four rattlesnakes, seasoned with red-pepper, and diluted with crude petroleum, one pint to be taken every half-hour. It is needless to add that, although a brave warrior and noted for fortitude under suffering, after taking the first dose, "the subsequent proceedings interested him no more."†

But the peaiman wars not so much against flesh and blood as against unseen principalities and powers, his chief function being as antidote to the kenaima, or slayer. The real kenaima is one who carries out the vendetta, or law of revenge; the imaginary and more dreaded kenaima is believed to exercise the power of separating his body and spirit, which latter, whether it enters into beast, bird, or insect, is still controlled by its owner, at whose door all diseases and death are laid. The peaiman's duties are, however, best understood in connection with the religion of the Guiana Indians. Concerning this, Mr. Im Thurn has gathered, without bias, valuable materials in support of the modern doctrine that "animism," or a belief in the existence of spirits as distinct, but not necessarily separate, from bodies, whether animate or inanimate, is the universal and earliest form of religion, and the germ from which the higher faiths of mankind have developed.

Here is a people, with civilisation on their seaboard, who have made scarce any advance beyond that primitive thought which draws little or no distinction between living and lifeless things, which credits not only swirling rapids and waving branches, but motionless rocks and stones, with indwelling spirits who can leave their abodes at will and pass into things intangible as the air or substantial as the mountain, on mission ever baneful. In the phenomena of dreams and visions, of sleep and death, we find here, as at like levels of culture, the sources of that belief in spirits anywhere and everywhere. Mr. Im Thurn relates how the vividness of their conviction of the actual occur-

* See KNOWLEDGE, Sept. 7, 1883, pp. 146-7.

* Page 179. (Published by the Folk-Lore Society.)

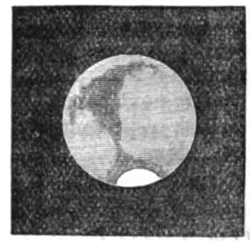
† *Vide* "Dorman's Primitive Superstitions," p. 359.

rence of the things dreamed caused the Indians of his party to come perilously near doing mischief to their supposed foes. And as with the dreams, so with the apparitions which appear to them in waking moments under abnormal states of the body. The identity between man and brute, which is established on these lines, is not so easy to follow in the case of inanimate objects which are without motion, but the barbaric mind draws no fine distinctions. The injuries caused by falling over rocks, or *vice versâ*, are referred to the spite and evil designs of spirits dwelling in them, and awe of them is quickened if the rock or erratic boulder bears any mysterious carvings or wears the shape of man or beast or monster. Beyond this general animism the Guiana Indians do not appear to have risen; the after-existence of the spirit is taken for granted, but only whilst the memory of the dead survives can their continued existence be assumed. The brain which gets puzzled at any attempt to count beyond the fingers can hardly frame conceptions of endless duration, or of any everlasting abode, whether of happiness or misery. The heaven of the Indian is but another country in some other part of the world, beyond the sea which he cannot cross, but whence the white man comes; or above the solid blue which he cannot pierce, whence descended the ancestors, and whither go the birds and the spirits of the wonder-working peaiman. In that spirit-realm no member differs from another in glory—they meet in "equal sky," as Pope describes it. Hence we find no approach towards polytheism, even at its lowest; still less towards any alternating supremacy of gods, or one almighty deity. His attitude towards spirits is to keep on guard, or call in the peaiman, against such as may harm him; as to the rest, since they are on his side, he need not trouble about them. He prays to no being whatsoever; rites and ceremonies, full moons and sabbaths exercise him not; he is only careful to abstain from meats forbidden or untested, lest evil befall him and his thereby. In brief, animism, as it exists amongst the Indians of Guiana is, as Mr. Im Thurn remarks, "much more primitive than has yet been suspected by most students of religious evolution." In the final chapters an account is given of the folk-lore and culture-myths of the tribes; and of the various antiquities, amongst these the pictured rocks of unknown origin, and the shell-mounds corresponding to the "kitchen middens" of the Baltic. But enough has been said about this charming book to indicate what a banquet the author has spread for the enjoyment both of the naturalist and the student of culture. They will find in the *menus* which the classified indices furnish, ample details of the good cheer, and in the illustrations useful ornaments to the text.

MARS IN A THREE-INCH TELESCOPE.

BY A FELLOW OF THE ROYAL ASTRONOMICAL SOCIETY.

THE study of Areography (Greek Ἄρης Mars), or minute Martial detail, can only be properly carried on by the aid of a telescope of considerable size and power. Notwithstanding this, we shall find that a good deal that is curious and instructive in connection with the physical structure of this planet is well within the capabilities of the instrument we are using; and we propose to-night to examine such features of his surface as are susceptible of exhibition with only three inches of aperture. A haze of cirro-strati is drifting over the sky as we turn our telescope on to Mars; but, as we observed on page 101, in connection with Jupiter, this really has the effect of subduing the general glare of the planet, sharpening such detail as is perceptible, and improving definition generally. Compared with the mighty orb of Jupiter, Mars presents such a small disc that we find it necessary rather to overpress magnifying power than otherwise, to see fairly the principal markings he exhibits. We then arm our instrument with a power of 204, and, on directing it to the planet, behold the spectacle depicted in the subjoined engraving.



Mars, Feb. 18, 1884, 9h. 35m. G.M.T., Power 204.

Unlike Jupiter and Saturn, the disc of Mars—in its present phase—appears circular. By-and-bye, at the beginning of May, when Mars is in so-called "Quadrature" with the sun, he will be very perceptibly gibbous (Fig. I., p. 86), but under no circumstances has any trustworthy measurement shown a sensible excess of his equatorial over his polar diameter. Dawes found the ellipticity absolutely imperceptible. Regarding then this circular disc, what do we see? A bright white patch at the bottom—or north pole—of the planet is probably the first thing that will arrest the attention of the observer, contrasting as it does markedly with the general orange tint of the planet. Bounded on one side by the limb, this brilliant marking exhibits the form of a double convex lens. It is entirely surrounded on its southern, or upper edge, by a greenish grey dark marking, from which rises another of a shape akin to that of the old-fashioned champagne-glass of the days of our fathers and grandfathers. Instead, however, of terminating in a symmetrical rim, the southern extremity of this bifurcates in a fashion which somewhat suggests the spreading of the wings of a sea-gull. The central southern portion of this is the darkest part of it. Our sketch was made, as stated above, at 9h. 35m. at night. Had we deferred it until four o'clock the next morning, the planet would have presented a very different aspect indeed; for Mars rotates on his axis in 24h. 37m. 22·735s. (thus taking a little more than half an hour longer than the earth to complete one rotation), and from this cause, of course, fresh portions of his surface are presented to our view, as the night advances, just as in the case of Jupiter. There is, however, this very notable difference between the markings on the two planets, that whereas, as stated on p. 101, the detail on Jupiter is in no legitimate sense permanent, existing as it probably does in a vaporous and very mobile envelope of enormous extent, in Mars the markings are persistent, and certainly form parts of his actual solid (and liquid) surface; so that maps of no inconsiderable accuracy have been formed of them. What, then, do they signify? It seems in a high degree probable that, in looking at Mars, we are regarding a miniature of our own world. That the general surface of the planet may well represent land of a geological structure allied to the "Triassic," or New Red Sandstone Rocks so well displayed at Exmouth, Dawlish, and Teignmouth in this country; that the darker markings are nothing but oceans, seas, straits, and lakes; while the conspicuous white patch at the pole has its origin in the existence there of the huge tracts of glacier and snow-covered land and ice-locked sea of the Martial Arctic regions. For, during the present

opposition of Mars, the north pole of the planet is turned towards the earth (Fig. 5, p. 72), and it will be seen, from the position of the planet's axis with reference to us, that his south pole is wholly hidden, from being on the other side of him. The reason for this is worthy of a brief examination. Mars goes round the sun in 686·98 days; the earth describing her orbit in 365·26 days; so that being in opposition, at any given date, an interval of 779·84 days must elapse ere they return to it. At least, this is the mean period between two successive oppositions; but owing to the great eccentricity of Mars' orbit, this varies as oppositions occur near his perihelion or aphelion (points of his nearest approach to and greatest recession from the sun) respectively. Moreover, the equator of Mars is inclined some 27° to the plane of his orbit. As the inclination of our own equator is only 23½° to the ecliptic, it is evident that the vicissitudes of the Martial seasons must be more aggravated than in our own case. But we have spoken of this tilt of the planet's axis mainly for the purpose of pointing out that while at certain oppositions, the north pole of the planet must be turned towards us, at others we must see his south pole; while at intermediate points—answering to our terrestrial equinoxes—we must see both poles, just as in the circular maps of the earth which form the frontispieces to so many books on geography. The south pole of the planet, as we have said above, is wholly invisible now; but, during the memorable opposition of 1877, a great white lenticular-shaped patch on the southern limb of Mars formed just as conspicuous a feature as the corresponding north polar one (then invisible) does now. From all this it will be evident that to get a true idea of areographical detail, we must watch, and carefully draw, Mars during several oppositions. It is very remarkable to note what a curious change in the aspect of any given feature, far north or south of the equator of Mars, is produced by foreshortening. Nothing but long-continued observation and abiding faith in the irrefragable principles of perspective will enable the student to identify a given spot or marking, when viewed under so very different an aspect. The best popular map extant of the leading Martial details is the one given by Prebendary Webb on p. 146 of his altogether admirable book, "Celestial Objects for Common Telescopes." It is, however, drawn on Mercator's projection, with its grossly-exaggerated polar dimensions; and the young observer must make allowance for this in using it for regions removed by any considerable distance from the equator. The great inverted conical marking shown in our sketch above is chiefly composed of the Kaiser Sea. The thin arm stretching out to the left is a part of Flammarion Sea; that to the right a portion of the Dawes' Ocean. The Delambre Sea (confused by foreshortening with Nasmyth inlet to the right), forms the dark marking surrounding the snow cap at the north pole (bottom) of the planet. Situated some 150° in longitude from the Kaiser Sea is a very notable marking in the southern hemisphere of Mars—unfortunately in an unfavourable position for the observer just now—which is called Terby Sea, and which, surrounded by Kepler land, presents a somewhat ludicrous resemblance to an eye. In the absence, however, of a map, detailed descriptions of particular markings become merely nugatory.

It only remains in conclusion to mention a few facts in connection with the general aspect of the planet. We have spoken of the very conspicuous white patches at, or near, the poles of Mars; patches occasionally so brilliant that irradiation causes them to appear as positively projecting slightly beyond the outline of his limb. That these, as we have previously intimated, consist of ice, or snow, or both, the evidence afforded by the spectroscope seems to render practically certain, showing, as it does, the presence of large quantities of aqueous vapour in the Martial atmosphere. There is, however, a somewhat similar appearance, which we have, among others, ourselves observed, that of lenticular-shaped white markings round the limb, which are by no means so easily explicable. A very little attention will show the observer with the telescope that the ruddy tint of the planet's face is most marked towards the centre of the disc, and that the limb is notably paler (occasionally so much so as to seem nearly white, as was the case in the autumn of 1879); but the markings of which we are now speaking are large white patches in the eastern and western limbs of Mars, for which it is anything but easy to account, though they are probably atmospheric. An extremely ingenious explanation of the general brightness of the limb will be found on page 65 of the "Essays on Astronomy," by the Editor of Knowledge. Mars has been described as a miniature of our own earth; and undoubtedly he does present features connecting his physical structure more closely with ours than any other planet; but still, irrespectively of size, there are differences which cannot fail to strike the thoughtful observer. The most salient of these is the difference of distribution of land and water. On the earth only about 51,500,000 square miles consist of dry land, while 145,500,000 square miles are covered by water. On Mars the land so far preponderates that the largest oceans, or rather seas, can only be described as more or less land-locked. Schiaparelli claims to have discovered a strange network of "canals," uniting various portions of the seas of the planet hitherto considered to be isolated; but, admitting, for argument's sake, the objective reality of these features, they are hopelessly beyond the optical power we are employing. We may mention, in conclusion, that a curious collateral indication of the existence of clouds, or vapour in some form, has been observed, in the shape of the dimming, or partial obscuration, of spots and markings on the surface of Mars; while others, at no great distance, have simultaneously retained all their usual sharpness and comparative precision of outline.

PLEASANT HOURS WITH THE MICROSCOPE.

By Henry J. Slack, F.G.S., F.R.M.S.

In the water mentioned in the last paper was a creature exhibiting to a wonderful extent the shape-changing power which those who like learned-looking words may call metabolism. Many infusoria besides the amœbæ can assume very different forms, and the common rotifer, which stands much above them in organisation, has an integument so elastic that it permits the creature to elongate itself like a worm, swell out like a pear, or become almost as round as a plum-pudding. The amœbæ have no permanent integument. Their outside readily becomes their inside, or *vice versâ*, but the layer which happens to be outermost and in contact with water appears less mobile than other layers which happen to be inside, and with their molecules surrounded on all sides by similar molecules. The little animal we have now before us has a very definite shape in its quiet state, and lives in a skin of considerable consistency and toughness. It is called *Astasia trichophora*. The first word indicates its belonging to a genus of form-changers, the *Astasiadæ*, and the second refers to its long whip, but not very accurately, as it is not at all hairlike. Fig. 1 is copied from Kent's valuable work representing

the little animal magnified 600 times linear. When quiet and stretched out it is an elongated somewhat pear-shaped bag, containing a multitude of granules, and having for its locomotive organ a very long whip, pretty thick at the base and thinning out gradually towards the tip, which is very fine. The letters *p h* indicate a mouth, and gullet, or pharynx, and *n* the nucleus and nucleolus, or, as Huxley calls these things, the *endoplast* and *endoplastule*. Balbiani thought these organs—which are found throughout the infusoria—represented male and female elements; but later observers differ from him, although it is admitted that they are

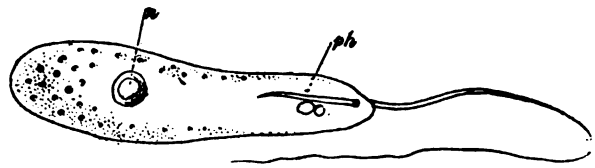


Fig. 1.

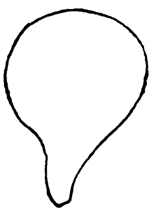


Fig. 2.

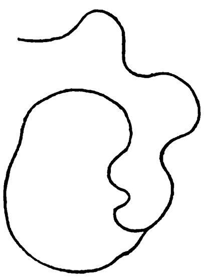


Fig. 3.

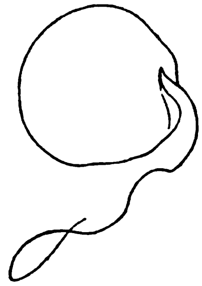


Fig. 4.

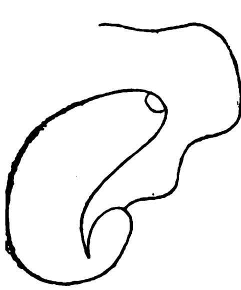


Fig. 5.

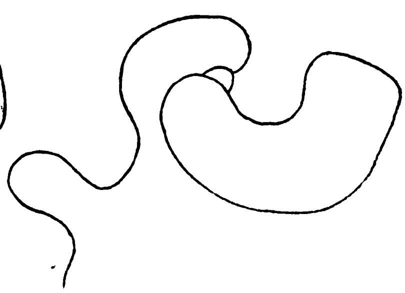


Fig. 6.

Asbasia Trichophora—Fig. 1 from Kent's copy of Bütschli, × 600. Figs. 2–6 outlines of changing forms as seen by the writer.

concerned in generative processes. The Astasia family is in many respects like the Euglenæ, but without the rich colour and the eye spot common to the latter. Their sarcode, or flesh, if so we may term it, is colourless, and their mouths larger and more stretchable than the tiny orifices of the Euglenæ. Both families are wonderful form-changers, but the species before us is, perhaps, the most astonishing performer of this sort. Not for two seconds was the one under notice either quiet or of one shape, and this double activity of locomotion and figure-alteration went on for several hours, and might have lasted longer if the water-drop had not dried up. Figs. 2–6 show in outline some of the strangest forms it assumed. In No. 2 the whip is not seen. It was all curled up on the other side. This whip exhibits, very strikingly, a thoroughly vital character. Every bit of it is as alive as a monkey's tail, and in this it differs entirely from the cilia common to so many infusorians, and which behave like elastic rods, and are moved from their bases, as a switch may be moved by twists of the wrist. It would be quite impossible even for a Japanese juggler to imitate many of the movements of this whip by hand-play with an elastic rod. The swimming is dexterously performed all the while the form-changes are produced, and there is nothing like random or awkward bumping against other objects. With human skill it would be difficult enough to row and guide an object of fixed shape with one long whip; but to adjust its guiding action to the rapidly changing and bewildering shapes of this astasia would indeed be wonderful. Why does the creature do these things? and what stimulates it to makes such alterations of its shape? The locomotion of the infusoria answers a double purpose. It carries them about as hunters after food, and it brings them into contact with the water currents their motions occasion, and thus assists their respiration. The changes of shape may help respiration, digestion, and also the elimination of waste matter. The food particles must be tossed and shaken in the digestive fluids as effectively as in pestle-and-mortar manipulation, or brisk spoon-stirring to quicken solution, and as any of the contents get aërated, either through the integument or by inception of fresh water, they must carry their supply of oxygen to all parts of the creature. No structure can be discovered in the whip, but it appears a sensitive organ as well as a locomotive one.

Ehrenberg first described this little animal as *Trachelius trichophorus*, thus placing it in a higher rank than is now assigned to it. Kent says he mistook "the thick, cord-like flagellum for an elongate neck-like prolongation of the body, similar to that met with in the ciliated genera, *Lachrymaria*, *Amphileptus*, and *Trachelius*." This was a mistake easy to make with the imperfect object-glasses and means of illumination at the great naturalist's disposal, and it was one of those errors that point to a truth. A careful examination with an excellent oil immersion, $\frac{1}{12}$th of Leitz, shows that the whip is used very much like the long neck of a Trachelius or Amphileptus. The term *flagellum*, or *whip*, which applies very well to many lash-like swimming organs, is an insufficient one for the long instrument of this Astasia, which may be often seen exploring with its tip, and executing movements more like a proboscis than a whip.

Kent says, concerning the internal structure of this creature, that Bütschli finds its contractile vesicle of "a somewhat complex character, the water discharged in the act of systole being partly driven into smaller lateral diverticula, as obtains in *Entosiphon* and certain species of *Anisomena*, and as also observable in *Paramecium*, and among higher ciliata."

It multiplies by *long division*—or longitudinal fissure, and can also develop internal germs.

There are many so-called "species" of Astasia, but how many deserve that term is doubtful. The water our specimens were taken from abounded, in a minute form, like very small shape-changing bags filled with shining granules at the widest end. They altered their shape, however, within much narrower limits than the bigger fellow, who supplied amusement for a considerable part of one evening.

TRICYCLES IN 1884.

By John Browning,

Chairman of the London Tricycle Club.

THE STANLEY SHOW.

THE novelties in tricycles this season are so numerous that space could not be spared in Knowledge to give even a list of them. I shall mention, then, only those which appear to me to possess great merit. If I omit to

name some contrivances which are undoubtedly very novel, it must not be supposed that I have overlooked them. There are many novelties brought out every year that are worse than useless—they are mischievous or even dangerous. The "Rover," which I have already referred to in my previous articles, seemed generally to be appreciated as highly as I had anticipated and had led my readers to believe it would be.

The "Traveller," No. 2 (could not the makers call it the "Triple-Steering Traveller," the "Triplex," or the "Triplycycle," to distinguish it?) came in for a good share of notice, which it well deserves, for the oftener I ride it the easier it seems to *travel*, and the more perfectly it steers. There were many machines of the "Humber" type at the Floral-hall; but while many of them were bad copies of the original model, this had special advantages peculiar to itself; the height of the saddle being adjustable, as well as the steering-handles, and the loose backbone and free hind-wheel being replaced by a backbone and hind-wheel always under the control of the rider.

The principal feature of the Stanley Show, however, I consider to have been the numerous two-speed gearings.

Three special machines with such gearings, kindly made for me to my own specifications, were exhibited, each by a different maker — a special light "Europa" by the St. George's Foundry Company, a special light "Sterling" by Adam Burdess, and a special light front-steerer by Rucker.

Of the "Europa" and the "Sterling" I can report most favourably, so far as I have been able to try them. The "Rucker" was exhibited in an unfinished state, and it is not yet completed, Mr. Rucker, like a true artist, desiring to almost remake it, though I would have accepted it as it was. It may be at the Agricultural Hall on the 6th inst. I expect a great deal from this machine, and I am not alone in this anticipation, for my friend, Mr. Grace, one of the best judges of a tricycle known to me, selected it, without my naming it to him, as the most promising machine in the whole exhibition. This "Rucker" will combine, I hope, the lightness and speed of a "Humber" with the safety of a front-steerer and the comfort of a two-speed machine.

The Coventry Machinist Co. exhibited two new two-speed gearings. One which had two chains, one on each side of the machine, will, I think, prove valuable, and may prove to be the best yet introduced. It is a double driver with either chain, that is when either the slow-gearing for hill climbing or the high-speed arrangement is brought into action. I hope to be able to give some definite details and information respecting this machine shortly.

The same firm exhibited a novel arrangement of a tricycle on springs, which promises to reduce vibration when riding over rough roads to a minimum.

Two special features of the exhibition seemed to me to be a reduction in the size of the wheels and a reduction in the weight of the machines.

A maker told me that a year ago only the demand for sociable tricycles was for wheels to be 48 in. in diameter, while now the principal demand is for wheels 44 in. only.

The makers of the "Rover" turn out a machine with 42-in. wheels, but destroy half its value by refusing to supply it except with level gearing. This 42-in. machine, geared up to about 50 in., would be one of the best possible mounts for a strong rider.

Referring to light machines, Starley Bros. exhibited a new light roadster, weighing 67 lb.—a distinct advance on anything they have hitherto done, the style and finish being equal to the plan of the machine.

Dr. Richardson has stated that it is of the utmost importance that everything possible should be done to reduce the vibration in tricycles. Messrs. Lamplugh & Brown have applied an adjustment to their well-known long-distance saddles, which enables any rider to loosen the stretched leather and enjoy the luxury of riding with a sagged saddle; and whenever, from the effects of wet or hard riding, the leather gives too much, so as to touch the steel frame, by a simple turn or two of a nut, to restore the saddle to its former condition.

Messrs. Brookes have in their new lever tension saddle, in which the leather is attached to helical springs, given us a still more luxurious seat. The test of time alone will show whether the saddle will retain these advantages unimpaired; but the work is excellent, and it should be durable.

Many of the exhibitors at the Floral Hall were sadly cramped for space. There are only two buildings in the Metropolis where such a number of novelties in tricycles as our makers now annually produce could be adequately exhibited and easily inspected. These are the Agricultural Hall and the Crystal Palace. But, possibly before next February, the Battersea Park Exhibition may be added to the number. A great exhibition of tricycles will open at the Agricultural Hall, on Thursday, March 6, the day before this number of KNOWLEDGE is published. Many new inventions will be shown at this exhibition which were not ready in time for the Floral Hall. There will be nearly a thousand machines well displayed, and I hope to give an account of the principal novelties in early numbers of KNOWLEDGE.

THE UNIVERSE OF SUNS.

By RICHARD A. PROCTOR.

(*Continued from page 97.*)

BEFORE passing from the celebrated Memoir of 1785, —which I have been careful to discuss at some length on account of the imperfect nature of the description usually given,—I deem it well to quote yet one more passage, partly because it contains sentences calculated to remove all doubt as to the nature of Herschel's ideas in 1785 respecting the galaxy, but chiefly because the closing words show that he was prepared for the possibility that later researches might lead to modifications of his theory :—"The rich parts of the Milky Way," he says, "as well as those in the distant broad part of the stratum, consist of a mixture of stars of all possible sizes, that are seemingly placed without any apparent order. Perhaps we might recollect that a greater condensation towards the centre of the system than towards the borders of it should be taken into consideration; but with a nebula of the third form, containing such various and extensive combinations as I have found to take place in ours, this circumstance, which in one of the first forms would be of considerable moment, may, I think, be safely neglected. However, I would not be understood to lay a greater stress on these calculations than the principles on which they are founded will permit; and if hereafter we shall find reason, from experience and observation, to believe that there are parts of our system where the stars are not scattered in the manner here supposed, we ought then to make proper exceptions."

In the year 1789, Herschel prefaced a list of 1,000 nebulæ by an essay of considerable interest, relating to the formation of nebulæ. His reasoning does not readily admit of condensation; and to give it at full length would occupy more space than can here be spared. I would, however, advise the student of Herschel's theories to pay close attention to this paper, as it contains a very distinct description of the ideas entertained by Herschel at this comparatively

early stage of his progress as an observer of the sidereal heavens. He enumerates at the close the theory that nebulæ are stellar systems in various stages of development, the planetary nebulæ in which the (supposed) component stars seem most equally distributed, being the most aged and even approaching towards a period of change or dissolution. "This method of viewing the heavens," he remarks, "seems to throw them into a new kind of light. They now are seen to resemble a luxuriant garden, which contains the greatest variety of productions in different flowering beds; and one advantage we may at least reap from it is that we can, as it were extend the range of our experience to an immense duration. For to continue the simile I have borrowed from the vegetable kingdom, is it not almost the same thing whether we live to witness successively the germination, blooming, foliage, fecundity, fading, withering, and corruption of a plant, or whether a vast number of specimens, selected from every stage through which the whole plant passes in the course of its existence be brought at once to our view?"

But the time was now approaching when Herschel was to abandon almost wholly the theory which he had enunciated in 1785, not only in its relation to the sidereal system but to nebulæ and star-clusters. Already in 1796, he pointed out that the hypothesis of a general uniformity of structure in the galaxy is too far removed from the truth to be depended upon. This, indeed, does not imply a distinct withdrawal from the theory of 1785, which was not in reality based on a general uniformity of stellar distribution; but the stress now placed by Herschel on probable varieties of structure is a novel feature in his theoretical treatment of the subject. I pass on to the memoir of 1802, which is the first in which Herschel distinctly states that his ideas had undergone a change.

It was in this paper that Herschel enunciated his views respecting binary stars. Such stars, as well as all forms of multiple stars are now for the first time distinguished from insulated stars. But it is specially worthy of notice, here, that Sir W. Herschel speaks of the insulated stars as alone, in all probability, the centres of planetary systems resembling our own. "The question will now arise," he says, "whether every insulated star be a sun like ours, attended with planets, satellites, and numerous comets? And here, as nothing appears against the supposition, we may from analogy admit the probability of it. But, were we to extend this argument to other sidereal constructions, or still farther, to every star of the heavens, as has been done frequently, I should not only hesitate, but even think that from what will be said of stars which enter into complicated sidereal systems, the contrary is far more likely to be the case; and that probably we can only look for solar systems among insulated stars."*

Now it will be obvious from a consideration of Herschel's theory of 1785, that though he by no means regarded the stellar system as spread with that general uniformity of distribution described in the popular travesties of the theory, yet he was equally far from regarding the stars of the Milky Way—taken in a body—as differing from the stars forming our constellations. The very principle on which his star-gauges were based requires that any considerable portion of the Milky Way should resemble in general respects a corresponding portion from regions of the stellar system nearer to the earth. There would be irregularities,—large stars and small stars, aggregations and segregations,—in one portion as in the other, and though the details of one would not resemble those of the other, yet a general resemblance Herschel undoubtedly supposed to exist between all such portions of the sidereal system. Now, however, in 1802, he entertains a widely-different view. "The stars we consider as insulated," he writes, "are also surrounded by a magnificent collection of innumerable stars, called the Milky Way, which must occasion a very powerful balance of opposite attractions, to hold the intermediate stars in a state of rest. For though our sun, and all the stars we see, may truly be said to be in the plane of the Milky Way, yet *I am now convinced by a long inspection and continued examination of it, that the Milky Way itself consists of stars very differently scattered from those which are immediately about us.*"*

And when, having described the various orders of star systems he passes on to the consideration of the Milky Way, Herschel shows clearly in what this difference of construction consists, according to his new views. "On a very slight examination," he says, "it will appear that this immense starry aggregation is by no means uniform. The stars of which it is composed are very unequally scattered, and show evident marks of clustering together into many separate allotments. By referring to some one of these clustering collections in the heavens, what will be said of them will be much better understood than if we were to

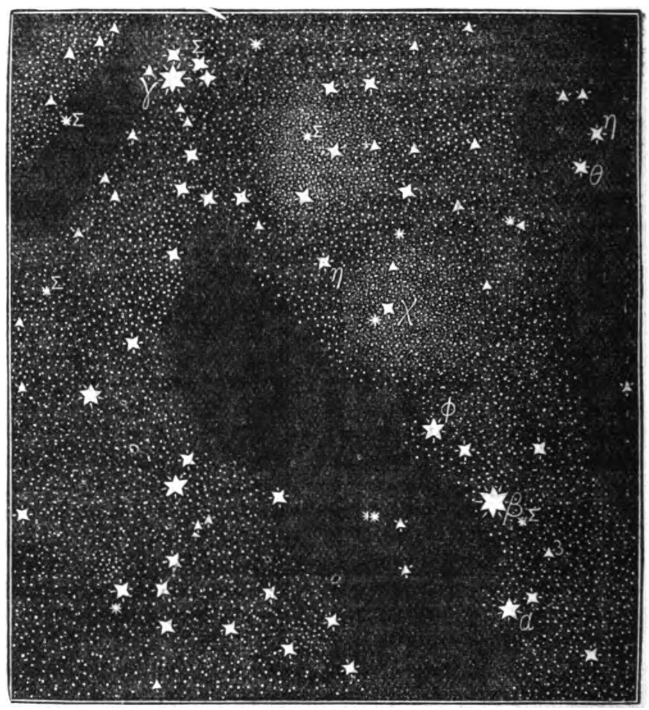


Fig. 6.—The Clustering Region of the Milky Way in Cygnus.

* Sir W. Herschel mentions among the stars which he supposes to be far removed from the disturbing influence of other orbs—Arcturus, Capella, Vega, Sirius, Canopus, Markab, Bellatrix, Menkar, Schedir, Algorab, and Propus. I cannot see why Propus—a fifth magnitude star on the borders of the Milky Way should be included in the list. All the others are bright stars more or less isolated, even Algorab being of third magnitude as well as in an exceptionally barren region. Propus was probably a misprint for Procyon.

* As some writers who have either not studied Sir William Herschel's original papers at all, or have paid but superficial attention to them, have bluntly contradicted my statement (which Struve also made) that he gave up later in his career the theory which he had adopted in 1785, and the principle of generally uniform distribution on which it was based, I have emphasised the closing words of the above passage. Writing seventeen years after the theory of 1785 had been advanced (even then only tentatively) and sixteen years before the close of his wonderful series of researches the statement is unmistakably clear in purport.

treat of them in a general way. Let us take the space between β and γ Cygni for an example, in which the stars are clustering with a kind of division between them" (Fig. 6), "so that we may suppose them to be clustering towards two different regions. By a computation founded on observations which ascertain the number of stars in different fields of view, it appears that our space between β and γ, taking an average breadth of about five degrees of it contains more than 331,000 stars; and admitting them to be clustering two different ways we have 165,000 stars for each clustering collection. Now *the above-mentioned milky appearances deserve the name of clustering collections, as they are certainly much brighter about the middle and fainter near their undefined borders. For in my sweeps of the heavens it has been fully ascertained that the brightness of the Milky Way arises only from stars; and that their compression increases in proportion to the brightness of the Milky Way.*"

Let the reader note the reasoning here very carefully; for the passage is most important and Herschel's condensed style and the absence of any direct reference to the theory he had enunciated seventeen years before has caused the

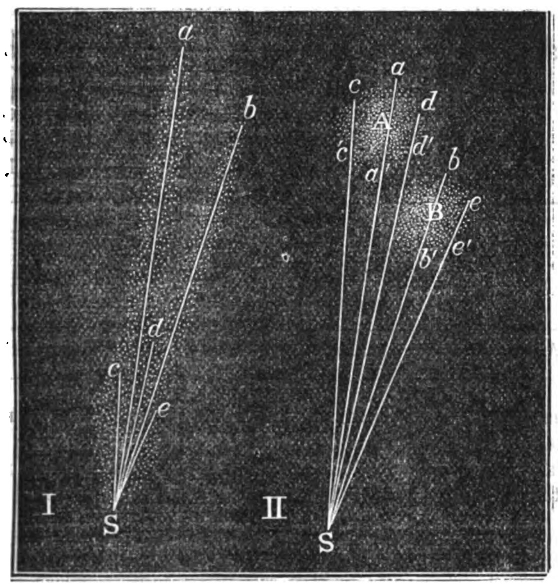


Fig. 7. Illustrating the views of Herschel (I.) in 1785 and (II.) in 1802.

significance of the passage to be commonly overlooked. Observe,—Herschel now regards this important portion of the Milky Way (the brightest part in the northern hemisphere), as composed of two clustering collections—that is, as surrounded on all sides by comparatively vacant spaces. Between us and this region of the Milky Way there lies, then, a vast space not so richly filled with stars. Now in the theory of 1785 Herschel explained rich regions of the Milky Way, such as those between β and γ Cygni, by supposing a great extension from the sun at S (Fig. 7, I.) towards the heart of each rich region as along the lines S*a* and S*b*, with relatively small extensions in directions S*c*, S*d*, S*e*. It involved a perfect change in the very conception of the Milky Way (as a whole) to explain the richness of these regions (typical, be it remembered, of many such regions) as due to a real aggregation of stars as at A and B (Fig. 7, II.) with such varying ranges of stars along the lines S*a*, S*b*, S*c*, S*d*, and S*e* as are shown in this second figure. The gauges can no longer be understood to indicate the extension of the sidereal system from the sun.

Sir William Herschel here abandons in a special case the very principle on which he had based the interpretation of the gauges. He had already in the same paper, as we have seen, abandoned the same principle as regards the Milky Way generally. Yet although he thus definitely gave up that principle generally, and specifically indicated by a remarkable illustrative case how and where he had abandoned it, that principle and the theory based (tentatively) on its application are calmly quoted in works on astronomy as if Sir William Herschel had definitely said the very reverse—or as if he had spoken thus:—"In 1784 I provisionally adopted a principle, and in 1785 I indicated a theory, which seemed suggested by the first application of that principle to the Milky Way. *Now after a long inspection and continued examination I am convinced the theory and principle are both sound, and that the Milky Way consists of stars scattered like those around us.*" Compare this with what he actually said (see the italicised passages above), and the absurdity becomes manifest.

We can understand, then, so much the more clearly what Herschel means when in this paper of 1802 he says that he *now* finds the Milky Way to consist of stars very differently arranged from those around us. But he makes further remarks respecting the clustering aggregations in Cygnus, which show yet more clearly what his new views were, though by a strange misconception those additional remarks have been supposed (even by the few who recognise his change of view) to signify that he regarded the question between the two views as an open one. He proceeds, "we may, indeed, partly ascribe the increase both of brightness and of apparent compression—in these clustering regions—to a greater depth of the space which contains these stars; but this will equally tend to show their clustering condition; for, since the increase of brightness is gradual, the space containing the clustering stars must tend to a spherical form, if the gradual increase of brightness is to be explained by the situation of the stars." Here clearly what Herschel means is, that we must take into account not only the clustering condition of two such groups as are shown at A and B (Fig. 7, II.) but also the fact that a line from the sun at S through the heart of each cluster (as S*a* through the cluster A and S*b* through the cluster B) passes through a greater range of stars than a line nearly towards the edge of either cluster as S*c*, S*d*, S*e*. (We see that *aa'* and *bb'* are greater than *cc'*, *dd'*, and *ee'*.) The gradual increase of brightness towards the middle, implies, he asserts, as strongly as the richness of the whole of these regions, that they are due to real clusters somewhat round in shape. He as explicitly accepts this view, in fact, as he implicitly rejects the view that the increase of brightness towards the centre is due to the state of things illustrated in the first drawing (I.) of Fig. 7. For in the latter case the increase would neither be gradual nor regular, unless we assumed a highly improbable adjustment of the two extensions in directions tending *exactly* from S.

In the paper of 1802, Herschel also for the first time advocates the theory that some of the nebulæ are not clusters of stars, but formed of some substance possessing "the quality of self-luminous milky nebulosity," and "possibly at no great distance from us." It is interesting to notice that he now no longer regards the light of the great nebula in Orion as arising from immensely distant regions of fixed stars. "Even Huyghens, the discoverer of it, was already of opinion that in viewing it, we saw, as it were, through an opening into a region of light. Much more would he be convinced now, when changes in its shape and lustre have been seen, that its light is not, like that of the Milky Way, composed of stars. To attempt even a guess at what this light may be would be pre-

sumptuous. If it should be surmised, for instance, that this nebulosity is of the nature of the zodiacal light, we should then be obliged to admit the existence of an effect without its cause,"—meaning probably that the zodiacal light is due to solar illumination, and that there is no sun which can be supposed adequate to the illumination of a region so large as that occupied by the Orion nebula. "An idea of its phosphorical condition," Herschel adds, "is not more philosophical, unless we could show from what source of phosphorical matter such immeasurable tracts of luminous phenomena could draw their existence and permanency; for, though minute changes have been observed, yet a general resemblance, allowing for the difference of telescopes, is still to be perceived in the great nebulosity of Orion, even since the time of its first discovery."

(To be continued.)

HOW TO MAKE USEFUL STAR MAPS.

By RICHARD A. PROCTOR.

In Fig. 6 I have put in the stars so as to correspond to the year 1840. Otherwise the comparison between the two figures would have been interfered with. But it is very necessary, indeed, that in constructing maps from a catalogue or atlas of an old date, the student should pay attention to the effects of precession, by which all the stars are shifting their place, not indeed on the heavens, but athwart the meridians and parallels of our maps. The reason why this is important is very obvious. If we want to find where some object we wish to observe is situated, we mark it down according to its recorded place on our map. Now if this is done on a map constructed nearly for the same date as that corresponding to the place given to the object, all is well. But if, instead of this, the stars of the map are marked in for another date, our object will be wrongly placed among them. We must, in fact, shift the object, so as to correspond to the date of the map. This is very readily done if we know the rate at which precession is shifting the places of stars in different parts of the heavens.

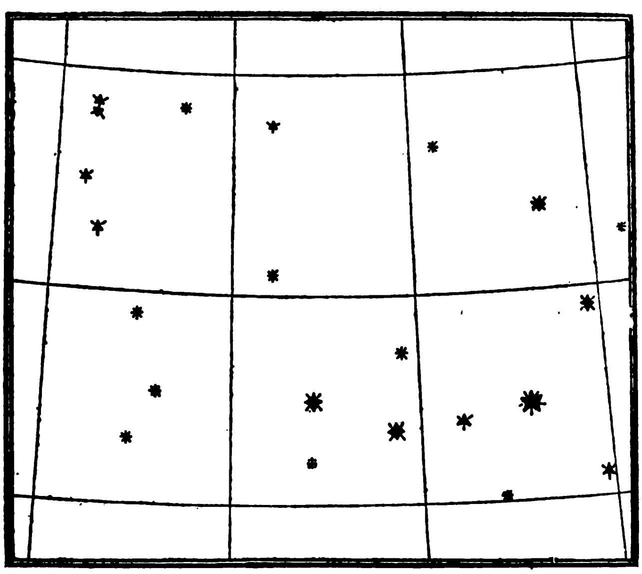


Fig. 6.

For this purpose the following table, abridged from one in my "Handbook of the Stars" (and calculated expressly for that work, which is now out of print), will be found useful:—

TABLE OF CHANGES DUE TO PRECESSION.

R.A.	0h.	1h.	2h.	3h.	4h.	5h.	6h.	R.A.
Annual variation in Dec.	20″ N S	19″ N S	17″ N S	14″ N S	10″ N S	5″ N S	0″	Annual variation in Dec.
R.A.	12h.	11h.	10h.	9h.	8h.	7h.	6h.	R.A.

ANNUAL VARIATIONS IN R.A.

Dec.	s.	s.	s.	s.	s.	s.	s.	Dec.
75° N	+3·1	+4·4	+5·5	+6·6	+7·4	+7·9	+8·1	75° S
60	3·1	3·7	4·2	4·7	5·1	5·3	5·4	60
45	3·1	3·4	3·7	4·0	4·2	4·4	4·4	45
30	3·1	3·3	3·5	3·6	3·7	3·8	3·8	30
15	3·1	3·2	3·2	3·3	3·4	3·4	3·4	15
—	3·1	3·1	3·1	3·1	3·1	3·1	3·1	—
15 S.	3·1	3·0	2·9	2·8	2·8	2·7	2·7	15 N
30	3·1	2·9	2·7	2·5	2·4	2·3	2·3	30
45	3·1	2·7	2·4	2·1	1·9	1·8	1·7	45
60	3·1	2·5	1·9	1·4	1·1	1·8	0·8	60
75	+3·1	+1·8	+0·6	−0·4	−1·2	−1·7	−1·9	75
R.A.	12h.	13h.	14h.	15h.	16h.	17h.	18h.	R.A.
Annual variation in Dec.	20″ S N	19″ S N	17″ S N	14″ S N	10″ S N	5″ S N	0″	Annual variation in Dec.
R.A.	24h.	23h.	22h.	21h.	20h.	19h.	18h.	R.A.

In this table, the two upper rows of hours of R.A. must be taken with the right-hand table of declinations to give the annual change in R.A., while the two lower rows must be taken with the left-hand table of declinations. Two

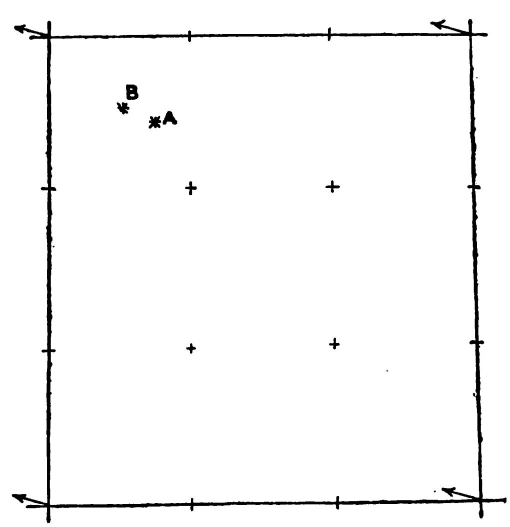


Fig. 7.

examples will serve fully to illustrate the use and meaning of this table.

Supposing we are drawing a map which includes the

space between 3h. and 4h. of right ascension, and between 15° and 30° of north declination:—Then at the corner of this space, corresponding to 3h. R.A. and 30° N. Dec. (the upper right-hand corner), the change for one year will be 14″ in declination northwardly and + 3·6s. in right ascension. So for the upper left-hand corner of the space the change is 10″ in dec. northwardly, and 3·7s. in right ascension. At the lower left-hand corner the change in declination is 10″, and in right ascension 3·4s.; while at the lower right-hand corner the change in declination is 14″, and in R.A. is 3·3s. Now we can draw at each corner of the space a little arrow indicating the change for as many years as the date differs from that of our atlas or catalogue, and each star as it is marked in must be shifted over a space equal in direction and distance to the arrow which falls nearest to it. Fig. 7 shows these arrows for such a space of the globe as we have considered above, and corresponding to the long interval of 100 years (that is, the variations 14″, 10″, 3·6s., &c., have all been multiplied by 100). A shows the place of a star in a map 100 years old, while B is its proper place for the present date.

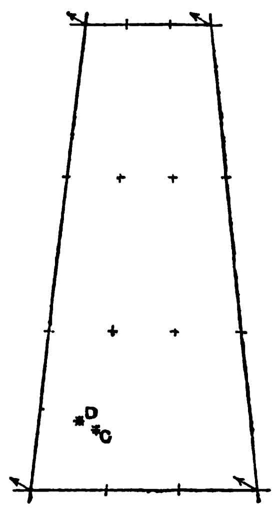


Fig. 8.

Secondly, suppose our space lies between 22h. and 23h. R.A. and 45° and 60° north declination. Then the table gives us the following changes:—

For upper left-hand corner 19″ N. in Dec. and + 2·5 in R.A.

For lower left-hand corner 19″ N. in Dec. and + 2·7 in R.A.

For upper right-hand corner 17″ N. in Dec. and + 1·9 in R.A.

For lower right-hand corner 17″ N. in Dec. and + 2·4 in R.A.

Fig. 8 represents the changes in this case, C being a star's place in a map a hundred years old as before, D its correct position.

The reader will find no difficulty in seeing how the table is used in all such cases, remembering the direction already given as to the use of the side columns with the upper and lower rows of R.A.; and also that where the letters $\frac{N}{S}$ or $\frac{S}{N}$ occur the one nearest to the hour row we are using must be taken.

It need hardly be mentioned that as Figs. 7 and 8, are only meant to explain the use of the table, it has not been thought necessary to pay particular attention to the shape of the spaces.

THE *EDINBURGH* REVIEW OF SPENCER'S "FIRST PRINCIPLES."

To the Editor of Knowledge.

Now that I have returned to my books I see clearly how that mistake of omitting exactly one line in the quotation from Mr. Spencer's "First Principles" came, viz., from the four words, "produces in us the," being repeated over each other in two successive lines, and my eye in copying mistook the second for the first. I wonder you did not perceive that with the book before you. And it was not the kind of mistake to catch one's attention in reading a proof of what I had not written—or rather, composed—myself. Mistakes of that kind, and worse, in copying have caused all the disputes about the various early MSS. of the Greek Testament, though of course the scribes thought their subject of infinitely more consequence than I did mine.

I did not like to contradict your version and derivation of *axiom* from ἄξιος without referring to dictionaries, both Greek and English. You say it is something "established by experience and worthy to be accepted." I find, as I expected, that it is exactly the contrary of the first of these things; and the second applies to all truths. Axiom is a technical word, a mere English writing of ἀξίωμα, which in philosophy meant "that which is assumed as a basis of demonstration, or a self-evident proposition"; for both which meanings Liddell and Scott give Aristotle as the authority. And I suppose they might have added Euclid, though I have not a Greek Euclid to refer to. I am satisfied Newton meant the same by his "Axioms or Laws of Motion," though he illustrated them by some experience which alone was quite insufficient to prove them. The first really involves the "principles of sufficient reason" too. Johnson defines an axiom as "that which is self-evident and cannot be made plainer by demonstration." That is the way I used it throughout the article, and I have no more to say of Mr. Spencer's use of it and "Postulate" than I have said there. I see, too, that Mr. Romanes in his last book on "Instinct" speaks of Mr. Spencer's habit of making definitions to suit his own purposes quite as strongly as I have.

The Edinburgh Reviewer.

[Recognising the mistake in copying as certainly not intended I was not on the look-out for its cause, which doubtless was as the Edinburgh Reviewer thinks.

The word ἀξίωμα is as unquestionably derived from ἄξιος as στερεώμα from στερεος,—and it can no more be doubted means a something weighed (ἄγω) and not found wanting, than that the second means something made firm (στερεόω). If we are to argue about words, I might ask how "that which is assumed as a basis of demonstration" comes to be "exactly the contrary of that which is established by experience." To assume as a basis of demonstration what was not only *not* established by experience but directly contrary thereto would be to lay the foundation of a rather singular process of reasoning. In philosophy the word "axiom" was early used to signify a self-evident proposition, though earlier the word meant a "decision," "something thought worthy of universal acceptance," because "so established by experience as to admit of no reasonable doubt." Whoever gave the definition, "that which is assumed" &c. was unable to express in English what very likely he understood very well in the Greek; for an "assumption" and a "decision" are two very different things, and the word "axiom" in each of its two uses,—the primary meaning, according to which experience was in question, and the

derived meaning, according to which obviousness was chiefly dwelt upon,—has always meant a "decision," not an "assumption."

The so-called axioms of Euclid are of both sorts, and of yet a third kind (8 and 9, Book I.),—they include (i.) decisions based on experience, (ii.) self-evident propositions, and (iii.) truisms (statements, indeed, which, in one aspect, may be regarded as merely definitions). Euclid however did not use the word ἀξίωμα, but called these preliminary statements κοιναὶ ἔννοιαι, common thoughts.

But the point is not what Euclid would perhaps have meant if he had used the word, nor even what Newton may have meant,—but what the Laws of Motion are. If I asserted that Newton used the word axiom in its proper sense as a decision based on experience, and deserving to be accepted, it was,—First because he is so careful to indicate a part of the experience by which the laws have been established, and secondly because he knew and shows over and over again that he knew, how manifestly incorrect it would be to describe as "self-evident propositions" principles which had escaped the keen insight of Greek philosophy, and had remained undetected during centuries of research and inquiry. The Edinburgh Reviewer says Newton perceived the first law to be a necessary truth on the "principle of sufficient reason," and again "it required Newton's power of divination to perceive why the first law is a necessary truth." If all the time and thought given to this subject before Newton failed to establish the law, and only the powers of divination possessed by him of whom it was justly said that he surpassed the whole human race in mental insight, finally established the law, then words must change a little in meaning before we can speak of the law as a self-evident proposition. As reasonably might the great law of gravity itself be called self-evident. But as a mere matter of fact Newton had no more to do with establishing the first law of motion than he had to do with establishing that which he numbers as the second, though it is more often given as the third. Kepler established the law that *every body perseveres in a state of rest, or of uniform motion in a right line, unless it is compelled to change that state by forces impressed on it;* Galileo established the law that *the alteration of motion is proportionate to the motive force impressed, and is made in the direction of the right line in which that force is impressed;* and Newton established the law that *the mutual actions of two bodies upon each other are always equal and in opposite directions*, or *to every action there is always opposed an equal reaction.*

As regards Mr. Spencer's use of words, or the nature of his definitions, it will be time enough to speak when the Edinburgh Reviewer condescends to touch the chief point of my complaint against him,—viz., that in his review he has been unjust to Mr. Spencer in assigning to him, contrary to the plain meaning of his words, doctrines so absurd as that the Almighty Being, ("touching" Whom Mr. Spencer really says, with Elihu the approved of God, "we cannot find Him out," He is Unknowable) is identical with Force, and other ideas scarcely less monstrous.—R. P.]

The output of coal in the United Kingdom is stated to have reached, last year, the high total of 163,750,000 tons, or an increase of more than $4\frac{1}{2}$ per cent. on the previous year's total. According to the late Professor Jevons' estimate, the quantity mined in 1883 should have been 178,100,000 tons, so that the actual output was 14,400,000 tons less.

Gossip.

From this time forwards we may hope to reduce rapidly the pressure both on our space for readable matter, and on our advertising columns. We are afraid to say how many columns of letters, notes, paragraphs, articles, &c., have been standing over for want of room.

Mr. Newton Crosland's letter appears this week as promised. I have explained that nothing I have said has been intended by way of ridicule, simply because there is nothing ridiculous in not being acquainted with particular facts or understanding special lines of reasoning. Of each one of us it must be said that even of the known we can have mastered but a most minute part, and the known is as nothing to the unknown. There are some to whom one form of not knowing seems absurd,—to wit not knowing that we do not know. But in reality this is common enough. Apart from paradoxists, whose special weakness this is, most of us are unaware how ignorant we are of some matters: and unless we have had time fully to find out, there is nothing absurd in the mistake. That Mr. Crosland does not know how little he knows of the most perfect and beautiful—most beautiful because perfect—process of reasoning by which Newton established the Law of Gravity, and applied it to explain the movements of the heavenly bodies is obvious in every line of his writing on the subject; but there is nothing to be ridiculed in this,—at least by those who, looking inwards, readjust their own readiness to fall into similar error.

To say the truth, the matter with which he deals, or tries to deal, is not so simple as it is supposed to be by most of those who have given the popular account of the matter. Sir Edmund Beckett in the preface to his "Astronomy without Mathematics" points out that some seemingly simple propositions have necessarily to be assumed in that work because there is no simple proof of them, and amongst these he mentions motion in an elliptic orbit. No wonder that when he, a passed master in the subject, treats it as not to be understood save by mathematicians, the incomplete attempts at explanation referred to by Mr. Crosland as if they were authoritative expositions fail to convince the learner.

Mr. Crosland cannot see how velocities (and consequently centrifugal tendencies) generated by gravity can overcome gravity, how therefore a body which moving in an elliptic orbit has drawn nearer to the sun can ever pass farther from him, if the law of attraction only is at work. It does not (I suppose) suffice for him, as it does for others who have not dealt with the mathematical proof, to be told that,—in elliptic motion under gravity there is an alternation of velocity on the one hand and amount of pull on the other hand, by which first one then the other has for the time the advantage, the two being only on a par at two points on the orbit. At these two points where the planet is at its mean distance, velocity and attraction are so perfectly balanced that if only the direction of the body's motion were right (at right angles to the sun's direction) the body would move in a circle, with that velocity, and at that distance, round the sun. But as matters are, the body at one of these points is moving on a course carrying it nearer to the sun, and its velocity is being increased by the sun's pull. The centrifugal tendency grows as the square of the velocity, and also as the distance diminishes; it gains

therefore more quickly than the velocity. The angle of approach continually diminishes, then, from this point, till at last approach is converted into recession. This happens as certainly, though the velocity is increased by solar gravity, as it would if the planet were an engine, and the engineer had turned on more steam. Then as the planet recedes, at first slowly then more quickly, centrifugal tendency is diminished till at the other point where the planet is at its mean distance the centrifugal tendency is exactly such as corresponds with the velocity, and would give a circular orbit, if the body's direction were right. But at this point the body is still receding, the sun is now diminishing the planet's velocity, centrifugal tendency is diminishing as the square of the velocity, and also as the distance increases. Hence the angle of recession continually diminishes until at last recession is converted into approach. Then as the planet approaches, at first slowly then more quickly, centrifugal tendency is increased, till at length it just corresponds with the velocity, and would give a circular orbit if the body were moving in the right direction.

WHAT is prettiest in Newton's demonstration of all this (and much more) is to my mind, where he in effect takes three successive points anywhere on the body's course, and shows that given the first two points on a particular ellipse (determined by the body's known velocity in passing from one point to the other) the third point lies also on that ellipse. This he does by a very simple but pretty application of the geometry of limits. All the rest follows at once. The second and third point lying on that ellipse so does the fourth; so the fifth; the sixth; the seventh; and so forth, to infinity. Nor does the mathematician need to be told that though the proof does not and cannot deal with really successive points, and though, if it did, no number of successive points would give a finite part of the orbit, the proof is mathematically perfect. It shows in effect that (1) let the three points first dealt with be ever so near they will lie on the ellipse, and (2) that any number, however great, of these ever-so-near points may be taken without the ellipse being departed from. Wherefore the ellipse will be traversed, for ever,—unless external disturbing forces intervene.

I HAVE been a good deal surprised, and so has Mr. Thomas Foster, to find that several correspondents (some of whom are not of the cavilling and ill-reasoning kind referred to by him in a foot-note in last number of KNOWLEDGE) misjudge entirely the gist and purport of his papers on the Morality of Happiness. They seem to imagine that the question of Conduct cannot possibly be considered without reference to Religion,—not to Religion in its stricter verbal sense, but to Religion as commonly understood by the word. Now of course there can be no manner of doubt that rules of conduct are regarded by an immense number of persons as essentially associated with religious doctrines. But it is equally indisputable that there is no *necessary* connection. How much or how little of conduct may be regarded as depending on religious doctrine is a question I need not consider. It is certain that *some* parts of conduct are with at least nine hundred and ninety-nine out of a thousand not governed by religious considerations at all. Mr. Foster at the very outset repudiated all reference to the religious element in regard to conduct. He set out by showing how conduct has been evolved in the struggle for life, how it continues to be evolved, and manifestly will be evolved hereafter, in such sort as to make conduct increasing the totality and fulness of life prevail in the long run over conduct which exercises a contrary effect. Then he showed how the totality and fulness of light are related to happiness. He is now engaged in showing how essential to the general well-being is due care of personal well-being. And he will show later how essential is care of the interests of others, not only for social well-being, but personal well-being also. If the line of conduct thus indicated as *right*, and the line of conduct which we are thus led to deprecate as *wrong*, be found to be closely akin to those approved or deprecated in former times with the sanction of what has been regarded as revelation (I am speaking here in the widest sense, so as to include much that *every one* of *every* religious persuasion would deny, and *does deny*, to be revealed) well and good: but it is no part of Mr. Foster's purpose to consider this point. Neither is it any part of his purpose to call in question any belief held by the members of any church or sect whatsoever. He simply indicates those lines of conduct which promise increase of general happiness, and those on the other hand which threaten the reverse. Among the multitudinous teachings of past ages, under different conditions, and in times when either much sterner or much milder conduct may have better suited the surroundings, and therefore have been best, are doubtless some agreeing with and some not according with those which seem best now. But this has nothing to do with Mr. Foster's purpose here, any more than—it should seem—with Mr. Herbert Spencer's wider teaching. The subject as treated by Mr. Foster is purely scientific, not religious at all, and he declines absolutely (with my sanction, as he would have declined at my request had not his view been the same as mine) to pursue any of the religious discussions which some correspondents have desired to raise over his scientific analysis of the Morality of Happiness.

BUT perhaps the very title Morality of Happiness has been by many misunderstood. Does any one by any possibility imagine that Mr. Foster's subject is the question whether it is moral or immoral to be happy? It would seem so, though the idea really appears too absurd to be entertained. Does Mr. Foster's reasoning read as if he wished to discuss a question so idle and so puerile? His subject really is the Morality of Happiness,—that is, he is inquiring what is the course of conduct indicated when happiness is taken as the guide, when the well-being of the body social and therewith the increase of the fulness of its life, is regarded as determining individual conduct. In other words Mr. Foster's question is,—*not*, Is there Morality in Happiness? but What are the moral rules suggested when we take general happiness as the guide of conduct?

THE mistake of several correspondents in regarding *this* as a religious subject reminds me of a story which some of our readers may not have heard:—A Highlandman asks a companion the truly perplexing question, "Say, Donald, is a pumple-pee a pird or a peast?" After some vain cogitation, Donald replies rather angrily, as is the way in such cases, "Eh, mon, ye mauna trouble folk with releegious questions on ilka day." ("Ilka day" or "each day" being here equivalent to "a week day."

LET me assure anxious readers that neither Mr. Foster nor any one else proposes "to trouble folk with releegious questions" in our professedly weekday magazine.

WHAT *has* been the matter lately with the erst genial *Punch?* It seems to find the present age so far inferior

to all past time that it is calling aloud—though vainly—for "the Muse that laughed and stung on Gulliver's indignant tongue,"—a very Billingsgate Muse—to denounce the coarseness and vulgarity of the age. A homœopathic remedy truly, though not a curing of like by like infinitesimal doses, but by doses such as our age would hardly swallow. Does the age really merit Mr. Burnand's accusation? Is it really so much worse than past ages that nothing short of the venom (strong poison, but still poison) of Swift as he neared the doom of long-threatened madness, can avail to correct its evil ways? What does the would-be Swift, the "stifled spirit" as he calls himself (with probable truth), the "vexed soul," find now that is worse than in the days when Pope showed us in the Dunciad what were the literary men of his time and how in his time they might be rebuked without offending the taste of his day? He finds "the statesman's cut and dried abuse:" this is not new, if the satire of past ages be believed. "The lawyer's pæans in his fees, and actor's noisy juggleries"; the critic, "*sans* scholarness or kindliness"; "patriots for bright sword carrying fish-fag's tongue": why, all this was said, but better, in the good days of old. Then Science has its turn, the biologist being taken as the typical wretch, "probing with his two-foot rod his muddy substitutes for God," though it takes an essentially unscientific eye to see anything like this in poor dear protoplasm. Lastly we are told, in lines meant to be fine, though in truth little better than windy nonsense, that we now have

> All our sires held first and best
> In pufferies of all sizes dressed,—
> Till England watch through England's press
> The fall of English manliness.

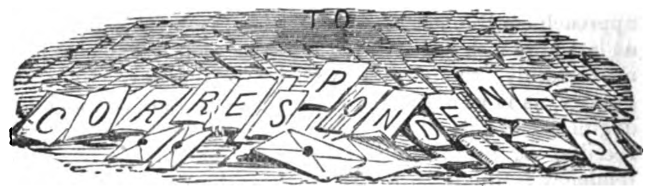
The only feature of the time which may be regarded as apparently uglier than of yore, might be worthily castigated in the pages of *Punch*. Our modern Juvenal who cries for "some Juvenalian verse," "which Nature" (as he truly says of his) "may desire in vain" has but feeble lash strokes for this foul evil, and expresses "in English carefully destroyed" his anger against

> Those who serve the weekly meal
> For jaded gluttons, keen to gloat
> On savoury sauce of Anecdote.

His picture of keen jaded gluttons gloating on sauce will not go far we fear to correct the evil of which our Society journals are the mere symptoms, not the cause. Students of the literature of the eighteenth century know that the evil existed then as now, though not then so public.

In passing we note that even in speaking of the memory of the well-loved Calverley, our contemporary shows the same touch of jaundice. Where only pleasant memories need have been evoked, he ponderously brings down his truncheon on "ponderous posers," "prigs and bores," "sombre wisdom," "the Big Bore race," "severe quidnuncs," and so forth—and is actually at the pains to call the attention of the "much-to-be-pitied Grand Panjandrums of Useless Knowledge" (ponderous prosing this) to Mr. Calverley's Examination Paper in "Pickwick." How it may be with Grand Panjandrums we know not; how it is with the author of this jaundiced tribute we can guess: but we should say that certainly scarce any others need to be reminded of so well-known a paper.

Thackeray says somewhere that a man is a bore when he is bored: we fear Mr. Burnand has been badly bored of late.



"Let Knowledge grow from more to more."—Alfred Tennyson.

Only a small proportion of Letters received can possibly be inserted. Correspondents must not be offended, therefore, should their letters not appear.

All Editorial communications should be addressed to the Editor of Knowledge; *all Business communications to the* Publishers, *at the Office, 74, Great Queen-street, W.C.* If this is not attended to delays arise for which the Editor is not responsible.

All Remittances, Cheques, and Post Office Orders should be made payable to Messrs. Wyman & Sons.

The Editor is not responsible for the opinions of correspondents.

No communications are answered by post, even though stamped and directed envelope be enclosed.

WORKS ON BOTANY.

[1129]—T. Garth wishes to know the name of "a cheap but trustworthy book" on botany, with which to follow my papers in Knowledge. This is rather a vague request, for everything depends upon whether he wants a book on general botany or one on the British wild flowers only. For the flower, I should recommend Oliver's "Lessons in Elementary Botany" (Macmillan, 4s. 6d.); for the latter, either Bentham's "British Flora" (Lovell Reeve, 12s.), or Hooker's "Student's Flora of the British Islands" (Macmillan, 10s. 6d.). Bentham is easier for beginners, and has an artificial key; Hooker is fuller, more technical, and more advanced. If T. G. has access to any library containing Syme's "English Botany," with Sowerby's large coloured plates (a big and expensive book in eleven volumes) he will do well to follow the papers with the illustrations there. So-called "popular" works, in which a few common species are ill described, ought to be avoided. Better go to half-a-guinea, and buy Hooker while one is about it. As to a lens, a sufficiently good one may be got for six or seven shillings. But here, too, one of Mr. Browning's platyscopics (lowest power best for a beginner) will give far more satisfaction, and prove a better investment in the long run; the price (I believe) is 18s.

Grant Allen.

THE "NEW PRINCIPIA."

[1130]—I am quite aware that you can, in your own columns, make me appear as ignorant and ridiculous as you please. I have no power to check this centrifugal force of your assumptions and arguments, which fly off at a tangent wherever I venture to control them. I quite "understand" why the centrifugal force of an engine on a railway "varies in potency;" it is owing to the guidance of the driver acting on obedient machinery; but no such explanation is available in the case of a planet moving in a curve round its primary. Newton and his disciples contend that the *acceleration* of speed, caused by gravity, develops a centrifugal "force" supplementary to a direct motion already assumed, which "force," in its turn, practically overcomes its own cause! I have shown, in the "New Principia," that this hypothesis is self-contradictory and impossible in reality; whereas, according to my theory, the variability in the motion of the planet is due to the variation in its polarity, the slanting direction of the axis and the presentation of a different pole to the sun being sufficient to account for the motion of the planet and the fluctuating of this motion. In the case of the engine on the railway, mentioned by you, if, when left to itself, without the aid of the driver, it changed its centrifugal tendency whenever it was prudent to do so, we should then be obliged to search for this skilful adaptation in some automatic arrangement. In the case of the planet, I find this automatic device in the attraction and repulsion of polarity; and no other explanation of the movements of the planets seems to me to be satisfactory. Every schoolboy knows that a body having once acquired a motion in a straight line has a tendency to continue in the same direction.

I rely upon you setting me right with your readers.

Newton Crosland.

GHOSTLY VISITANTS.

[1131]—The book you refer to in your article on ghosts is, I think, "The Night Side of Nature;" but my recollection of the circumstance is somewhat different from yours. I think that the authoress (?) said that Sir Walter was passing through his *entrance-hall* when he thought he saw Byron standing on the opposite wall, and, after vainly trying to dispel the illusion where he stood, he walked up to find a gentleman's *cloak* hanging on a hat-peg.

I lately heard a well-authenticated story of a "vision" connected with a family in this neighbourhood. One of the sons was an officer in the army before Sebastopol. One night another son, at home, thought he saw his brother standing in his room, his uniform smeared with blood flowing from several wounds in his breast. Next morning he acquainted his family with what he had seen, adding that he felt sure that his brother was killed.

The next mail brought a letter from the colonel of the regiment, informing the father that his son had been killed in the trenches on the very night when the spectre appeared to his brother.

After the war the regiment returned to England, and one day the brother was invited to dine at the officers' mess. Naturally, they began to talk of their comrade's death, when the young man interrupted the narration by saying, "Stop! Tell me; were his wounds here, and here, and here?" pointing to his own breast. One of the officers said, "How do you know this?" "Wait," he said; "am I right?" "You have described the places exactly," replied the officer. The young man then told them what he had seen.

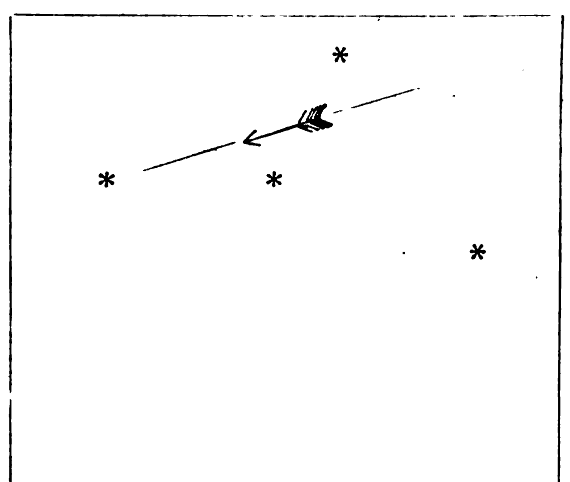
I have heard another equally well-authenticated case, of a young man in Valparaiso seeing the appearance of his mother (whom he had left in Scotland) twice in one night, of his noting the fact next morning on a sheet of paper, which he handed sealed to a friend, and of his learning by next mail that his mother had died that night, as he had feared. His nerves were so much shaken by the circumstance, that he gave up his situation and returned home.

An intimate friend of mine assured me that he had the story from the young man's own lips immediately on his return.

Broughty Ferry, *Feb.* 16, 1884. SENEX.

METEOR.—RED GLARE.

[1132]—Large meteor seen here (Pornic, Loire-Inférieure) a few minutes past 8 p.m. Dec. 19, in Cygnus as per sketch. Light blue; lit up everything like moon five days old; left trabs for few



seconds. 17th, about 6.15 p.m. (Paris time), I noticed the Zodiacal light for the first time this winter; earlier than usual, but not so early as last year; faint, but distinct. Sunday evening not so clear, and Zodiacal light hardly visible at all. The red sunsets, green moon, and lamps have been very remarkable here, since Nov. 30 especially. I have had a keen eye on the skies for forty years, and never saw anything like them before, especially in this respect. From one window (N.W.) is pitchy black, with nimbus (16th); I look from the other (in the same wall) to S.W., and meet a blaze of crimson light, which pains the eyes, night having fallen. Now, *usually* a red west would include *all* the west. A squall and shower succeeded, as if the phenomenon were wholly terrestrial, whereas the localisation of the glare over the sun's place would almost suggest a more cosmical origin than as explained by Mr. Ranyard, *i.e.*, that the Z.L. (supposed to extend beyond the orbit of Venus?) had this year from some cause come as far as our own. That night (16th) there was up to 11 p.m. a brightness in the west, which was not visible over the horizon from south to north-by-west, except these, in a region not opposite the moon, then shining, but to the south of the sun's place.

On the 17th, when night had practically begun, the effulgence of the west made my eyes ache, not red, but yellowish white. Just after sunset the whole west sky was streaked with milky bands, all radiating from the sun, but not like clouds. They looked like masses of matter. They must have been there before sunset, but were then invisible. They were not rays of light, being not perfectly straight and definite. I never saw such before. A procession of small nimbi were sailing rather rapidly from north to west (wind north-east), so these fleecy pencillings must have been much higher up, being motionless. HALLYARDS.

MIGRATION.

[1133]—"Naturalist" hits the weak point in Mr. Grant Allen's theory of migration exactly, when he says that it will not apply to small birds. He might have stated further, and with equal truth, that there are very few birds it will apply to. Mr. G. Allen generalises too much; he constantly speaks of "the bird," as if all birds, or, at any rate, all migrants, were much the same in their habits and powers of flight.

He contrasts our slow and tortuous methods of getting from one point to another with "the bird's" free and easy plan of rising high enough in the air to see his destination, and sailing straight for it. The very idea of, say, a willow or golden-crested wren, or any, indeed, of our warblers, soaring up in the air like a skylark and making a "bee-line" for some object, is, to any one who has at all studied the way of birds, so absurd that it is hard not to think that Mr. Allen must have forgotten how large and varied a number of our "feathered friends" are birds of passage.

Now, in reality, birds travel much as we do, only they use hedges, and especially watercourses, as we do roads; they are, in fact, their highways. Birds of all kinds will follow rivers for days, and flocks of swallows have been seen to fly for miles along the seashore itself. (Perhaps they were looking for a "landmark?")

Then, again, birds constantly travel by night. Our summer visitors usually arrive in early morning. This I have known to be the case on our east coast. How about the "landmarks" in this case?

Young birds, too—last year's nestlings—often arrive by themselves, some days after the older birds. This is a well-known fact in the case of some of our Norwegian visitors. These young travellers could know no landmarks.

But though Mr. G. Allen's theory can scarcely apply to all, or even to many, of our migrants, it may to some, as the vast tribe of swallows, of which, possibly, he was chiefly thinking; and yet even here I may be allowed to doubt that the most sagacious swallow can "spot" Beachy Head from Boulogne "as the tallest white cliff," or recognise St. Martha's Down by its "high-perched chapel tower." Still, this may be so. C. COURTENAY HODGSON.

CAVERNS IN THE EARTH AND MOON.

[1134]—Perhaps you will allow me as one who knows the neighbourhood to correct the quotation from Mr. Mattieu Williams, relative to the caverns in the counties of Galway and Mayo. In the first place, the river joining Lough Mask and Lough Conn ought to be the river joining Lough Mask and Lough *Corrib*. In the second place, I doubt if it is correct to speak of this river as deriving its corroding powers from its passage through a peat-bog. There is no peat-bog between the lakes. The upper lake (Lough Mask) is fed by a number of streams, some of which, no doubt, pass through peat-bogs, but the principal tributaries enter the lake at a considerable distance from the point where the water leaves for Lough Corrib, and do not enter it directly from peat-bogs.

The extent to which the limestone rocks of this district are caverned by water can only be known by those who visit it in winter. I am inclined to think that the honey-combing stretches across the country for fully twenty miles, the two most remarkable phenomena marking its limits. One of these is the disappearance of the river connecting Lough Mask and Lough Corrib, the other is the disappearance of the Turloughmore river for a distance of fully three miles (I believe) between Corofin and Claregalway. In both places some water runs overground during the winter, the disappearance being complete in summer. Between the two rivers a third, known as the Blackwater, which passes Shrüle and runs near Headford, exhibits similar disappearances on a small scale, and I have a suspicion that

some of the rises of water near Headford belong to the Turloughmore river instead of the Blackwater. A small stream which falls into Lough Corrib near the little village of Cross, between Cong and Headford, shows the same characters, and a rise which takes place just below the village of Cross is probably connected with Lough Mask, though the stream comes from a very different direction. In winter, water (usually from Lough Mask) bursts up in numerous places, which a summer visitor would never dream of—the outburst sometimes amounting to a considerable river, which again disappears after a very short course. In other places it forms a small temporary lake, which becomes dry in spring. It was, I believe, the outburst of water from beneath which led to the abandonment of the canal to which Mr. Williams refers. It seems evident that fish can pass freely through these underground caverns.—I remain, &c., W. H. S. MONCK.

Dublin, Dec. 22, 1883.

THE FIRST INVENTOR OF THE TELEPHONE.

[1135]—An article headed thus in KNOWLEDGE, Vol. IV., No. 108, reminds one of a very old, but a very true proverb, "that there is nothing new under the sun." This applies to the telephone as well as to many other things.

The *speaking telephone* (not the electric one, of course), may have been *re*-discovered by Philip Reis and others, and, in that sense, be an original invention; but it has been known for some generations, if not longer, to the natives of India, as the following story will show:—

When the speaking telephone was first talked of and began to be heard of in India, a friend who then held an official position in the Punjaub spoke of this then wonderful new invention to his bearer, a native servant who had been with him some years.

The man replied, "But, Sahib, this is a thing which we natives know very well; I and a fellow-servant use it daily in your compound when we wish to talk to each other across it from our godowns" (a native servant's room is thus called).

His master, very much surprised, asked to see this instrument, which was afterwards produced for our benefit also; it consisted of two hollow pieces of bamboo, each about 8 in. in length, and closed at one end with a piece of parchment, through which was passed a string some 50 to 100 ft. in length; it perfectly answered the purpose for which it was intended.

I have been told that the natives of Ceylon use a similar apparatus to enable them to communicate with each other across the deep valleys in that island. COSMOPOLITAN.

LETTERS RECEIVED AND SHORT ANSWERS.

OBTUSE.—Not obtuse but acute and right. The hour is later the farther east the place is, not the other way as Dr. Brewer puts it.—J. PARRY. "Quite too awfully exactly described."—E. WAL WARMINGTON. The subjects you object to are not objected to by quite so many as you seem to think, but quite the contrary. Just before your letter four others expressing the contrary opinion were opened. As to what you propose you must please yourself, and allow us to try to please the great majority. Yours is the one solitary letter expressing that view out of about the last two hundred received. If 1 in a company of 200, or 5 in a company of 1,000, told the host to "shut up" as you politely tell us, while the rest wished him and many specially asked him to go on, do you suppose he would view with *very* great pain a threat of withdrawal on the part of what could hardly be called even a minority (but rather, simply "a disturbing element which had no business in the company")?—H. B. You are quite right. There is not the slightest reason for supposing that the sun shining on a fire puts it out. You might easily miss the reference to the matter in early numbers of KNOWLEDGE, as it was under a different heading. It was in an article of mine on Fallacies I think that the question was touched on. Yesterday I watched with some interest the vigorous burning of a fire in an open fire-basket in the full blaze of sunlight. It *looked* dull enough, because the sun's light was so strong; but it needed only a little watching to show that the fire was full of vigorous life: and the men close by who used the fire for their work were manifestly not troubled by any fear that the sun would put it out. Dr. Brewer beautifully explains the matter somewhat like this; The rays of the sun put out a fire because they act unfavourably on the process of combustion.—CANIS MINOR. 1. In an astronomical telescope the north pole of Mars (as seen from northern places) appears below the southern. 2. In the southern hemisphere the reverse happens. 3. I suppose my reply to 2 *does* bear on the flat-earth theory; but it is impossible to say *what* does or does not bear on a theory whose only positive quality is absurdity. 4. I imagine the reason why in most maps the north part is put uppermost is that at our northern stations our heads are directed more nearly towards the north pole of the heavens than the south.—A. F. OSBORNE. Thanks; oddly enough, the day I had sent off that reply I saw Anster's translation (published by Messrs. Routledge) on a railway bookstall. Mr. Spencer's statement was made to Professor Fiske, and published in America several years ago.—J. T. PEERS. It has been somewhat blatantly *asserted*—but not by any one competent to form an opinion—that the Gulf Stream does not appreciably affect our climate. You will scarcely find any "authority" saying so.—F. W. HALFPENNY. Doubt much whether the numerous ladies we count among our readers would altogether appreciate your suggestion that they should take to smoking in order that their minds may be turned to more serious matter.—ERNEST EVANS. Thanks.—W. THOMSON. Do not know.—A. G. BIGGS.—S. M. B. They must be badly bound, it is not so in any of my volumes.—L. ALLEN. Thanks.—J. B. ADAMS. Thanks for amusing parody, which should appear but that I fear to wrong some who love the original even as I have myself suffered from parodies of well loved lines.—J. H. C. Yes, certainly, the parts of tire nearer the ground move more slowly in space though they go round the centre at the same rate.—M. J. HARDING. Your letter reached me after the number for Feb. 29 was already arranged. Thanks for suggestions in your other letter. Your suggestion, that my "Gossip" should *commence* each number, comes oddly soon after a suggestion by a rather ill-mannered correspondent that it should, on no account, appear at all.—C. CARUS WILSON. I know of no remedy for the spots which have formed on the O. G. of your telescope. The glass will not be much the worse for them unless they cover much of the surface.—W. WALTERS. Do not know the work; but should think it very probable De Morgan wrote it, as he did many for that series.—E. K. HALL. It is quite evident your corrections of the account of the motion of the terminator at p. 222, Vol. III., are needed (and one more, 38′ for 37′ in third statement)—*i.e.*, daily motion, 12° 11′ 26″·7; longitude of central point at end of first day, 77° 49′ 33″·3; of second day, 65° 38′ 6″·6.—J. J. POLLOCK. There is such a line, or rather circle; and if on one side of the line it is Wednesday, on the other it is Thursday. An "insurmountable anomaly" you call this; but not really much more so than the existence of a different instant at two places when Wednesday changes to Thursday. For instance, when Thursday has begun with us in England it is still Wednesday with you in Ireland.—THOS. PEARSON. There is but one way in such cases: send article on approval with stamped and directed cover for return if not approved.—J. W. STANIFORTH. We owe you an apology; your name was, we remember, most clearly written.—H. B. LINDSAY. Many thanks. The lines are beautiful, though they would be *caviare* to the dullards who asked what Vega meant by making a sun speak. Albeit I object strongly to Goethe's invitation to *comets* to roll. It can*not* be permitted.—E. PLATT. Nay; if there is a subject we should not care to touch on in these columns it is the one you name. It is one of those on which, as on many, we personally take a "median way;" and we have long since learned that however true in one sense the proverb *medio tutissimus iter* may be, it is in another sense most untrustworthy. A traveller along the middle way finds himself saluted with missiles from both sides,—meant to go across the way perhaps, but if they "fetch" *him, that* is no consolation.—G. M. Thanks.—P. DE HAMEL. Thanks.—EXCELSIOR, T. L. HALL, G. W. G., H. A. B. Mean time, not solar time.—AN AMATEUR, W. POOLE. Properly to discuss your theories would take too much space. Cannot under any circumstances answer through the post.—H. D. CUTBILL.—ARIEL.—E. GROTT.—C. F. C. NOAD.—R. M. PRATT.—J. H. GARFIT.—J. M.—J. B. PEARSE.—J. L.—D. B. CAZAUX.—J. E. MACKLAND.—T. B. BATTERSBY.—R. PHILLIPS.

Our Mathematical Column.*

BY RICHARD A. PROCTOR.

NOTES ON EUCLID'S FIRST BOOK.

WITH SPECIAL REFERENCE TO THE SOLUTION OF GEOMETRICAL PROBLEMS.

THE First Book of Euclid treats chiefly of the properties of triangles and parallelograms. An examination of the book suffices to show that Euclid had proposed a definite line of treatment leading up to certain important propositions. Hence many

* As the Editor has been repeatedly requested to give only in this column what is easily followed and likely to be useful to the general student, he has decided to give a series of papers on the successive books of Euclid, accompanied by a series of easy riders on the several propositions of the different books. On alternate weeks will appear "Easy Lessons in Co-ordinate Geometry."

useful properties are left untouched in this book. It is surprising however how many valuable propositions Euclid has succeeded—by a judicious method of treatment—in introducing into his plan without marring its symmetry.

In attacking deductions either immediately depending on the First Book of Euclid or involving it in part only, it is necessary that the student should have at his fingers' ends, so to speak, all the most useful properties established in the First Book, and also several important properties deducible from this book. We proceed to examine the most valuable of these.

In the first place, let us run through the first book and notice whether there are any properties whose converse theorems, though not proved in Euclid, may be readily established.

Euclid has proved the converse of prop. 5 in prop. 6, of prop. 13 in prop. 14, of prop. 18 in prop. 19, of prop. 24 in prop. 25, of props. 27 and 28 in prop. 29, of prop. 37 in prop. 39, of prop. 38 in prop. 40, and of prop. 47 in prop. 48. The other propositions which admit of a converse are the following:—

Euc. I., 15, of which the converse is, "If two straight lines C E, D E (Fig. 1) on opposite sides of a line A B, make equal angles C E A,

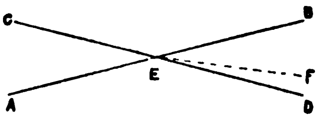


Fig. 1.

D E B with A B; then C E and E D are in the same straight line. This is obviously true, since—if C E produced fell in some other direction as E F, we should have the angle B E F equal to the vertical angle C E A, and therefore to the angle B E D, which is absurd. We shall refer to this proposition as Euc. Bk. I., prop. 15, *Conv.*

Euc. I., 17. The converse of prop. 17 is Axiom 12. We touch here on the great defect of Book I., a defect, however, with which our subject does not lead us to deal.

Euc. I., 34.—This proposition contains three theorems, each of which has a converse, but the converse of the third is not true. The converse of the first part of the proposition is this:—*If the opposite sides of a four-sided rectilinear figure are equal, the figure is a parallelogram.* This is obviously true; for, having A B (Fig. 2)

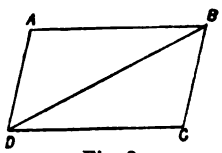


Fig. 2.

equal to D C and A D equal to B C, also the base B D common, we have the angle B A D equal to the angle B C D, Euc. I., 8, and the triangles B A D, B C D equal in all respects, Euc. I., 4, so that, the angle A B D being equal to the angle B D C, A B is parallel to D C, and similarly, A D is parallel to B C. We shall refer to this proposition as Euc. I., 34, i. *Conv.*

The converse to the second part is also true. It is,—*If the opposite angles of a four-sided rectilinear figure are equal, the figure is a parallelogram.* In this case, having the angle D A B (Fig. 3) equal to

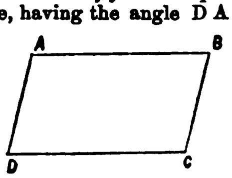


Fig. 3.

D C B, and A B C to A D C, we have the sum of the angles D A B and A B C equal to the sum of the angles A D C and D C B, or either sum equal to half the sum of the four interior angles of the figure; that is, to two right angles Euc. I., 32, cor. 1: hence (Euc. I., 28) A D is parallel to B C., and similarly A B is parallel to D C. We shall refer to this proposition as Euc. I., 34, ii. *Conv.*

The converse of the third part of the proposition is not a true theorem, for it is clear that besides D C B (Fig. 2) an infinite number of triangles may be drawn on the base B D equal to A B D, each of which would give a quadrilateral divided by B D into two equal triangles, but this quadrilateral would not be a parallelogram.

Euc. I., 35 and 36.—Each of these propositions has a converse which may be established in the manner of props. 39 and 40. We shall refer to these converse theorems as Euc. I., 35 and 36, *Conv.*

Euc. I., 43, has the following converse,—If *H K G* (Fig. 4) *is drawn parallel to the two sides A B, D C of a parallelogram A B C D and E K F parallel to the other two sides, in such a manner that the parallelogram H F is equal to E G, then H F and E G are complements about the diagonal A C,—in other words the point K lies on the diagonal A C.* This may readily be proved to be true. For, if A C

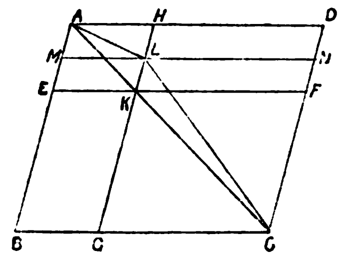


Fig. 4.

had some other position as A L C, then drawing M L N parallel to A D or B C we have the complement B L equal to the complement L D; therefore B K is less than L D: but B K is equal to K D; therefore K D is less than L D, the less than the greater, which is absurd. Hence B K and K D are complementary parallelograms,—or K is a point in the diagonal A C. We shall refer to this proposition as Euc. I. 43, *Conv.*

(*To be continued.*)

Our Whist Column.

By "Five of Clubs."

A WHIST GEM.*

THERE are some very excellent rules at Whist, and others not quite so good, which are too often held by average Whist-players as general rules. The idea is that if any exception exist at all to these rules, it is only when some "passed master" of Whist sees the way to a *grand coup.* For ordinary players these rules are supposed to be practically universal. The result is that average players steadily obey these rules, which are not general at all, to the discomfiture of the best laid plans of a partner of wider experience and deeper insight.

We lighted, however, the other day on the book before us, a little work sold freely at railway stations and elsewhere, which besides laying down several of the good old rules as general (though they are open to numerous exceptions), gives a number of perfectly preposterous rules as valid. Let us cull and comment on a few of its remarks.

"Only lead Ace from Ace and five others,"—misprint for "four others." A rule almost universal, but did not Mr. Lewis show recently in our columns how even as an original lead the penultimate may occasionally be the right card? However, let this rule stand as advisedly a general one.

"Wherever you can as fourth player, only make the trick with the Ace, play it. Never pass an Ace trick in order to keep the lead." On the contrary it repeatedly happens that keeping back the Ace is essential to success in the strategy of the hand,—apart from cases, by no means infrequent, where it is obvious that if you play the Ace you make no other trick, while if you hold it up you make two tricks.

Here is an odd general rule, but odd only because of bad English: what the writer means is sound enough:—"Draw a conclusion and adhere to it until the play proves it wrong: then proceed again." It would be pleasant to have a partner who was always forming wrong conclusions and acting on them till they were proved wrong, then beginning again, and so forth. For "until" read "unless" and all is pretty well.

"Always throw out a forcing card, when, whatever the loss to you, the loss to the enemy must be greater." Here probably the author of this remarkable book should have written "if" for "when," or omitted the comma before "when." With the comma the rule implies that every "force" is good. The rule correctly given admits of being made much more general in the form, "Always be ready to sacrifice a trick if you can gain two or more by so doing." This is an excellent rule, and would be as useful as sound, if it only showed *how.*

"The golden rules in Whist are (1) lead the lowest of a strong suit, (2) the highest of a weak suit." Consequently from Ace,

* "Whist." By J. R. W. Warne's Bijou Books. (London: Warne & Co., Strand.)

King, three small ones,—plain suit,—*always* lead a small one; and from King two small ones, *always* lead the King: you have no idea how pleasing the effect will be on a choleric partner.

"Never lead from two plain cards while you have more of another suit:" not even if you know, for instance, that your partner must be strong in the suit to save game, and that leading from any other suit must destroy that chance.

"Where you must lead from a weak suit, lead invariably the highest"—an unnecessary repetition of that second golden rule which, as we have suggested above, would make your partner so cheerful if always followed to the letter.

"When a suit is at once the second player's strong suit, and the fourth's weak, by all means lead it: the advantage to your partner is great;" for instance, if second player holds the master-card in the suit, has signalled for trumps, and having secured command in trumps can bring in his partner's established suit, the advantage to your partner must be great; for he will be saved all further trouble, and so will you. Now had you perhaps not followed the law thus laid down, you might have made a trick or two in your own hand and your partner's suits, and positively have had the trouble of playing another hand before the game was quite over.

But when we turn to another page and find *this* rule, "In all suits, trumps apart, only lead King when you have *Queen* and *Knave*," we find it only necessary to ask in conclusion, What sort of Whist players are we likely to meet if Messrs. Warne's "Bijou Whist" is very widely read and very carefully followed?

ANSWERS TO CORRESPONDENTS.

Correct solutions of Endings, p. 62, received from A. S. Orr, Cucumber, E. F. Lewin, J. N. Prang, and E. Le Mesurier; of 2nd (1st already correctly solved), from Q. T. V.; of 1st, from A. Jenkins, Emily Carmichael, Club Knave, and Cucumber.

Q. T. V.—Even if position could not have arisen in play, it might still resemble a Chess problem. I have seen a problem in which a King's Bishop was afield though King's Pawn and KKt's pawn unmoved. Who says that Y-Z had won all the other tricks? See illustrative game last week, which was in one striking point like the ending. Yet it occurred in play several months since. Its occurrence in play led one of the company to remark that he remembered something like it in an ending of Mr. Lewis's: and this having been hunted out we thought worth quoting here.

W. G. NICKLES.—Counting at short whist is simple enough. The side which first makes five points wins game, which counts as *three* if other side have made no points, as *two* if they have made no more than two points, and as *one* if they have made more than two. The rubber is won by the side which first wins two games. The scoring for the rubber is obtained by adding *two* to the points for the two won games, and deducting the points for the game lost, if any.

IN answer to many correspondents we note that of course there was an erratum in the reply at p. 105, that at long whist the question, "Can you One, partner?" cannot be asked until the first trick is completed. The words which follow show that the "not" should have been omitted. We were indicating up to what moment the question can be put.—FIVE OF CLUBS.

Our Chess Column.

AN UNSOUND BUT AMUSING GAME.

THE following game illustrates the lesson which our brilliant Chess Editor has taught, that the armour of Saul though good in itself may not suit the stripling warrior. Black had been learning the openings, and also the general rules for the conduct of a game. In guiding my play against him, I took at once his knowledge and his want of knowledge into account. Had he not shown a proclivity for certain little weaknesses I should never have ventured on the line of play pursued in the little game which follows. Even if I had had the aid of Queen's Rook, how Mephisto would have turned the attack against me after the 8th move, if I could conceivably have tried such an experiment on him or indeed on any full-grown Chess warrior!—

KING'S BISHOP'S GAMBIT.

Remove White's QR.

White. R. A. Proctor.	Black. Another Amateur.	White. R. A. Proctor.	Black. Another Amateur.
1. P to K4	P to K4	6. Kt to QB3	B to Kt2
2. P to KB4	P takes P	7. P to Q4	Kt to K2
3. B to QB4	P to Q4	8. B tks BP (ch) (a)	K takes B
4. B takes P	Q to KR5 (ch)	9. Kt to KB3	Q to KKt5
5. K to B sq	P to KKt4		

White. R. A. Proctor.	Black. Another Amateur.	White. R. A. Proctor.	Black. Another Amateur.
10. P to KR3	Q to Q2 (b)	20. K to K sq	Q takes Kt
11. Kt tks P (ch)	K to K sq	21. Q to KB6(ch) (g)	Q to B2
12. Q to KR5(ch)	Kt to Kt3	22. Q takes P	Q takes Q
13. Kt takes RP	Q to KB2	23. B takes Q	B to K2
14. Kt to Q5 (c)	Kt to QR3	24. R to KB sq	K to Kt2
15. P to QB3 (d)	Kt to K2	25. B to K5 (ch)	K to Kt3
16. QKt to B6(ch)	B takes Kt (e)	26. R to KB6(ch)	K to R4
17. Kt tks B (ch)	K to B sq	27. K to KB2	P to QB3
18. Q tks R (ch)	Kt to Kt sq	28. K to KB3	R to QB sq (h)
19. Kt takes Kt	Q to QB5 (ch) (f)	29. White mated in four (i)	

(*a*) Up to this point the moves have been "book"; White's 8th move is a novelty and a tolerably unsound one.

(*b*) Queen falls if she goes anywhere else.

(*c*) Unsound; but White *knew* from experience that Black would reply as he did.

(*d*) Also here, that Black would play to dislodge the QKt and proffer exchange of Queens.

(*e*) Or Queen falls.

(*f*) Far better take the Kt at once: White usefully displaces his King, and gains a move.

(*g*) To improve his position before exchanging Queens.

(*h*) Playing near the precipice. Black had pushed on his King with the idea that he was developing his game. In moves 28 and 29, his object has been to bring his Q Kt and Rook into play. White's object in advancing his K to B3 is entirely overlooked.

(*i*) 29. P to Kt4 (ch) / K to Kt4 30. P to R4 (ch) / K takes P 31. R to R6 (ch) / K to Kt4 32. B to B4. / Mated.

R. A. P.

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3. THE MOON.
4. THE PLANETS.
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See Advt. Pages for full Syllabus.

The following arrangements are complete: the numbers in brackets referring to above list.

BIRKENHEAD, March 10 (6).
ALTRINCHAM, March 11 (5).
CHESTER, March 12, 13 (1, 2).
HAVERSTOCK-HILL, March 14 (2).
BLACKHEATH, March 17, 18.
REIGATE, March 19 (2).
HITCHIN, March 20 (5).
UXBRIDGE, March 21 (1).
LONDON (Brixton Hall) March 28, April 1, 4 (1, 2, 3).
„ (Memorial Hall), March 24, 27, 31, April 3 (1, 2, 3, 4).
BIRMINGHAM (Town Hall), April 18, 23, 25, 28; May 2 (1, 2, 3, 5, 6).
LEAMINGTON (Royal Music Hall), Four Afternoons, April 17, 19, 24, 26 (1, 3, 5, 6); Two Evenings, April 17, 24 (2, 4).
OXFORD (Town Hall), May 12, 13, 19, 20.
S. HELEN'S (Lanc.), April 22 (2).

NOTE.—*All communications respecting Lectures should be addressed to Mr. John Stuart, Royal Concert Hall, St. Leonards.*

KNOWLEDGE
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PLAINLY WORDED—EXACTLY DESCRIBED

LONDON: FRIDAY, MARCH 14, 1884.

CONTENTS OF No. 124.

THE EXTRAORDINARY SUNSETS.

By A. Cowper Ranyard.

THE information which has come to hand in the course of the last three months shows that the extraordinary sunset and sunrise phenomena, which first attracted attention in England at the beginning of November, have been equally attracting attention in Australia, Japan, South Africa, India, and North and South America; and that the area within which blue and green suns and extraordinary sunsets were first observed, had some relation to Krakatoa as a centre. But there is still a great deal which requires explanation, especially as to the way in which the Krakatoa dust-cloud spread, apparently against prevailing winds.

There can be little doubt that the blue and green appearance of the sun observed in tropical countries for a few weeks after the great eruption was caused by dust floating in the atmosphere. Similar appearances have frequently been observed during dust-storms. Professor Piazzi Smyth, in Vol. XIV. of the "Edinburgh Observations," gives a coloured picture, showing what he describes as a blue sun. The sketch was made on entering Palermo Bay on the evening of March 10, 1872, when the sirocco wind, laden with fine dust from the African desert, was blowing.

Mr. Edward Whymper, while ascending Chimborazo on July 3rd, 1880, saw an eruption of Cotopaxi, 65 miles distant. A column of smoke of inky-blackness, as seen against the sky, rose into the air with prodigious velocity. He estimates that it rose 20,000 feet above the rim of the crater in less than a minute; it was then borne away parallel to the horizon, and ultimately passed over Chimborazo between the observers and the sun. Mr. Whymper, in describing the appearance in *Nature*, Vol. XXIX., p. 199, says, "Several hours passed before the ash commenced to intervene between the sun and ourselves, and when it did so we witnessed effects which simply amazed us. We saw a green sun, and such a green as we have never either before or since seen in the heavens. We saw patches or smears of something like verdigris-green in the sky, and they changed to equally extreme blood-red or to coarse brickdust-red, and they in an instant passed to the colour of tarnished-copper or shining brass."

Mr. G. F. Chambers, at the January meeting of the Astronomical Society, stated that the engineer of some works at Eastbourne, at which large quantities of sea-beach are crushed by steam machinery, had informed him that he had frequently seen the sun appear blue and green through the fine dust which rises into the air when the operations are in progress. Dr. Budde, of Constantinople, writing to *Nature* of Dec. 20, states that when in Southern Algeria, he heard a Frenchman exclaim, "C'est la première fois que j'ai vu le soleil bleu." Upon asking others present, "he was assured by the whole company that the sun, seen through the fine dust of a Sahara wind, had a decidedly blue colour." I will give one more quotation in proof of the connection between dust floating in the atmosphere and the blue appearance of the sun. Mr. O'Reilly, writing to *Nature* of Jan. 17, quotes Richtofen's work on China, Vol. I., p. 97. After describing the dust-laden atmosphere of the Loes district, he states that for many days the air often appears yellow and opaque, "the view is completely hemmed in, and the sun appears as a dull bluish disc."

When the accounts of the blue and green appearance of the sun, observed in India, first arrived in England, it was suggested that the appearance was due to the absorption of the sun's rays by aqueous vapour which had been thrown into the upper air during the eruption of Krakatoa. Mr. Lockyer stated that he had seen "the sun a brilliant green through steam blowing off in great quantities from a little steamer on Windermere; and again, when it was seen through a thick mist on the summit of the Simplon Pass." I have several times looked at the sun through steam, but have not succeeded in seeing such an appearance. If aqueous vapour were present in the upper air in the condition of steam, the heavens would appear covered with cloud; but the blue appearance of the sun is not described as having been seen through cloud. Indeed, many of the observers are careful to state that there was no cloud. We have in England, in winter, plenty of opportunities of observing the sun through an atmosphere charged to various degrees of saturation with aqueous vapour, but the sun does not appear blue.

In his article in the *Times*, Mr. Lockyer assumes that the green colour was caused by "blue and red molecules" floating in the air. He states that the "red molecules are coarser than the blue ones, and would therefore be the first to fall from the upper air as dust." But it is not necessary to assume that matter of an exquisite blue colour was floating in the air. No such blue or red matter fell in sufficient quantities to be noticeable.

The blue colour of the sun, when seen through an atmosphere laden with fine dust, may be explained by simple physical considerations. A particle whose diameter is small compared with the wave-length of light disperses different proportions of red and blue light—the longer the wave-length the less is the intensity of the dispersed light. Lord Rayleigh has shown, in a paper "On the Light of the Sky," published in the February number of the *Phil. Mag.* for 1871, that when light is scattered by small particles the intensity of the scattered light varies inversely as the fourth power of the wave-length. We are all familiar with the blue colour of the heavens, and of water in which fine particles are suspended. Under ordinary circumstances, the light dispersed by the matter floating in the air is not sufficiently intense, sensibly to affect the colour of the direct light of the sun. But if the amount of fine dust floating in the atmosphere were greatly increased, the intensity of

the dispersed light would be increased, and the blue colour of the light dispersed from the part of the atmosphere between us and the sun would sensibly affect the colour of the sun.

When the dust particles are small compared with the wave-length, the colour of the dust does not sensibly affect the colour of the dispersed light. The phenomena seen by Mr. Whymper from Chimborazo was caused by comparatively large dust particles, in which the colours of the transmitted ray seems to have been red. The dust fell upon Mr. Whymper's party, and he brought specimens of it home for examination. He estimated that the finer particles weighed about the 1-25,000th part of a grain.*

There are two reasons why the sun does not appear blue every evening when as it sets the rays pass through a long distance of dust-laden atmosphere. In the first place, the particles floating in the atmosphere near to the surface of the earth are, many of them, not small compared to the wave length of light, so that the greater part of the light dispersed by particles in the lower air, is dispersed by particles which are large compared with the wave-lengths of the visible spectrum; and next, the aqueous vapour and other gaseous constituents of the atmosphere absorb more light at the blue end of the visible spectrum than at the red, so that in the long passage through the atmosphere the loss of light at the blue end of the spectrum more than counterbalances the bluish colouring caused by the light dispersed from such fine particles as float in the lower atmosphere.

In the tropics, shortly after the Krakatoa eruption, when the air was laden with fine dust, the blue sun became green as it neared the horizon, and ultimately it sank as a reddish ball, the absorption at the blue end at last entirely overpowering the admixture of blue colour.

There can be little doubt that the brilliant sunset colours observed in Europe and other places distant from Krakatoa are due to the ordinary atmospheric absorption, chiefly at the blue end of the spectrum, of the light dispersed by dust floating at a great height in the air in smaller quantities than in the tropical regions where the blue and green sun phenomena of the first few weeks after the eruption were seen.

Some experiments carefully made by Prof. Tyndall, render it probable that under ordinary circumstances terrestrial dust is not carried to great altitudes. In September, 1877, he opened on the summit of the Bel Alp, in Switzerland, 27 flasks containing infusions of beef, mutton, turnip, &c, which had been boiled for five minutes in London, and sealed during ebullition. A gentle breeze was blowing at the time from mountains which were partly covered with snow, towards the precipice where the experiment was made. Prof. Tyndall kept his body to the leeward of the flasks, and care was taken to cleanse the pliers with which the sealed ends were snipped off in the flame of a spirit-lamp.

Twenty-three similar flasks were taken immediately afterwards into a shed containing some fresh hay, and their sealed ends were then snipped off with the same pair of pliers. The two groups of flasks were then placed in a kitchen, where the temperature varied between 65° and 90° Fahrenheit. All the flasks opened on the edge of the precipice remained as clear as distilled water, while twenty-one of the twenty-three flasks opened in the hayloft became turbid and filled with organisms. This experiment confirms a similar experiment made by Pasteur on the Mer de Glace, and shows that terrestrial dust is not under ordinary circumstances carried by the winds to great altitudes, for the germs are so minute that they would certainly be carried upward by ascending currents if terrestrial dust were carried upward. It cannot be supposed that the cold experienced at such altitudes kills them, for laboratory experiments show that the germs survive intense cold, and that long exposure to the atmosphere and drying does not kill them.

But there is always dust floating in the upper air. This is proved by the radial polarisation of the light of the sky, as seen from high mountains. In 1878 I examined the polarisation of the sky as seen from Gray's Peak, in Colorado, 14,450 feet above the sea. The heavens appeared a deep, dark blue, but by no means black. There was sufficient light from the sky to render visible a small piece of white paper at the bottom of a brass tube, when the tube was held at right-angles to the sun's rays. The polarisation of the sky showed that the light from it was dispersed by very fine particles. The phenomena of twilight also show, that the sun's light is dispersed by particles floating at a great height in the atmosphere.

Where does this dust in the upper air come from? Till recently I thought that it could not come from below, and, therefore, argued that the fine dust of the upper air must be entirely due to the *débris* of the millions of small meteors encountered by the earth in the course of the year, and burnt in the upper air? The greater number of them are burnt out at a height of more than sixty miles above the sea level; and I thought, and still think, that their ashes may, owing to the fine powder to which they are reduced, take years in sinking to the earth. But the facts which have come to light with regard to the Krakatoa dust-cloud have shaken my faith in the conclusion which I had previously come to, and had begun to treat as an axiom, that the dust of the upper air must come entirely from outside space.

(*To be continued.*)

* Taking the scientific gravity of the matter ejected from the volcano as 2·5, a cubic inch would weigh 630 grains; and a little cube of such matter, weighing 1-25,000th of a grain, would measure along its edges 1-250th of an inch, a quantity which is very large compared with the wave-lengths of the visible spectrum.

THE CHEMISTRY OF COOKERY.

XXX.

By W. Mattieu Williams.

THE changes which occur when starch granules are subjected to the action of water, at a temperature of 140°, have been described. If the heat is raised to the boiling point, and the boiling continued, the gelatinous mass becomes thicker and thicker; and if there are more than fifty parts of water to one of starch a separation takes place, the starch settling down with its fifty parts of water, the excess of water standing above it. Carefully-dried starch may be heated to above 300° without becoming soluble, but at 400° a remarkable change commences. The same occurs to ordinary commercial starch at 320°, the difference evidently depending on the water retained by it. If the heat is continued a little beyond this it is converted into *dextrin*, otherwise named "British gum," "gommeline," "starch gum," and "Alsace gum," from its resemblance to gum-arabic, for which it is now very extensively substituted. Solutions of this in bottles are sold in the stationers' shops under various names for desk uses.

The remarkable feature of this conversion of starch into dextrin is that it is accompanied by no change of chemical composition. Starch is composed of six equivalents of

carbon, ten of hydrogen, and five of oxygen. $C_6H_{10}O_5$, *i.e.*, six of carbon and five of water or its elements. Dextrin has exactly the same composition; so also has gum-arabic when purified. But their properties differ considerably. Starch, as everybody knows, when dried, is white, and opaque and pulverent; dextrin, similarly dried, is transparent and brittle; gum-arabic the same. If a piece of starch, or a solution of starch, is touched by a solution of iodine, it becomes blue almost to blackness, if the solution is strong; no such change occurs when the iodine solution is added to dextrin or gum. A solution of dextrin when mixed with potash changes to a rich blue colour when a little sulphate of copper is added; no such effect is produced by gum-arabic, and thus we have an easy test for distinguishing between true and fictitious gum-arabic.

The technical name for describing this persistence of composition with changes of properties is *isomerism*, and bodies thus related are said to be *isomeric* with each other. Another distinguishing characteristic of dextrin is that it produces a right-handed rotation on a ray of polarised light, hence its name, from *dexter*, the right.

The conversion of starch into dextrin is a very important element of the subject of vegetable cooking, inasmuch as starch food cannot be assimilated until this conversion has taken place, either before or after we eat it. I will therefore describe other methods by which this change may be effected.

If starch be boiled in a dilute solution of almost any acid, it is converted into dextrin. A solution containing less than one per cent. of sulphuric or nitric acid is sufficiently strong for this purpose. One method of commercial manufacture (Payen's) is to moisten 10 parts of starch with 3 of water, containing $\frac{1}{150}$th of its weight of nitric acid, spreading the paste upon shelves, allowing it to dry in the air, and then heating it for an hour-and-a-half at about 240° F.

But the most remarkable and interesting agent in effecting this conversion is *diastase*. It is one of those mysterious compounds which have received the general name of "ferments." They are disturbers of chemical peace, molecular agitators that initiate chemical revolutions, which may be beneficent or very mischievous. The morbific matter of contagious diseases, the venom of snake-bite, and a multitude of other poisons, are ferments. Yeast is a familiar example of a ferment, and one that is the best understood. I must not be tempted into a dissertation on this subject, but may merely remark that modern research indicates that many of these ferments are microscopic creatures, linking the vegetable with the animal world; they may be described as living things, seeing that they grow from germs and generate other germs that produce their like. Where this is proven, we can understand how a minute germ may, by falling upon suitable nourishment, increase and multiply, and thus effect upon large quantities of matter the chemical revolution above named.

I have already described the action of rennet upon milk, and the very small quantity which produces coagulation. There appears to be no intercession of living microbia in this case, nor have any been yet demonstrated to constitute the ferment of diastase, though they may be suspected. Be this as it may, diastase is a most beneficent ferment. It communicates to the infant plant its first breath of active life, and operates in the very first stage of animal digestion.

In a grain of wheat, for example, the embryo is surrounded with its first food. While the seed remains dry above ground there is no assimilation of the insoluble starch or gluten, no growth, nor other sign of life. But when the seed is moistened and warmed, the starch is changed to dextrin by the action of diastase, and the dextrin is further converted into sugar. The food of the germ thus gradually rendered soluble penetrates its tissues; it is thereby fed and grows, unfolds its first leaf upwards, throws downward its first rootlet, still feeding on the converted starch until it has developed the organs by which it can feed on the carbonic acid of the air and the soluble minerals of the soil. But for the original insolubility of the starch it would be washed away into the soil, and wasted ere the germ could absorb it. The malster by artificial heat and moisture hastens this formation of dextrin and sugar; then by a roasting heat kills the baby plant just as it is breaking through the seed-sheath. Blue Ribbon orators miss a point in failing to notice this. It would be quite in their line to denounce with scathing eloquence such heartless infanticide.

Diastase may be obtained by simply grinding freshly germinated barley or malt, moistening it with half its weight of warm water, allowing it to stand, and then pressing out the liquid. One part of diastase is sufficient to convert 2,000 parts of starch into dextrin, and from dextrin to sugar, if the action is continued. The most favourable temperature for this is from 140° to 150° Fahr. The action ceases if the temperature be raised to the boiling point.

The starch which we take so abundantly as food appears to have no more food-value to us than to the vegetable germ until the conversion into dextrin or sugar is effected. From what I have already stated concerning the action of heat upon starch, it is evident that this conversion is more or less effected in some processes of cookery. In the baking of bread an incipient conversion probably occurs throughout the loaf, while in the crust it is carried so far as to completely change most of the starch into dextrin, and some into sugar. Those of us who can remember our bread-and-milk may not have forgotten the gummy character of the crust when soaked. This may be felt by simply moistening a piece of crust in hot water and rubbing it between the fingers. A certain degree of sweetness may also be detected, though disguised by the bitterness of the caramel, which is also there.

The final conversion of starch food into dextrin and sugar is effected in the course of digestion, especially, as already stated, in the first stage—that of insalivation. Saliva contains a kind of diastase, which has received the name of *salivary diastase* and *mucin*. It does not appear to be exactly the same substance as vegetable diastase, though its action is similar. It is most abundantly secreted by herbivorous animals, especially by ruminating animals. Its comparative deficiency in carnivorous animals is shown by the fact that if vegetable matter is mixed with their food, starch passes through them unaltered.

Some time is required for the conversion of the starch by this animal diastase, and in some animals there is a special laboratory or kitchen for effecting this preliminary cookery of vegetable food. Ruminating animals have a special stomach cavity for this purpose in which the food, after mastication, is held for some time and kept warm before passing into the cavity which secretes the gastric juice. The crop of grain-eating birds appears to perform a similar function. It is there mixed with a secretion corresponding to saliva, and is thus partially malted,—in this case *before* mastication in the gizzard.

At a later stage of digestion, the starch that has escaped conversion by the saliva is again subjected to the action of animal diastase contained in the pancreatic juice, which is very similar to saliva.

It is a fair inference from these facts that creatures like

ourselves, who are not provided with a crop or compound stomach, and manifestly secrete less saliva than horses or other grain-munching animals, require some preliminary assistance when we adopt graminivorous habits; and one part of the business of cookery is to supply such preliminary treatment to the oats, barley, wheat, maize, peas, beans, &c., which we cultivate and use for food.

HOW TO GET STRONG.

(*Continued from page* 136.)

MUSCLES OF THE WAIST.

In dealing before with the muscles of the waist, I considered those in front chiefly, and the exercises then mentioned were intended to strengthen and tauten those important and usually much-neglected muscles. I have still to consider other waist muscles, and also to inquire what exercises are suitable for extending and rendering more elastic those same muscles for whose strengthening I have suggested appropriate measures.

First let me add to the exercises I before mentioned rowing, jumping, vaulting, and leaping. I am told that mowing acts very effectively to strengthen and harden the abdominal muscles, and I can well believe it; but as I have never mowed a square yard in my life, I cannot answer from experience. I should take some exception to mowing on the ground of its one-sidedness. For symmetrical development of the body, and for the increase of grace and lissomeness of movement, which I take to be much more important points than mere local development of strength, all exercises should admit of being interchanged from right to left. Thus the movements of fencing, though not conveniently altered in actual foil play (for every fencer will in actual contests put his best hand foremost), should be systematically repeated with the foil or a substitute for it in the left hand, and the right leg supporting the body's weight. So with sparring exercises, and others in which usually the sides of the body are differently employed. But in mowing one cannot well reverse the action, without having a scythe specially prepared for left-handed work. However, very few persons care to practise mowing, so this particular objection need not trouble us much.

There is one very trying exercise for the abdominal muscles which I would not recommend any except those already very strong-stomached to attempt. Sit on one of the parallel bars and carefully place both feet under the other; now steadily lower the body backwards, until you are lying horizontally athwart one of the bars and prevented only from going over by the hold of your feet under the other. Now steadily raise the body to the sitting position from which you started. This exercise may seem in effect much the same as one already given, in which sitting on a bed with legs horizontal and the feet placed under the lower bar of the foot-rail of the bed, you lower the body till it is horizontal and then raise it again—repeating as often as you may find convenient (not too often at first). But as a matter of fact you will find the parallel bar form of the exercise very much more trying than the other. Moreover on a bed if you feel unwilling to make a final rise from the horizontal position, all you have to do is to lie still and rest. But if on the parallel bars you lower yourself after you are nearly used up you cannot well avoid the effort necessary to come upright again. You certainly cannot rest comfortably across a horizontal bar, with so much of your weight on one side of it that only the catch of the feet under the other keeps you from falling over backwards. On the whole, I refrain from advising this particular exercise for hardening the abdominal muscles. When they are hard enough to stand it they are about as hard as you need them in ordinary life; and it should be remembered that exercise given unnecessarily to a set of muscles already well developed may be regarded in most cases as exercise unwisely withheld from muscles which need strengthening.

And now to consider those exercises which instead of giving the abdominal muscles energetic contractile work tend to stretch them and limber them up. These are especially desirable for all who lead a sedentary life; for the abdominal muscles get contracted and weakened in the sitting attitude, and especially in sitting over a desk. For this reason, and because there is no time so good for spinal exercises as the very time when undue rest resulting in the contraction of muscles has to be corrected, I give as many exercises as possible for stretching and limbering the waist muscles without apparatus or occasion to leave the room,—sitting-room, study, or even counting-house,—where the abdominal muscles are losing strength and activity.

It is a natural instinct after long sitting over a desk to extend the arms outwards upwards and backwards, throwing the shoulders back, and projecting the stomach a little forward. It is the movement seen in vigorous yawning; and many restrain it in themselves or check it in others as if it meant laziness. Probably if a merchant were to see half-a-dozen of his clerks simultaneously engaged in this natural and wholesome movement, he would be disposed to feel angry with them,—the action *is* so lazy looking. But if he knew what was good for them and therefore for him, so far from checking these movements, he would encourage his clerks to systematic stretching of this wholesome kind. To take off the lazy look he might invite them to get off their high stools, and standing well erect go through the backward stretching steadily and strongly several times in succession before resuming their sedentary work.

As a muscular exercise this stretching is excellent, and any one who has an artistic eye, and notes carefully from week to week the effect which it has on the figure of the chest, shoulders, and abdomen, will see that it is a beautifying exercise also.

Let us consider how the stretching, to be most effective, should be managed.

Stand well erect, with arms hanging straight down, and the feet somewhat apart to give a firmer stand. Draw a long breath, continuing the inspiration while the arms in the movement to be described are moving so as to expand and deepen the chest. Bring the arms from the sides steadily forwards, upwards, backwards, and then downwards, with the backs of the hands towards the ground. Breathe out now slowly, letting the arms sink still lower, but keeping them as close together as they can be brought in their backward position,—till they are hanging straight down as at first. Repeat the process four or five times, being careful to take as full a breath as possible in the first part of the stretching movement and to breathe out as fully as possible during the latter part.

Next slightly modify the exercise. Stand erect as before. Then, keeping the arms down incline the head and shoulders backwards as far as possible, arching the body forwards and hollowing the back, until the hands go as near as they can go to the heels. Recover slowly and steadily the erect position. Repeat this exercise half-a-dozen times,—or a dozen if you feel equal to it. If you give a little time and attention to this exercise every day you will not only find the abdominal muscles wonderfully improved, but that your lower limbs have felt the strain and benefited by the exercise.

On the stage you may see the extent to which, by such backward bending, the muscles of the abdomen may be stretched and made elastic. But it is not essential to your happiness, or the happiness of those around you, that you should be able to touch your heels with your head, or going a little farther that way, to bring your head forward between your feet and smile upon your friends from that comparatively low level. But though this, judged by Mr. Foster's rule, is not necessary or even desirable (for as Bishop Peter of Rum-ti-foo remarked, such contortions might pain your friends very much) yet the abdominal muscles cramped by much sitting over books and manuscripts, will be all the better for so much of this sort of exercise as may stretch and well loosen them.

There are many other exercises without apparatus or with only dumb-bells or clubs which have an excellent effect in the same direction.

In using clubs (Indian) for the backward swing, the ordinary way, and an excellent way for its particular end, is to keep the body rigidly upright. The work then tells chiefly on the chest, shoulders, and arms; in so far as it acts at all on the waist muscles it tends rather to harden and contract than to limber them. But now change the mode of swinging, letting the body yield as the clubs are swung forward overhead and backward,—so that as the clubs end their backward sweep the body is well arched forwards and the ends of the clubs almost strike the calves or even the heels. (But avoid actually striking, for while the blow will cause discomfort, it will not help the development of any particular set of muscles). Now after the full backward swing has thus been reached, swing the clubs well over the head to the front, making all the stretched muscles of the front of the waist take part in the beginning at least of the pull. Do not mind the shoulders and chest coming well forward as the clubs swing down past the feet and backwards. In fact throughout *this* club exercise give up the firm upright position which is essential in the ordinary form of the overhead swing. Make your body bend well forward as the clubs come down in front,—even so far if you like that the swing of the club backwards beyond the feet may be necessary to save you from toppling over forwards. Contrariwise let your abdomen come well forward as you send the clubs backwards over your head,—so far that there is an elastic backpull available from the abdominal muscles as you begin to bring the clubs up again.

This exercise has a splendid effect in correcting the defects of most of our English exercises. You will find the arms get less work than you might expect, though they are not idle. The shoulders are well worked; but the waist muscles, front and back, are those most benefited. (We will look after the side waist-muscles later on.) The hardest work of all, however, is generally done below the knee in this fine exercise.

The dumb-bells can be similarly used with a swaying body, bending forwards and arching backwards alternately. But they do not give the same pleasant sensation as well chosen Indian clubs (not too long). Growing lads in particular get much more benefit from the swaying exercise with the clubs than with the dumb-bells. Indeed, heavy dumb-bells are apt to check growth.

Blackie mentions rightly as good for the extension and limbering of the abdominal muscles,—"All work above the head, such as swinging an axe or sledge; putting up dumb-bells, especially when both hands go up together; swinging by the hand from a rope or a bar or pulling the body up till the chin touches the hands; standing with back to the pulley weights" (described in former papers), "and taking the handles in the hands and starting with them high over the head, then pushing the hands far out forward; standing two or more feet from the wall and placing the hands side by side against it almost as high up as your shoulders, then throwing the chest as far forward as possible," besides the work of special trades, such as ceiling work by the plasterer and painter, hauling down ropes by sailors, and so forth.

Sparring and fencing are both excellent exercises for the front abdominal muscles. You get the best exercise in this way, alone; because you can work systematically using right and left sides equally. The part of the work which tells most in the way we are considering is not the lunge in fencing or the delivery of the blow in sparring, but the recovery in the former and the drawing backward of head and shoulders in the latter. Quickness in all such movements is well worth acquiring, apart from mere muscular development.

A capital plan for sparring is to face a mirror too far from you to be struck, and to deliver your blow sharply in such a way that your fist hides whatever part of your image in the glass you may wish (as forehead, eye, nose, mouth, neck or chin); then recover sharply, bring the striking arm (elbow bent) down to your side, and throwing up the other sharply as if to ward a blow aimed at your face.

(To be continued.)

A YEAR'S WEATHER FORECASTS.

By John W. Staniforth.

PART II.—THE WEATHER.

The forecasts of the general state of the weather may be considered under the heads of rainfall, temperature, and humidity.

(*a*) *Rainfall.*—Occasionally rain is predicted in such definite terms as "rain at times," "some rain," "showery," but often it is only vaguely indicated by such nondescript terms as "changeable," or "unsettled." In analysing the forecasts for rain, I have only taken account of the former class, and I find that on 167 days rain or snow was confidently predicted. The rain or snow fell on 120 of these days, but failed to put in an appearance on the remaining 47. In addition, rain or snow fell on sixty days when the prophecy led us to expect neither.

(*b*) *Temperature.*—Changes in temperature are not often predicted, the office contenting itself with such terms as "cool" or "warm." Occasionally, however, a sudden change is accurately foretold, as on April 6 and 7, when the forecast for the 6th said "colder" and that for the 7th "warmer," which was exactly what happened, the thermometer falling as low as 30° F. on the 6th, but rapidly rising again on the 7th. Altogether 69 forecasts were made with regard to temperature, of which number 58 proved correct.

(*c*) *Humidity.*—Only twice (April 23 and May 16) did the office venture to predict a *dry* state of the atmosphere, and on the first occasion the relative humidity, as indicated by the wet and dry bulb thermometers, was 91 at noon and 100 at 6 p.m. On the second occasion, however, there *was* a minimum quantity of watery vapour in the air, the relative humidity being 57 at noon and 73 at 6 p.m. With the opposite condition the office was a little more fortunate, predicting fog, mist, or haze on 42 occasions and being correct on 24.

Taking these factors into consideration, together with such conditions as "dull," "fair on the whole," "cloudy," "thunder," &c., I valued the general weather forecasts (if I

may so call them) under the heads of correct (C^3), moderately correct (C^2), and incorrect (C^1). These letters were entered after the A's and B's, so that each day the full value of the forecast was ascertained and indicated by such formulæ as $A^4\ B^4\ C^3$ (maximum of correctness) $A^4\ B^3\ C^1$, &c. I may add that in no case was the forecast ever consulted before writing out the report—a plan which is, in my opinion, very necessary if an impartial test is to be made. In the following table is given the success achieved by these forecasts for each month of 1883.

	C^3	C^2	C^1
Jan.	15	7	5
Feb.	13	8	3
March	14	6	7
April	10	8	7
May	14	7	6
June	15	9	2
July	13	8	5
Aug.	12	7	8
Sept.	16	6	3
Oct.	16	8	3
Nov.	16	3	7
Dec.	15	7	3
Totals	169	84	59

Summarising these results, we find the office has been successful in nearly 70 per cent. of its forecasts of the wind's direction, in 60 per cent. of those relating to the wind's force, and in slightly less than 55 per cent. of prophecies of the general weather.

What is the conclusion to be drawn from these figures? It is, I think, that the office may fairly claim our confidence in its wind predictions, but cannot justly claim it for forecasts which are right in 55 per cent., doubtful in 25, and incorrect in 20.

WEEVILS.

By E. A. Butler, B.A., B.Sc.

AMONGST the great order Coleoptera, or beetles, there is no more destructive group than the weevils. They are vegetable-feeders, and some of them severely attack many of the objects of cultivation in this country, doing a considerable amount of damage, especially during their larval existence. Nearly 500 species have been recorded as belonging to the British Fauna, but these do not attain any great size; only one or two are over half an inch long, and the majority range from quarter-inch downwards, the smallest being only one-twenty-fourth-inch in length. Their shape is remarkable and characteristic. The head is produced into a snout or beak—technically, the *rostrum*—which is sometimes short and thick, but frequently long, slender, and arched, and in a few instances even as long as all the rest of the body together (Fig. 1). This apparatus which, because of its proceeding from between the eyes, looks like a nose, though it certainly does not perform the functions of such an organ, being used neither for breathing nor smelling, carries at its apex the organs of the mouth, which, throughout this group, are reduced to small dimensions, though similar in general plan to those of ladybirds, described in a former paper. It is obvious, therefore, that every particle of food taken by the insect has to traverse the whole length of the rostrum before it gets anywhere near that part of the alimentary canal where the stomach is situated. To the sides of the rostrum are attached the antennæ, composed of from nine to twelve joints, the basal one of which is generally, though not always, so much larger than the others as to form nearly half the length of the entire organ; the terminal part, consisting of some slender joints followed by a club or knob, can be bent at an angle to the basal joint, thus making what is called an elbowed antenna. The whole antenna can be folded up after the manner of a pocket-map, the terminal part being bent back upon the basal joint, and this again upon the rostrum, on each side of which a groove is excavated for its reception. The thorax is either like a truncated cone, or nearly globular, and has no sharp edge at the sides, as most beetles have. The elytra cover the greater part or the whole of the abdomen, and are always present, though in many cases there are no wings for them to protect. The whole body is often covered more or less closely with tiny scales, which, being variously coloured, white, yellow, red, black, green, and so on, add greatly to the beauty of the insect. Some few are clothed with frosted green scales, which glow and sparkle in the sunshine, and render the creatures dazzlingly beautiful. This beauty, when viewed by the aid of reflected light under the microscope, becomes a perfect blaze of splendour, which reaches its climax in the well-known exotic Diamond Beetle, a member of this section. The scales sometimes form a pattern on the elytra; they are much too small to be individually distinguished by the naked eye; but put the insect under the microscope, and that which before appeared simply a variegated design, is seen to be a lovely mosaic composed of an immense

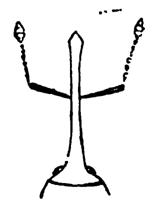


Fig. 1. Head of Nut-Weevil.

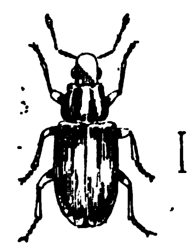


Fig. 2. Sitones lineatus.

number of pieces. And here, let me say, that the microscopical appearance of insects is so familiar to an entomologist, that the mental images he habitually forms of them are such as are suggested by this, and not by their ordinary appearance, and his descriptions of beauties of form and colour are apt to appear somewhat overdrawn to one who sees the things only in the insignificance in which they appear to the eye uninstructed by the revelations of the microscope. The scales are not very strongly attached, and the beetles, after having done a little knocking about the world, look decidedly the worse for wear; the dull-black or dark-brown of their bodies showing here and there, where the scales have been worn off, gives them a very "seedy" appearance, when compared with the dandified look of the freshly disclosed insect. The legs are stout and the feet broad, an arrangement which enables the creatures to take a firm foothold upon the vegetation on which they live.

A good notion of a weevil larva may be obtained from the fat, whitish, footless, wriggling maggots which often, unpleasantly enough, take the place of the kernel in nuts and filberts: these are the larvæ of the nut-weevil, concerning which more anon. The pupæ, as with beetles generally, show distinctly all the parts of the future insect, the beak being bent under and folded lengthwise along the breast.

Peas, beans, and other leguminous crops, such as clovers and trefoils, are very subject to the attacks of weevils. There is a genus called *Apion*, containing nearly eighty British species of tiny beetles, the largest only about $\frac{1}{8}$ in.

in length, which are something like animated notes of exclamation (!) minus the dot, and plus six straggling legs and a pair of antennæ which are not elbowed. They have also been not inaptly compared to pears and to pegtops, the "peg" being the rostrum. They are mostly black or dark blue, and several are destructive to leguminous plants, in the seeds of which sometimes the larvæ live. They are gregarious, and many occur in fields and amongst rank vegetation in immense numbers, though they are far too small to be noticed without being specially looked for.

Some species of the genus *Sitones* (Fig. 2), a much larger set of insects, attaining the length of nearly ¼ in., are still more hostile to the leguminous crops. Nineteen species of this genus have been detected in this country, but most of them are so much alike that none but an experienced entomologist would be able at all easily to separate them. They are of an ochreous or clay colour, and yet this is not so; they are really black, but the whole body is so closely covered with ochreous scales that no trace of the ground-colour can be seen, except after the insect has suffered somewhat severely from the vicissitudes of fortune, and has lost its ornaments. They are a great contrast to the slender-snouted Apions, for they are cylindrical in shape, and the rostrum is extremely short and almost as broad as the head. Contrary to what is usually the case, they seem to be much more harmful in the perfect than in the larval state. In a field of peas, the leaves of the plants are often seen jagged or scalloped at the edges; this is the work of these weevils, and sometimes they carry their depredations so far as to leave nothing but the mid-rib and its branches to testify to the former presence of a leaf. They treat many other plants in the same way, and when not engaged in the business of their life, they may be found at the roots of plants, under stones, clods of earth, &c. It is not easy to find them actually feeding, as they are wary insects, and, on the approach of an observer, will suddenly fall to the ground as if dead, where they easily escape detection. In spring, large numbers may frequently be seen in towns, crawling about our walls, especially where gardens are attached to the houses.

(*To be continued*).

ELECTRO-PLATING.

I.

By W. SLINGO.

THERE is, perhaps, in the whole range of electrical science no branch that is capable of offering greater or more varied attractions to the amateur or the student than that known as electro-chemistry, although, of course, in its deeper and more intricate paths there are theories and principles which demand much closer application than the amateur is, as a rule, able to give. Variety is itself a source of pleasure to the human mind, and renders gratifying that which would otherwise be looked upon as tedious and irksome, whilst there is also to be mentioned as an element of attraction the small amount of technical or purely scientific knowledge requisite to enable one to pursue the subject in its smoother paths. Nor, again, is the apparatus (without which the study of any branch of physical science must be extrememly monotonous) of an elaborate or expensive character. It is, however, essential, if good results are required, that the experimentalist's powers of observation should be called into play. Upon this a great deal depends. Especially on such points as the efficiency of any particular battery in use, the best relative constitution of the solutions, and the time taken to produce any required effect. It is no wonder that, with its many attractions, the first announcement of the effect of a current upon a sulphate of copper solution should have set the world a-going, repeating the announced experiments in a multitude of ways, which rapidly led up to most extensive business operations in the nobler metals as well as in copper. The almost universal practice of using plated instead of solid silver or gold articles is one which promises to extend very considerably, and that for many reasons; but perhaps the chiefest is one upon which those gentlemen who delight to go "a-burgling" can say most, as they have lost a very profitable source of income.

The subject of electro-chemistry is not by any means a new one, nor is that particular branch of it known under the general heading of "Electro-plating," which, for my purposes here, I shall use to embrace all those various applications of electricity in which a metal is deposited upon an "electrode" (a term used to indicate that portion of the solid conductor which is immersed in a liquid), whether that electrode be metal or otherwise. It is one of those subjects which seem to have been almost lost sight of in the general excitement produced by the re-introduction of the electric light, but it is one which, nevertheless, may be made to yield many a pleasant evening's amusement during the winter months, and, for that matter, during the summer months also.

The science of electro-chemistry might almost be styled the science of liquid electrical conductors, because, in the first place, the liquid must be a fair conductor, or the current will not traverse it, and therefore the liquid will not be affected; and in the second place, because there are very few liquids which do not suffer a chemical change on the passage of an electric current. The simplest form of change so produced is that resulting from the action of a current of electricity upon water, which is divided into its two constituent gases, oxygen and hydrogen. Although this is a commonly-known experiment, its lessons are but little known. It is a remarkable fact that electricity is the only form of force by which we can readily decompose water without giving the produced gases something else with which combination may take place, as happens when a piece of potassium or sodium is thrown on to water. In this case the water is decomposed by the metal, which combines with the oxygen and half the hydrogen, and releases the other half of hydrogen. An electric current effectually separates the two gases, and they may easily be collected if suitable apparatus is employed. While the recent extensive fire which destroyed several timber-stacks was raging a theory was started that the heat of the fire decomposed the water, and that the escaping inflammable hydrogen caught fire, and so "added fuel to the flames." I am not aware, however, that the theory received any measure of support. When an electric current decomposes a substance, the products are not necessarily equal in volume or in weight. Thus water, when decomposed between platinum electrodes, yields one volume of oxygen to two similar volumes of hydrogen, while the relative weights are as eight of the former to one of the latter. These results are governed by the proportions of the several elements which enter into the various combinations, and by the relative weights of the ultimate atoms of each element.

The decomposition of a liquid by the passage of an electric current is termed electrolysis, and is produced by dipping the wires or other conductors in connection with the battery into the liquid. It may happen that one of the wires or electrodes is a metal assailable by one of the products. Thus, if copper electrodes are used for the decomposition of water, the hydrogen may be col-

lected; but the oxygen combines with the copper, and forms an oxide of that metal. This fact is well worth remembering, because by its aid we may readily ascertain the previously unknown direction of a current flowing through a circuit. To do this, all that is necessary is to moisten the finger with saliva or any saline solution, and then place upon it the two copper wires of the broken circuit. Bubbles of gas will be seen to accumulate on one wire, which the other turns black, and it may be noticed that however many times we repeat the experiment the bubbles always appear on the wire in connection with the zinc pole of the battery, and these bubbles are of hydrogen gas. Consequently we may always rest assured that the positive pole is that one which oxidizes, and the current travels from it through the solution to the negative electrode. Where it is desired to collect oxygen, copper manifestly must not be used, but small pieces of platinum foil or wire instead; and similarly where chlorine is one of the products, platinum must be avoided, otherwise the platinum will combine with the chlorine, and platinum is 35s. per ounce. A very simple and instructive experiment may be made by passing a current through a solution of common salt (known chemically as sodic chloride, $Na\,Cl$) slightly coloured by litmus or indigo. The salt is separated into sodium and chlorine, the former forming with water caustic soda, and remaining in solution; the latter exercising the property, peculiar to chlorine and a few other substances, of bleaching the solution. The bleaching occurs in that portion of the solution surrounding the electrode in connecting with the positive pole of the battery, while the sodium appears at the electrode in connection with the negative or zinc pole of the battery. In this experiment the electrodes must be of carbon, because of the absence of chemical affinity between it and chlorine. If, again, a solution of potassic iodide (KI) be subjected to the passage of a current through it, it is decomposed, and the previously colourless solution is rendered brown in the vicinity of the positive electrode, due to the freed iodine; while the other constituent, potassium, is separated at the negative electrode, and, combining with the water, forms caustic potash, hydrogen being set free.

A somewhat similar, but perhaps more instructive experiment is to divide the electrolytic cell into two divisions by means of a piece of porous earthenware, or what will answer the purpose almost equally well, a few sheets of blotting-paper stitched together. Fill both with sodic sulphate solution, adding a few drops of litmus solution. To one division, that intended to contain the negative electrode, add two or three drops of sulphuric or hydrochloric acid, the effect of which is to redden the blue litmus solution. On the passage of the current the liquid surrounding the positive electrode becomes red, while that surrounding the negative electrode becomes blue. The reason of this is that the current decomposes the sulphate of sodium, the acid portion appearing at the positive electrode and the metal at the negative electrode. The acid portion (SO_4) combines with the hydrogen of the water, and so produces sulphuric acid ($SO_4\,H_2$) while the other constituent of the water, oxygen, escapes (the electrodes being of platinum). The sodium acting upon the water forms caustic soda (NaHO) which is a substance known as an alkali, and has the property of rendering red vegetable solutions blue, being the opposite of acids which redden blue solutions. By subsequently reversing the electrodes the colours may again be made to change places. One important point to be gathered from this article is that in all cases the metal travels with the current, that is to say, it is liberated at the negative electrode.

The experiments above enumerated are all of a simple nature, and may be performed with the aid of a current produced from two good cells, Bunsen, Grove, Bichromate, &c., of moderate size—say one quart capacity.

THE UNITY OF NATURE.*

THIS is a book concerning which the time-honoured dictum may be emphatically repeated: that whatever in it is new is not true; and whatever is true is not new. Certainly it would have rejoiced the heart of the old lady who was so lost in admiration of that Providence which always caused large rivers to run by cities—if she could only have kept awake over it. Doubtless, too (similar conditions being pre-supposed), it will be regarded as more or less convincing by every one who is previously in agreement with its noble author. Whether, though, it will carry conviction to the minds, or in the slightest degree alter the views, of that vastly preponderating number of scientific men who differ from him, we venture gravely to doubt. Professedly theological in its end and aim, its avowed purpose would exclude it from notice in these columns; but for the fact that its illustrations are, or profess to be, drawn from the most recent results of scientific research. It is, then, to the Duke's treatment of these that we must perforce confine our criticism. The work before us contains evidence of a good deal of desultory reading; and of the amassing of a large number of imperfectly appreciated facts, mainly in Natural, as contradistinguished from Physical, science. Where any of these seemed to lend countenance to the thesis of the author, they have been quoted. In the cases in which they obviously militate against the conclusions he wishes to establish, they are either calmly ignored or (what is more annoying to the reader who demands argument and not assertion) pooh-pooed. His very eclectic quotations from Max Müller will serve as well as anything to illustrate what we mean by this. The House of Lords were told, on a memorable occasion, that they were "up in a balloon." Whatever may have happened to his Peers, it is tolerably evident that the Duke of Argyll is still so soaring, and that from this supreme elevation he looks down with a kind of wondering pity on men of such inferior intellectual and ratiocinative powers as Lyell, Huxley, Tyndall, Herbert Spencer, G. H. Lewes, Lubbock, Christy, Lartet, Pouchet, Wallace, Pengelly, and, the greatest philosophical naturalist the world has yet seen, Darwin—the results of whose imperishable labours are, it seems, to be overborne and thrust aside in deference to the opinions of a Mr. Dawson, who is apparently the head of some college in Montreal. What boots it that there is a practical consensus of opinion among our chief living embryologists as to the significance of rudimentary organs, and of the sequence in the development of the fœtus? The Duke (pp. 37 *et seq.*) knows better. To admit that Reason exists in the lower animals would seriously militate against the views advanced; as would assuredly the confession that they possess anything equivalent to language: so both are, not disproved, but denied (pp. 92 and 108). That "Conscience" may have had its origin—to put the matter in its curtest form—in our surroundings, would be a conception which would utterly fail to fit in with the author's argument. So this idea is also contumeliously dismissed. It would be a curious subject for inquiry, how, on the principles developed on pp. 322, 323, &c., of the work before us, the fact is explicable that thousands of women who would almost die

* "The Unity of Nature." By the DUKE OF ARGYLL. (London: Alex. Strahan.) 1884.

with shame at the idea of raising their dresses four inches above their ankles in the presence of a strange man, will yet lie, in the most strenuous manner, with unimpaired cheerfulness, when any end is to be gained by it. Among other assertions (for nothing worthy of the name of argument is vouchsafed) we find the predication of the origin of the entire human race from a single pair (pp. 386 and 398), as also that of the practically unchanged physical geography of the earth since man's first appearance upon it. It happens curiously that, as we write, the news comes of the arrival of Professor Hull, the Irish Director of the Geological Survey, from Palestine with his party, who have been engaged in making a geological survey and map of the Holy Land. We refer to this here, because Professor Hull gives it as his deliberate opinion that, at the date of the traditional Hebrew exodus from Egypt, the Mediterranean and the Red Sea were in continuous connection! So much for the historical immutability of the present distribution of land and water, insisted on on p. 403. Bare mention will suffice of the very far-fetched explanation of animal worship given on pp. 493 and 494; and of the employment of that stalest of all polemical devices, the imputation of moral obliquity on p. 528 to all who differ from the opinions of the Duke. "I wish," said Sydney Smith, apropos of Lord Macaulay; "I wish that I were as cock-sure of anything as Tom Macaulay is of everything." In this respect, though probably in this alone, the mantle of the great Whig historian certainly seems to have fallen on the Duke's shoulders. Amid this "intolerable quantity of sack," it would be strange indeed if the "ha'porth of bread" were not somewhere to be found; and there is one portion of "The Unity of Nature" which presents a certain amount of freshness, and something approaching to logical treatment. We refer to the section in which the question of the savagery or civilisation of primæval man is discussed. Dealing with the subject of the existence of such races, our author adduces the present condition and geographical location of the Esquimaux, the Fuegians, the Bosjesmans, &c., as affording cogent proof that they are merely the degenerate descendants of higher and more civilised nations, from which their progenitors have in times past been driven by war or cognate causes; and that hence, so far from the earliest human inhabitants of the earth having been savages, they were probably in reality vastly higher in the social and intellectual scale than many of their descendants. It is this portion of the book which relieves it from the level of dulness that rests like a pall over the other parts of it. The argument, even if it can scarcely be pronounced convincing, exhibits a certain amount of ingenuity; and will probably be read with more or less pleasure and interest by many for whom other parts of the work will possess but scant attraction. That, however, as we began by saying, any one will be convinced by this latest essay of the Duke of Argyll's, who did not agree with him previously, we regard as a matter of the gravest doubt. With perverted ingenuity he has treated a subject of the highest and most enduring interest in a fashion so dull and dreary as to be almost inconceivable by any one who will not take the pains—which we have done—to wade sedulously and conscientiously through its 552 pages. That, though, we may give it all the praise to which it is honestly entitled, we may add, in conclusion, that it is followed by a really good index.

THE BRITISH MUSEUM.—The Government have declined for the present to provide funds for lighting the British Museum by electric lamps, so that the keeping open of the building after dark has been deferred.

THE MORALITY OF HAPPINESS.

By THOMAS FOSTER.

CARE FOR SELF AS A DUTY.

(*Continued from page* 123.)

IT will perhaps be sufficient in response to numerous inquiries addressed to me respecting the supposed religious bearing of these papers to remark that they are not intended to have any religious bearing whatsoever. I am simply inquiring what are the rules of conduct suggested when each person takes as his guiding principle the increase of the happiness of those around, an expression which must be taken as including himself in the same somewhat Hibernian sense in which Milton included Adam among "those since born, his sons." I may add that nearly all the letters addressed to me have been interesting, and some have been singularly well-reasoned,—all utterly unlike the rather spiteful and very silly letters I referred to in a footnote to my last paper. Yet I cannot suffer the religious element to be imported into the subject,—no matter how courteously or kindly the thing may be done. I have just the same objection to see the question of the evolution of conduct considered from that side, which the student of astronomy or geology has against dealing with the objections and difficulties raised by those who seem always to suspect that under the teachings of God's work, the universe, there may lie some grievous deceptions if not some monstrous falsehoods. If my reasoning is bad, it can be met and overcome on its own ground.

I may, however, make this general remark with regard to all systems of morality whatsoever, including those which have come before men in company with religious teachings. Without a single exception every one of these systems includes—and professes to include—features suitable to the special time and the special place when and where it was propounded. How much of any system may thus be regarded as local or temporary or both may be a moot point; but that some of each system is of that sort is absolutely certain. "Because of the hardness" of men's hearts the Mosaic system for instance had certain rules; and because of the weakness of their hearts (who can doubt it?) the system which replaced that of Moses had certain other rules. The same is true of every system of conduct ever propounded. We may believe the rule sound and good in its own time and place, "Whosoever shall smite you on the right cheek turn to him the other also," and "If any man will sue thee at the law and take away thy coat, let him have thy cloak also." A man may believe these rules to be more than sound and good, to be of divine origin,—yet recognise that in our own time, and here, in Europe or America, the rules would work ill. He who so taught recognised in the same way that other rules which had been good in their time had lost their virtue with changing manners. He knew *where* it is written, "Thou shalt give life for life, eye for eye, tooth for tooth," and so on; yet he only quoted these Scripture teachings to correct them,—"But *I* say unto you, that ye resist not evil, but whosoever," &c. When he thus corrected what was "said by them of old time," he did not show disrespect—whatever the Scribes and Pharisees tried to make out—for the teachers of old time, whose words he read and expounded. He knew that "old times were changed," and therefore old manners and morals gone. He said "Suffer little children to come unto me," and loved them, *not* teaching—as had seemed more convenient and was (let us believe) better, in earlier days—that the child would be spoiled unless diligently belaboured with the rod.

These times and the races and nations now most prominent on the earth are even more unlike the community in Palestine nineteen centuries ago, than that community was unlike the Jewish people in the days of the more ancient lawgiver. The opponents of evolution may prefer to believe that the human race has been stereotyped; but facts are a little against them. And even if we admitted the imagined fixedness of the human race for nineteen centuries, they would still have to explain the contradiction between two systems for both of which they find the same authority. Of course there is no real or at least no necessary contradiction. Grant the human race to be what we know it to be, a constantly developing family, and the contradiction vanishes,—we simply learn that what is best for one time is not best for another, even among one and the same people; how much more then must the best rules of conduct vary when different peoples as well as different times are considered.

All this however is a digression, which should have been unnecessary but has in a sense been forced on me by the misapprehensions of many well-meaning critics, [and a few who are not well-meaning at all, but of the Honeythunder order, teaching the law of love by reviling and worse].

The duty which each man owes to himself in regard to the maintenance of his health, the development of his powers and so forth, which becomes a duty to others when regarded with reference to those more immediately around him or dependent upon him, and is still manifestly a duty in relation to others where the advancement of the general well-being so far as he can influence it, is considered, has another aspect when considered in reference to those classes (D and E of our list, p. 122) whose encouragement or increase would be injurious to the body social. It is not only essential to the evolution of conduct in the right direction that those who may be classed as "men of good-will"* should increase relatively in number and influence, but also that those who are either absolutely men of ill-will, or are so far not of good-will that they disregard the well-being of others, should be checked and discouraged.

This requirement for the evolution of the more altruistic kind of conduct involves in many cases—as a duty—conduct of a kind which the few real members of class A and the many members of class C who speak of themselves as belonging to class A—regard as self-assertive. It becomes a duty, when the matter is viewed in this light, to assert just rights and resist wrongful claims. For, every act of carelessness or self-neglect in such matters tends to the encouragement of the less valuable or noxious classes which profit by it. It may be that to uphold just claims or resist wrongdoing may be less comfortable than to give way. In such a case the duty becomes an altruistic one, however egoistic the action based on the consideration of such duty may appear. But in a number of cases the claim upheld may be well worth upholding in itself, the wrong resisted may involve gross injury. In such a case the care of a personal right or the resistance of a wrong is, in itself, egoistic. Yet may it well be that the person concerned may esteem it better to give up the claim or to yield to the wrong, until he recognises that the idea of self-sacrifice, however beautiful in itself, may involve a far-reaching wrong to the better members of the body social.

* It may not be generally known outside the Roman Catholic community that the message rendered in the authorised version of the New Testament "Peace and good-will towards men," is otherwise rendered "Peace to men of good-will." The revised version reads "Peace among men in whom He is well pleased," which would in effect be nearer the Roman version.

We touch here on considerations which are in question every day, almost every hour, of our lives.

Consider home-life for example. In nearly every home there are those who are disposed to take unfair advantage of the rest; and they are far better restrained by the quiet resistance of their attempts than in any other way—certainly far better than by yielding, continued till nothing but the anger roused by some attempt, more barefaced than the rest, moves to resistance. We see this especially exemplified in the families of careless parents—unselfish perhaps in a sense, but really negligent of their duties. It has been said for this reason that unselfish parents have commonly selfish children, which seems contrary to the law of heredity, but illustrates rather the natural influence of defective training. The fact really is that the children of selfish parents are as a rule more selfish in character than those of the unselfish; they grow up to be as unpleasant in their ways as the children of careless, unwatchful parents; and their unpleasantness is more apt to be permanent. Yet the unchecked ways of children whose parents yield unwisely to them, illustrate well on a small scale (even though happily the mischief is often transient)—how the assertion of just claims, and the restraint of wrong-doing, involve a form of egoism which must be regarded as a duty.

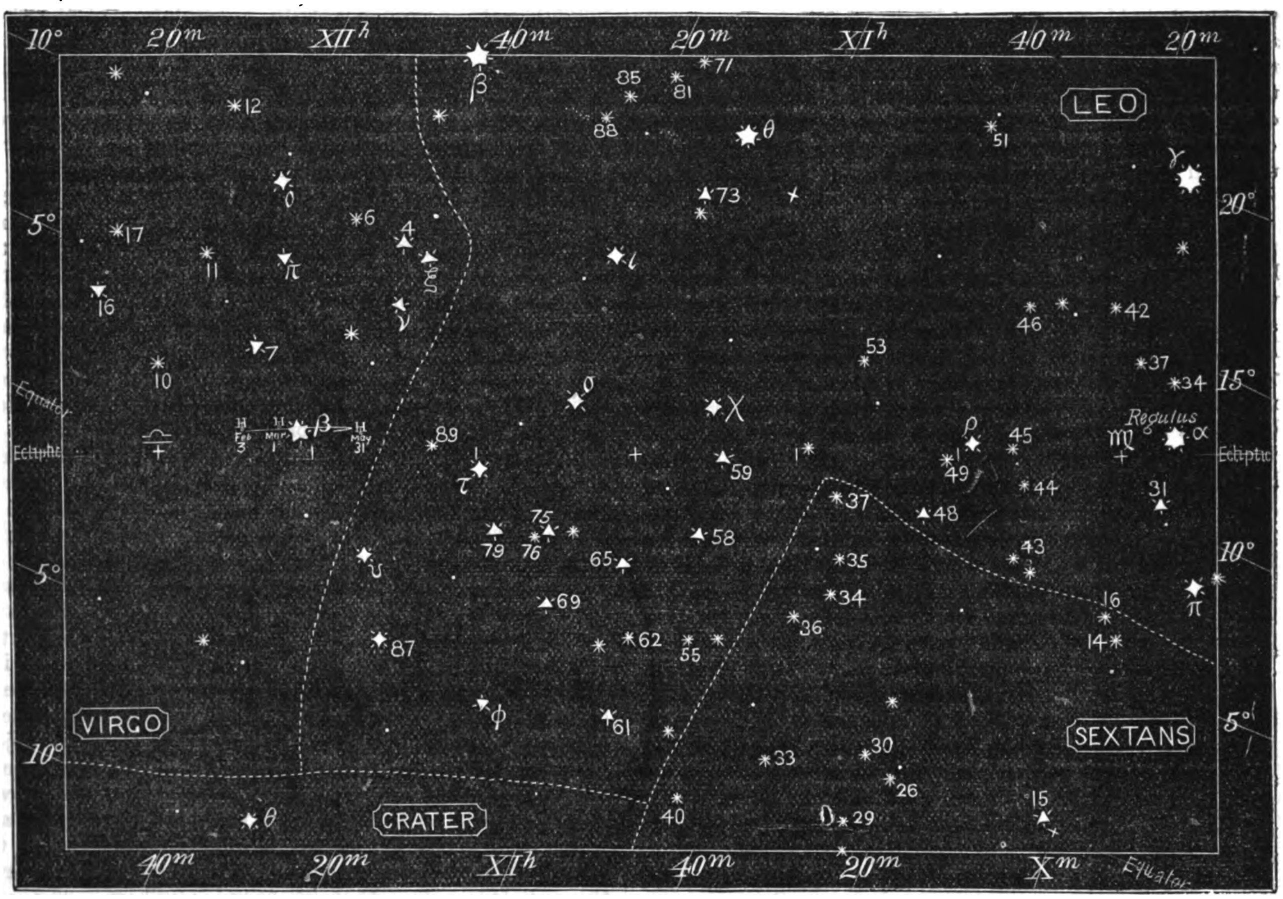
In life outside the family, we constantly find the duty of resisting evil presenting itself in apparently egoistic aspect. In hundreds of ways the members of class C show their readiness to become members of class D and members of class D to develop their unpleasant ways. The adoption of considerate habits and care for the just claims of others in all the multitudinous details of our daily life, constantly lead to attempts by the selfish and obnoxious to take advantage of what they regard as mere weakness of disposition. In such cases while it is by no means desirable to give up ways which are in themselves essential to the well-being of the society of which we form part, we must—as a duty—resist the encroachments of objectionable persons,—not the less that the matter insisted upon is one to which we attach importance, so that our firmness has its egoistic aspect. Men are but children of a larger growth, and there is no surer or better way of eliminating at least the grosser forms of selfishness than by so resisting unjust claims that they—simply fail. This is the appropriate punishment,—akin to that which Mr. Spencer regards (most justly in my opinion) as the only proper form of punishment for children, viz, punishment which is the direct consequence of ill conduct. Of course, it will happen that mere resistance of a wrong may bring definite punishment—directly or indirectly—to the wrong-doer: but (apart from such cases, in which we have to ask whether justice may not need to be tempered with mercy) all I would insist on is that the selfish, grasping, oppressive members of the body social, should be so resisted that, whenever it is possible, they fail of their unfair purpose.

The rule applies in small matters as well as great. Mr. Spencer himself notes (though it is when dealing with selfishness specifically) a case of not infrequent occurrence, and perhaps of a trifling enough kind,—the acted falsehood of railway passengers who, by dispersed coats, make a traveller believe that all the seats in a compartment are taken when they are not. Here the detection and resistance of the attempted wrong, contemptible as it is, may excite some sense of shame in the wrong-doers, though conceivably not (for such wrong-doers are of a shameless sort); but the defeat of their purpose will at the least involve disappointment and serve as a discouragement from such attempts in the future. Of course a very zealous opponent of the obnoxious section of society might not be content with what I here advocate

as the simple line of duty in such cases. He might (as an earnest opponent of evil did—rather harshly I think—the other day), take on himself to punish as well as to resist evil: and having been met with the customary falsehood as to some article deposited in a vacant seat, might pitch it out of window with the remark that he would be responsible to the real owner when he appeared. But this is going beyond the strict line of duty in such matters.

It will appear manifest, I think, on careful consideration of the matter by any one who notes, for a few days or even hours, the course of events around him in his family and in society, that he who neglects to defend his own rights against the encroachments of class D as well as of class E, and of class C as well as of class D, fails as clearly in his duty to the social body, as the parent who overlooks selfish and unruly conduct in his children. And just as the children themselves whose training is thus neglected have really just reason did they but know what is good for them to complain of such mistaken kindness, so even the more selfish (all but the members of class E) have no less reason than the unselfish, did they but know their own interests, to desire that considerate but firm and self-regardful conduct should prevail throughout the body social.

(*To be continued.*)



THE ZODIACAL SIGN FOR FEBRUARY,

WITH THE PATH OF URANUS IN 1884.

WE give this week the sign of the Zodiac—to wit Virgo, which is now dominant in the midst of night, and the path therein of Uranus. The constellations now occupying this sign where once Virgo reigned supreme, are Leo and the head of Virgo.

ELEPHANTIASIS AND ELECTRICITY.—An interesting communication on the treatment and cure of elephantiasis amongst Arabs by Doctors Moncorvo and Silva Aranjo has been presented to the French Academy of Sciences by M. Gosselin. The cure consists in decomposing the tumid swelling of the limbs, known as elephantiasis, by means of electrolysis, but at the same the general health of the patient is also treated hydropathically, that is to say, by the cold-water cure, sea-baths, tincture of iodine, iodide of iron, arsenic, and other tonics. These medicines are intended to renovate the constitution, but are not of themselves sufficient to reduce the tumours. Electropathy, however, applied as soon as possible after the first manifestation, checks, and ultimately cures it. The cure is generally perfect, and takes place at the end of a few days in some cases; but if the elephantiasis is of long standing the cure is also a long process, and must be accompanied by proper medicines. The electrolysis is effected both by continuous and interrupted currents sent through the tumid swelling.—*Engineering.*

Gossip.

THE *Spectator* considers the views recently expressed by Professor Tyndall about political matters erroneous, ("and so," by the way, "does this doggrel bard"); but is that a reason for making rude jests about that eminent man of science? That because a man has shown exceptional power in scientific research his judgment about matters outside science is not therefore necessarily sound may be true enough; but it is going rather too far to assert that therefore his judgment is necessarily unsound, and going a great deal too far too imply that therefore the man of science makes himself ridiculous by expressing an opinion at all. On the whole his opinion is at least as likely to be correct as that of the average writer of newspaper leaders. But then the public seem to think that every leading article in the daily and weekly papers is written in solemn conclave of all the staff, and that the staff includes several members of the Government, three or four generals, an admiral or two, and a fair sprinkling of bishops.

THE same sort of treatment has been extended to a literary man—Mr. Sala. When he writes in a daily paper without his name appearing, readers regard what he says as the utterances of that mysterious entity, a newspaper. But when he writes over his own name the admirable weekly "Echoes" in the *Illustrated London News*, he is regarded as fair game for the foolish folk who suppose that because a man has shown marked skill in one subject he must therefore necessarily be not worth listening to when he speaks about any other.

THE last polite thing said to him was that as his name has a foreign sound he is not of purely English blood, and "were my will paramount" wrote a solemn idiot, none but those of purely English blood should express an opinion about English matters,—which would be rather hard on "the queen and all the royal family."

THE red afterglow that has caused so much discussion among philosophers is now explained by a correspondent of the *Scientific American*, who asserts that the phenomenon is due to the red spot from the planet Jupiter. This great rosy cloud disappeared several months ago from the atmosphere of Jupiter, has had just time, according to this correspondent, to travel to our earth, and is now hovering over us, causing the ruby colouring of our skies night and morning. Nobody ever has or will be able to prove that this is not the fact; therefore, it must be true, says the correspondent. The question is settled; it is useless to talk further about cosmic dust, Java ashes, or aqueous vapour.

IT is stated, says the *Electrician*, that the *Great Eastern* has been purchased by a firm who intend to use her as a coal-hulk at Gibraltar. Perhaps it is better that she should be made use of than that her bones should rot in Milford Harbour. But what a come-down for a vessel which laid the first Atlantic cable! She has in her lifetime played many parts, and, after all, perhaps the last will not be the least useful.

A TIME CORRECTOR.—Mr. Latimer Clark, by his recent study of the transit instrument and the publication of his transit tables for 1884, has given private individuals a means of getting accurate time from the stars, though they have no acquaintance with astronomy. The transit instrument required is a very simple one, easily used, and the tables save all calculation. Another useful device to serve the same end is the "solar regulator" of M. Corneloup, a French clockmaker. This is really a portable sun-dial, which can be carried on the person and set up anywhere. A compass enables it to be placed in the true horizontal position, and a graduated sector with clamp, carried by an upright support, enables the hand or style to be set at the proper inclination, which should correspond with the latitude of the place. Noon is shown by a ray of sunlight shining through a hole in the style, and falling on the noon line drawn beneath it.—*Engineering*.

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.—In accordance with a resolution of the Committee of Section A (Mathematical and Physical Science), passed at the meeting in Southport, 1883, the organising committee of the Section have selected the following subjects for special discussions during the meeting in Montreal (1884):—1. On Friday, Aug. 29: "The seat of the electromotive forces in the voltaic cell." 2. On Monday, Sept. 1: "The connection of sun-spots with terrestrial phenomena." The Organising Committee hereby invite mathematicians and physicists to co-operate with them in sustaining the discussions by contributing original papers or oral communications bearing on the selected subjects. In order that the best arrangements may be made, the names of those who propose to read papers or are willing to take part in the oral discussions should be sent not later than June 1, 1884, to the Secretaries of Section A, British Association, 22, Albemarle-street, London, W. No paper should in reading occupy more than fifteen minutes, and no speech more than ten minutes. Communications on other subjects in mathematical and physical science will as heretofore be cordially welcomed, and of these notice should be sent to the Secretaries, as above, not later than July 1, 1884.

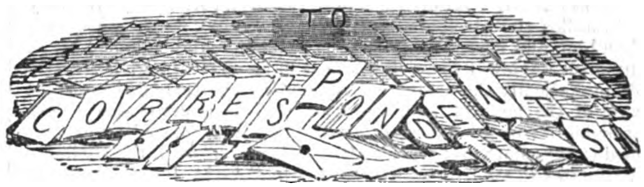
THE SUN'S ATMOSPHERE.—In considering solar surroundings we must set on one side the notion—which still seems to underlie the reasonings of many on this subject—that we have ordinary atmospheric conditions to deal with. Whatever elements exist around the sun either above or below that visible surface which we call the photosphere, they cannot possibly be in such a condition as to be subject to those laws of gaseous pressure which we recognise in gases at ordinary temperatures. This may easily be shown. Let us suppose an atmosphere of some gas around the sun which at a given height, say a hundred miles below the visible surface, has one-hundredth of the density of air at the sea level, or—to take round numbers—say one-100,000th of the density of water. Then were the laws of gaseous pressure as they exist in our own air prevalent in the sun, the density of this hypothetical gas would be doubled about nine times, or increased more than 500 times a mile nearer the sun's centre, 500 times 500 times or 250,000 times a mile lower down, where it would be therefore equal to $2\frac{1}{2}$ times the density of water,—or considerably greater than the density of the sun. Now above and below the visible surface of the sun that diffused complex envelope extends for thousands of miles which we are accustomed to regard as a complex atmosphere; and it would be absurd to admit the possibility that within a range of only a few miles a density exceeding the average density of the sun's whole globe would be acquired by an atmospheric gas much rarer than hydrogen at the sea level. Either then the envelope is not in the condition which we call gaseous, or the behaviour of such gases at or near the surface of the sun is quite unlike that which they display under such conditions as we can experiment upon on this earth.—R. P.

THE FACE OF THE SKY.

From March 14 to March 28.

By F.R.A.S.

THE sun continues to exhibit fine individual spots and groups of spots, and should be examined with the telescope on every clear day. The zodiacal light may be looked for in the west after sunset. The night sky is delineated on Map III. of "The Stars in their Seasons." Minima of Algol (id. Map. I) occur 28 minutes after midnight on the 16th, and at 9h. 17m. p.m. on the 19th. Mercury is a morning star during the next fourteen days, but is practically invisible. He comes into superior conjunction with the sun; in other words, passes behind him at the end of the month, and is too close to him to be seen. Venus continues to increase in brilliancy as an evening star, and is a beautiful object in the western heavens. It is nearly half-past 10 o'clock at night towards the end of the next fortnight, before she sets, which she does in the NW by W point of the horizon. Mars is visible during all the working hours of the night, but is best seen between the hours of 8 and 10. His angular diameter has now perceptibly diminished, and he appears but a small object as contrasted with Jupiter. The Zodiacal Map on p. 70 shows his path in Cancer during the period covered by our notes. Jupiter may be also well seen during the greater part of the night, but should be looked at as soon after dark as possible to be caught in his most favourable position for observation. His angular diameter has also decreased since January, but the change is less apparent than in the case of his neighbour in the sky. His pendulum-like path in Gemini and Cancer is depicted in the Zodiacal Map on p. 40. The phenomena exhibited by his satellites before 1 p.m. during the next fourteen days are both numerous and interesting. To-morrow evening (the 15th) the Shadow of Satellite II. will enter on to his disc at 7h. 24m. Satellite II. itself will pass off his opposite limb at 8h. 5m., followed by the shadow at 10h. 19m. Analogous phenomena of Satellite I. will occur subsequently, but after our prescribed hour. On the 16th Satellite III. will be occulted at 7h. 47m., as will Satellite I. at 10h. 49m. Satellite III. will re-appear from occultation at 11h. 20m. p.m.; only, however, to suffer eclipse at 12h. 24m. 7s. On the 17th the transit of Satellite I. will begin at 8h. 8m. p.m.; that of its shadow at 9h. 17m. The Satellite will leave the planet at 10h. 28m.; the shadow it casts at 11h. 37m. p.m. On the 18th Satellite I. will re-appear from eclipse at 8h. 43m. 27s. p.m.; as will Satellite IV. on the 21st. at 8h. 22m. 19s. This last-named re-appearance should be looked for by every possessor of an accurate clock, whose rate and error are known, for comparison with the predicted time of its occurrence. On the 22nd Satellite II. will enter into Jupiter's face at 7h. 39m. p.m.; followed by its shadow at 10h. 1m. The egress of the satellite happens at 10h. 34m.; that of its shadow at 56m. after midnight. On the 23rd, Satellite III. will be occulted at 11h. 30m.; as will Satellite I. at 12h. 40m. p.m. On the 24th, Satellite II. will reappear from eclipse at 7h. 56m. 6s. p.m. Then the transit of Satellite I. will begin at 9h. 59m.; as will that of its shadow at 11h. 11m. The satellite quits Jupiter's opposite limb 19m. after midnight; the shadow not until half-past 1. On the 25th, Satellite I. reappears from eclipse at 10h. 38m. 49s. p.m. On the 26th, the egress of the shadow of this same satellite takes place at 8 o'clock; and, finally, on the night of the 27th, the shadow of Satellite III. passes off the face of the planet at 9h. 59m. Saturn is now rapidly approaching the west, and must be looked for as soon as ever the twilight deepens sufficiently for him to be seen. He sets about half an hour after midnight to-night, and between 11 and 12 o'clock by the 28th. Saturn continues to the west of ϵ Tauri, the upper star is the Hyades ("The Stars in their Seasons," Map I.) Uranus is now getting very close to β Virginis ("The Stars in their Seasons," Map V.). In fact, he will be only some 4¼′ north of that star on the night of the 28th. He comes into opposition to the sun at 6 a.m. on the 16th, and is visible all night long. Neptune is invisible. The moon is 16·7 days old at noon to-day; and, of course, 28·7 days old at the same hour on the 26th. Then on the 27th at noon her age is 0·3 day; and quite evidently 1·3 days 24 hours later. No occultations of fixed stars occur during the next fortnight. The moon is in Virgo to-day and to-night, but at 10 o'clock to-morrow morning (the 15th) passes into Libra. She continues in this constellation until noon on Monday, the 17th, when she enters the narrow northern prolongation of Scorpio. She takes ten hours to cross this, and then emerges in Ophiuchus. This she quits for Sagittarius at 7 p.m. on the 19th. She occupies until 8 a.m. on the 22nd in travelling through Sagittarius, at which hour she enters the north-western part of Capricornus; which she quits at midnight for Aquarius. There she remains until 6 p.m. on the 25th, when she crosses into Pisces. Her journey through this great constellation is not completed until 4 p.m. on the 28th, when she passes the boundary into Aries. She is in Aries when these notes terminate.



"Let Knowledge grow from more to more."—Alfred Tennyson.

Only a small proportion of Letters received can possibly be inserted. Correspondents must not be offended, therefore, should their letters not appear.

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MR. SPENCER AND THE *EDINBURGH REVIEW.*

[1136]—You ask me to "condescend" to answer your complaint that I have attributed to Mr. Spencer "an absurd opinion about the Almighty contrary to the plain meaning of his words." I will, when you tell me what you take to be his plain meaning about the Almighty, consistent with his language throughout the book. Mr. F. Harrison has told us very plainly, indeed, in the present number of the *Nineteenth Century*, what he takes it to be, and intimates that Mr. Spencer had better not have left it so hazy—at least in language. And Mr. Harrison looks at him from a very opposite pole to mine. I referred to several passages in the "First Principles," which, with sundry others, at pages 161, 169, 170, 552, and 555, &c., prove that he attributes no action at all to his "Unknowable, Unconditioned," &c., except making or maintaining Persistence Force, which "works in us," and is "the Ultimate of Ultimates" and several more tremendous things; and also saying that it makes no practical difference whether that Unknowable made or is itself that Ultimatum; wherein I admit that he is right, if that is all that it does. Now, then, please, let us have your "plainly worded and exact description" of his plain meaning, and we shall know where we are, and what I have to answer.

Axioms.—I have not the least objection to your proving that the Greek philosophers did not know Greek enough to see that they had no business to use ἀξίωμα for only self-evident or necessary truths, not depending on experience; and that the Greek and English dictionaries are equally wrong in so translating it and its merely English form; and that what they ought to have used it for was "all truth that is worthy (ἄξιος) to be received": that is, all truth, and all experimental results, and all the laws of nature, old and new. But, on the whole, I have a preference for using words in their "agreed senses"; and no less a person than Professor Huxley says, in his "Hume," that it is mere beating of the air for arguers not to do so. I thoroughly agree with him in that, though I have seen him charged with conspicuously breaking his own rule in that very same book; but that is nothing. If a great man may not set up his own vocabulary and use it as long as is convenient, and then quietly slide back upon the old one, this would no longer be a free country, and a great deal of new philosophy would be nowhere. I only showed that Mr. Spencer largely availed himself of that privilege, not only as to axioms and postulates, but many other words. I cannot write a great part of the review over again here to prove any of these things, and, if I have not shown them already to those who choose to see, I should not be more successful here. The Edinburgh Reviewer.

[On the first part of the Reviewer's letter, I simply note that to ask me what Mr. Spencer understands by what (with others whose opinion has been thought of some weight) he admits to be not understandable, is a convenient way of putting off an act of justice till the Greek Kalends. Mr. Spencer expresses in his own way (in the words the Edinburgh Reviewer finds so tremendous) very much what the Apostle of the Gentiles expressed in his, when he said "How *unsearchable* are God's judgments and His ways *past finding out!* For *who hath known* the mind of the Lord? or who hath been

his counsellor? Or *who hath first given to Him, and it shall be recompensed to Him again?* For *of Him and through Him and to Him are all things.*" If Paul does not here say of the Almighty (the very word implies the attribute of Persistent Force) what Elihu said before him, and what Mr. Spencer is so blamed and ridiculed by an Edinburgh Reviewer for saying, words have no meaning. Distinctly and emphatically the comparatively modern apostle and the ancient writer of the book of Job, speak of God as Unknowable ("unsearchable," "past finding out," "who hath known, &c."), unconditioned ("Who hath first given to Him, His ways," &c.), the Ultimate of Ultimates ("Of Him and through Him," &c.). If this is not the plain meaning of Mr. Herbert Spencer, neither is it of the older writers. The Edinburgh Reviewer should have begun his unfair attacks earlier and directed them elsewhere.

With regard to the word Axiom, the Edinburgh Reviewer conveniently begs the whole question, and (as conveniently) overlooks what I originally said—that Newton used the word axiom in the sense of "a fact or law established by experience, and (so) known to be worthy of acceptance,"—as distinguished of course from facts or laws established by reasoning. This includes those axioms which the careless regard as the only kind of axioms,—self-evident propositions; for their self-evidence arises only from the commonness of the experience. It also includes the meaning which some careless makers of dictionaries have omitted to notice,—those laws or general principles which though not self-evident have been established by experience, and do not admit of being demonstrated, but must be used as the basis of demonstrations. They might be defined as "laws which have to be admitted without proof," and explained as "laws which are either self-evident or established by universal experience." No one that I have ever heard of except the Edinburgh Reviewer has ever thought of regarding the Laws of Motion as self-evident.

Here follow a few extracts which may interest an Edinburgh Reviewer:—

AXIOM: a word derived from the Greek ἀξίωμα, which is formed from the Greek verb ἀξιόω, *to think worthy of* and hence to *desire* or *demand*. " . . . ." It is usual to define an axiom *as a self-evident proposition*; but this . . . . is not a good definition." . . . . "A self-evident proposition as such ought not to be called an axiom."—De Morgan in the *Penny Cyclopædia*.

AXIOMS: the fundamental elements or acceptances of science . . . . *Propositions that result from universal experience* in the elementary physical sciences, such as the sciences of motion and force must be accepted as axioms.—Author of the article Axioms (Qy. Sir W. Thomson) in Nichol's "Cyclopædia of the Physical Sciences."

ἀξίωμα, That which is thought fit; a decision.—Liddell & Scott.

AXIOMS.—"The general principles which are necessary to knowledge are *axioms*.—Aristotle, Anal. Post, i. 2. "It is the office of *observation* to supply principles in each subject." Aristotle, Anal. Prior., i. 30.

AXIOMS.—"Aristotle and other ancient philosophers not only asserted in the most pointed manner that all our knowledge must begin from experience, but also stated in language much resembling the habitual language of the most modern schools of philosophy, that particular facts must be *collected*; that from these general principles must be obtained by *induction*; and that these principles, when of the most general kind, are *axioms*."—Dr. Whewell's "History of the Inductive Sciences," vol. i., p. 75.

INNATE PRINCIPLES, AXIOMS, OR MAXIMS.—"It is an established principle amongst some men, that there are in the understanding certain innate principles, some primary notions, *κοιναὶ ἔννοιαι*, characters, as it were, stamped upon the mind of man which the soul receives in its very first being, and brings into the world with it: it would be sufficient to convince unprejudiced readers of the falsehood of this supposition if I should only show how men, barely by the use of their natural faculties, may attain to all the knowledge they have, without the help of any innate principles, and may arrive at certainty, without any such original notions or principles"—with much more showing how axioms or maxims are established by experience—"discovered by the application of those faculties that were fitted by nature to receive and judge of them when duly employed about them."—Locke on "The Understanding."

AXIOM.—"An established principle in some art or science which, though not a necessary truth, is generally received."—Webster's "Dictionary."

SIR ISAAC NEWTON ON THE AXIOMS OR LAWS OF MOTION. "Hitherto I have laid down *such principles as have been received by mathematicians and are confirmed by abundance of experiments*. By the two first laws and the two first corollaries, Galileo discovered that the descent of bodies observed the duplicate ratio of the time, and that the motion of projectiles was in the curve of a parabola, *experience agreeing with both*, unless so far as these motions are a ittle retarded by the resistance of the air. On the same laws and corollaries depend those things which have been demonstrated concerning the times of the vibration of pendulums, and are *confirmed by daily experiments* of pendulum clocks." Then follow five or six pages of experiments by which Newton himself, following a line of experiment "begun by Sir Christopher Wren, Dr. Wallace, and Mr. Huygens, the greatest geometers of our times," established the third law. "And thus," he says, after the first part of his series of experiments, "the third law, so far as it regards percussions and reflexions, is proved by *a theory exactly agreeing with experience*." "In attractions," he proceeds, "I briefly demonstrate the thing after this manner";—then follow the experiments and reasonings. He concludes with the remark that he wished "to show *by those examples* the great extent and certainty of the third law of motion."

This shows, (what indeed is self-evident) that Newton did not regard the laws of motion as self-evident propositions. The extracts preceding the last show that he was nevertheless perfectly justified as well by the ancient as by the modern use of the word, in calling them *axioms*.

I do not hold myself bound to give an Edinburgh Reviewer further opportunities to repeat (which is all he does) his unfair and misleading attacks on Mr. Spencer. I was moved by his injustice to the comments I made, which he has not met in any way. I believe the stroke of the publishers in advertising Mr. Spencer's work with the Edinburgh Reviewer's most acrimonious sentence appended as if it were a laudatory criticism, was almost as good a way of responding as the case admitted. The review however homicidal in intent proved but suicidal in effect: it has gone far to destroy the attack it was intended to sustain.

RICHARD A. PROCTOR.

"THE NEW PRINCIPIA."

[1137]—In your impression of to-day you assert that I cannot see this, that, and the other, all of which I do see very plainly; and in reply to one set of arguments you deal out a quantity of irrelevant elementary information which is to be found in every text-book on the subject. I am well acquainted with Sir Edmund Beckett's "Astronomy without Mathematics," but as far as my ideas are concerned, it is a work quite beside the mark. Grant Sir Isaac Newton's premisses and his conclusions are irresistible; but what I dispute is not his demonstrations but his assumptions. For instance, when the earth's motion is accelerated in perihelion, Newton *assumes* [!!, R. P.] that the increased speed developes an increased centrifugal tendency which "grows as the square of the velocity." This would be the fact in relation to two bodies mechanically united, one compelled to revolve round the other; but I contend that as an explanation of the movements of the planets, which are not mechanically attached, it is an arbitrary surmise and an "impossible" supposition. What Newton mistook for centrifugal force, under the circumstances, is in fact, according to my theory, a phenomenon of magnetic repulsion.

You tell us that your system is not simple, and that it cannot be understood except by mathematicians. On the other hand, I claim for my system that it enjoys the advantages of simplicity, and of being intelligible to any educated mind of average ability.

I have shown in "The New Principia" that certain phenomena—viz., the extra labour of ascending a mountain, the behaviour of comets and asteroids, the tides, and the planetary notions generally—are not reconcilable with Newton's hypothesis of gravitation; but that they are beautifully confirmatory of the theory of Polarity. Nothing that you or any one else has said has in the slightest degree succeeded in shaking my position. On the contrary, you have effectually shown that your doctrines are complicated, untenable, and visionary to the last degree. Instead of making your theory distinctly, simply, and incontestably, explanatory of your facts, you are obliged to trim, twist, and qualify your theory to suit the facts.

London, *March* 7, 1884. NEWTON CROSLAND.

[Mr. Crosland must excuse me if I fail further to deal with his notions. Readers complain already and with some justice. The subject he has attacked is simply outside his range. It is rather hard on him perhaps to publish the above letter, as it so clearly shows how utterly astray he is. But in justice to myself it must appear. I have done my best for him, and must now in all courtesy withdraw.—R. P.]

THE FELLOWSHIP OF THE LEARNED SOCIETIES.

[1138]—I have been reading with much interest the remarks by "N. W." in your issue, number 119. They are, as you say in your foot-note, "very sweeping," but I certainly think they are very true. It is a subject which has afforded me some thought lately, and I am very glad to see others take it up. It is sad indeed to see these societies in such a bad condition. They are old, most of

them, and have been honoured greatly by some names which they have enrolled in their lists, and it is a great pity to admit such people as "N. W." refers to. Surely there are still some Fellows left who have pride in their society, and would try to prevent this growing evil. I would propose that it could be remedied by appointing, say, three members of the council to hold periodical examinations, either by paper or *vivâ voce*, and for candidates only who have passed this examination to be eligible for a Fellowship. It would not involve very much trouble; half-a-dozen searching questions would be a sufficient test, and would soon work a reformation in the class of men who would be admitted.

I hope "N. W." will see this letter, and, if he is a Fellow of any of these societies, that he will endeavour to carry out this plan, or any other that he may conceive which would bring about the same result.

The subject is one on which many people feel strongly; and, if either the letter of "N. W." or this one has the effect of stirring up some Fellows to action, we shall both feel gratified in thinking that our words have brought forth some good fruit.

S. DE C. THOMPSON.

THE GREAT PYRAMID.

[1139]—In your book on the Great Pyramid you said that the probable date of its erection was at or near 3,400 B.C. I was reading at the same time a book on the ancient history of the East (I forget the name of the author), and it gave the dates of the different dynasties, according to Manetho. I believe the earliest Egyptian date that is really known is the Expulsion of the Shepherds in the year 1703 B.C.

Manetho gives, if my notes are correct, the following periods for the different dynasties :—

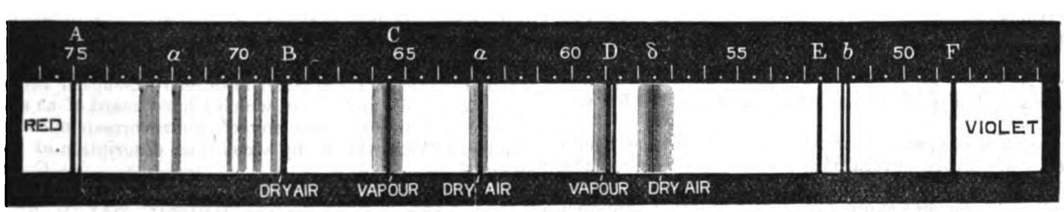
For the 1st, 253 years
„ 2nd, 302 „
„ 3rd, 214 „
„ 4th, 284 „
„ 5th, 248 „
„ 6th, 203 „
For the 7th to 11th, uncertain interval computed at 436 years
For the 11th and 12th, 213 years
13th, 453 „
14th, 184 „
Shepherds, 511 „
Ending at 1703 B.C.

As the Great Pyramid was built at the beginning of the 4th Dynasty, these figures, if added together, would bring the date to about 4200 B.C.; but, probably, the 13th and 14th Dynasties were reigning in what we call Central Egypt, while the Shepherds were reigning in the Delta. According to my idea, the dates would be something like the following :—

		Length of reign.
Accession of 4th D.	3435 B.C. ...	284
Great Pyramid built	3403 B.C.	
Accession of 5th D.	3116 B.C. ...	248
„ 6th D.	2868 B.C. ...	203
Interval (probably overrun by barbarians)	2665 B.C. ...	112
Accession of 11th D.	2553 B.C. ...	213
„ 12th D.	2340 B.C. ...	453
Invasion of Shepherds	2214 B.C. ...	511
Accession of 13th D.	1887 B.C. ...	184
„ 14th D.	1703 B.C.	

The 14th D., beginning at the expulsion of the Shepherds. Have any dates really been fixed for these events? MENES.

[Dates have been given—the trouble is that so many have been suggested.—R. P.]

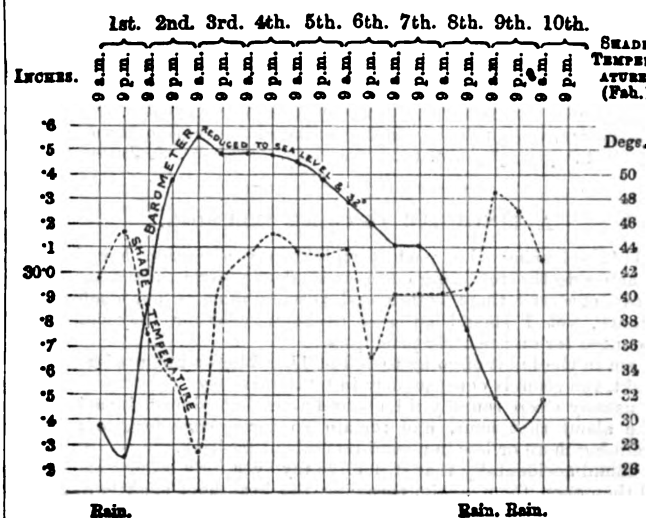


BAROMETER CURVES AND A SPECTROSCOPIC EXAMINATION OF SUNRISES AND SUNSETS.

[1140]—I have often been struck with the symmetry of barometrical curves, although from want of sufficient mathematical knowledge I am unable to investigate their properties. The curves to which I allude are formed by the union of points on a scaled diagram taken at intervals of twelve hours.

On looking at my meteorological diagrams I find one such curve from July 1st to 13th last year, a period of fine dry weather, broken only by thunderstorms on the 9th and 10th. But a still more remarkable example is found from Feb. 1st to 9th of this year. I give a draught of this curve, as well as of the curve of shade temperature, taken at the same hours of observation.

DAYS OF THE MONTH OF FEBRUARY, 1884.



The barometer readings are taken by a standard instrument verified at Kew, and they have been reduced to sea-level and 32 deg. The shade temperature (with Kew corrections) is taken in a Stevenson screen.

A short time ago one of your correspondents, I think, suggested the spectroscopical examination of "fore" and "afterglows" at sunrise and sunset. I have accordingly observed them carefully for the past month or so. I have used for this purpose one of Browning's microspectroscopes, measuring the lines and bands with the attached micrometer, having previously ascertained the positions of the solar lines, and set them down on a map. The result I have compared with Angstrom's map of the spectrum measured in wave lengths, especially with those parts of the spectrum which are marked with the variable telluric (absorption) lines and bands. I enclose a diagram of the spectrum so marked.

As an illustration, I may take the beautiful sunrise which occurred here on the 13th of this month. At 7 a.m. there was a dark, blood-red strip close to the horizon, and dark clouds above. At 7.15, yellow rays spread over the sky as if the sun were first about to make his appearance. At 7.25, I again looked out, and what was my astonishment to find the sky lit up with an intense red, crimson near the horizon and pink near the zenith.

On looking through the spectroscope at an angle of about 10° elevation, I noticed a nearly total obscuration of the violet end of the spectrum—the dark green was very faint; but the red end was very brilliant. I could just see the line *a*: B was broadened out to a thick black stroke; C was enveloped in an aqueous vapour band. The line marked *a* was very pronounced and dark. But by far the most prominent of the absorption bands was the broad shading marked *δ*. This seems to be the most characteristic band of the whole spectrum, and occurring, as it does, near the lightest part of the spectrum, is seen earliest in the day and latest in the evening. There was no very considerable vapour-band to the immediate left of D, and I could not detect any absorption at *b*, or to the left of F.

Whatever may be the cause of the brilliant colours at sunrise and sunset, it seems clear that they do not arise from *excess o*

vapour; for the most characteristic bands at B, *a*, and δ are not vapour-bands, but dry air-bands, and appear to be due to the presence of some gases or particles of other bodies floating in the air, such as meteoric dust, &c.

Perhaps your correspondents may be able to throw some light on this interesting question. HENRY J. POOLE.

Feb. 15, 1884.

INVENTION OF THE TELEPHONE.

[1141]—In KNOWLEDGE of Feb. 29 you have a most interesting article on "Who Invented the Telephone?" but there is a mention of a telephone at an earlier date than that referred to by Mr. W. Slingo, which I find in a small book on electricity by R. M. Ferguson, Ph.D., and published by W. & R. Chambers in 1867, which refers to a telephone invented and patented by Reis in 1861, which, from its construction, was evidently an earlier-dated conception than that described in your paper of the above date. W. S. JEFFERY.

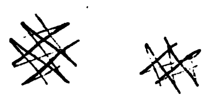
A SINGULAR OPTICAL ILLUSION.

[1142]—During the past fortnight my attention has been attracted to the following curious visual experiences; and though I have observed them before, and possibly mine is not a solitary instance, yet I should be glad to know whether they can be accounted for on scientific principles.

I am in the habit of going to bed at 11 to 11.30 p.m.; not burning a light, the room is consequently in total darkness.

I usually sleep soundly till about 4 a.m., but almost invariably wake about that hour, and remain so for a considerable time, remaining more or less in a semi-recumbent position.

I found accidentally that if I close my eyes for a minute or two, and then open them again, throwing the head back slightly at the same moment, that I see exactly opposite to, and apparently within six feet of me, two figures, thus—



the dark lines being composed of the most brilliant streaks of white light, as bright as a bit of magnesium wire.

Occasionally the form of these objects varies, and their size also; for sometimes they are as large as a sixpenny piece, and at others do not exceed in size the head of an ordinary pin. Some nights, by this same process, I have been able to reproduce this effect several times in succession, though a short interval seems necessary between each such experiment; and, again, some nights no efforts of mine will cause these scintillations to appear—they look almost like meteors or falling stars, invariably seeming to drop down and so disappear.

I have also tried on first getting into bed to see if I could induce them, but without success; they never come till I have had at least a couple of hours' sleep.

I ought, perhaps, to add that for ordinary purposes the sight of my eyes is unequal; the focus of the right is much shorter than that of the left eye, any object or printed matter appears *at least* one-third smaller when looked at with the right eye, yet these luminous lines and stars always seem the same size to both eyes.

I send this, hoping that some of your correspondents may be able to tell me whether these optical illusions arise from general causes, or from some peculiar nervous action of the retina.

COSMOPOLITAN.

A STRANGE DREAM.

[1143]—A curious instance of a dream coming true occurred to me this winter.

While at college last term I had the following dream:—I dreamt I was going to visit a strange place, and I was permitted to take a friend with me. I took a fellow named A., and we started for the visit.

It was a pouring wet day, and we were compelled to take a cab from the station. We drove several miles before we reached the house, and when we did so the friend had gone.

I rang the bell and a servant showed me into a room—I supposed the drawing-room. On my left on entering was a fire-place; a lady was working by it. I could not recognise her features. A window was opposite, through which I saw a lovely landscape, though spoilt by the rain; a young man was in the window, and, as I entered, he was saying, "Bother the rain; I meant to have gone to meet H." (myself). I noticed his face particularly. I also noticed other articles of furniture in the room.

With that my dream ended. I was so struck with it that I related it fully to A., and also to another friend, next morning.

I forgot all about it until the vacation.

I was staying with A., when I had a letter from some distant relations, up to now unknown by me, inviting me to go and stay with them while I was in the neighbourhood. I accepted, and the day on which I left A.'s house was a "drenching" one.

I reached the station of E——, and as it was so wet, I thought no one would meet me, so I took a cab. C—— was five miles from the station. I reached the house at length, and was shown into the drawing-room.

The lady of the house was working by the fire on my left, as I entered; and one of my cousins was standing at the window, and I heard him saying, before he saw me, "I wish H. would come. Bother the rain; I meant to have gone to meet him." He turned towards me as I went in, and I recognised at once the face in my dream.

The view was exactly the view I had seen before, and the details of furniture were placed as dreamed. I had never seen the people before, nor even heard of them, and had never been within fifty miles of the house.

I was much struck with the circumstance, and should like to know any good theory as regards prophetic dreams.

H. D. HINDE.

STRANGE DREAMS (ABSTRACT).

[1144]—Your article on "Ghosts and Goblins" in KNOWLEDGE of the 15th inst. reminds me of a dream I had some years ago. An old aunt of mine, of whose illness I had not heard (nor of herself for two months) appeared to me, after I had been some time asleep, in her usual attire, with a little basket of fruit in her hand, smiling and beckoning to me, exclaiming at the same time "Come along, my dear; come along," the words sounding as if really spoken by a human tongue. I awoke and looked at my watch; it was about four minutes past 12 o'clock. The background was so bright that it threw the figure of the body out, so that I could discern even the wrinkles in the face. For a time, I could hardly rid my mind of the thought that she had really appeared in person. But, regarding it as only a dream, I determined to inquire after her next day. Next morning I chanced to meet a person who knew her. Before I had spoken a word, he said, "You know old P— H—? She's dead." "Dead!" I said. "Why, I dreamt of her last night. At what time did she die?" "As near as possible, three minutes past 12." I then related the dream.

I have since had two or three such dreams. Last December, a person whom I knew very well was ill, and on the previous afternoon I inquired of a friend how he was getting on. He told me the doctors said that they had done all they could for him. That night, or rather next morning, I saw a coffin at the bottom or end of my bed, with his name and date of death upon the plate. Meeting my friend in the morning, I said, "Then So-and-so is dead," mentioning the name. He said, "How did you know that?" I said, "He died this morning, didn't he?" He said, "I have just had a telegram to say so." I then told him the dream. I can partly attribute the dream to my having heard "the doctors had done all they could for him." This might have impressed my mind; but still the coincidence is singular, as I was in excellent health at the time. I am not at all superstitious, neither do I believe in ghosts.

WALTER HOLLEBON.

ON CURIOUS STONES FOUND IN SWITZERLAND.

[1145]—Chancing to have to refer to a former number of KNOWLEDGE, I saw it mentioned therein that the Geneva correspondent of the *Times* says that certain curiously-shaped stones, covered with dots, lines, circles, and half-circles are not unfrequently found in parts of Switzerland.

During this last summer, when travelling in Scandinavia, my attention was directed to stones somewhat similar in form and markings to those which he describes. I am most anxious to procure drawings of these latter in order to compare them with those in Scandinavia, and others which I have heard of as existing in the North of Europe. Can any of your correspondents or of your readers tell me where any account or description of them is to be found? COSMOPOLITAN.

STRANGE PHENOMENON—FALSE SUN.

[1146]—On the 20th Jan. I was strikingly reminded of a report of a sunset a few weeks ago when the shadows of two gentlemen were thrown by the sun's rays on a cloud of fog or mist.

It was about 8 A.M. on the 20th January, 1884, when, leaving the station at Eston, I walked through the works of Messrs. Bolckow, Vaughan, & Co. On passing the entrance a large engine was emit-

ting clouds of steam, which the wind blew across my path, and when sufficient bodies of it passed I saw the shadow of a man at work—a very giant. I stood and watched for a few minutes, and saw at about forty yards from me some slag running into the vessel used for carrying it away. Exactly in a line with myself and about ten yards nearer was a man at work with a shovel engaged (I think) in opening the beds for the reception of the iron "pigs." Then, between the man and myself, the sheets of steam were forming a screen upon which the figure of a man and his actions at his work were clearly depicted.

Being a passenger on the North-Eastern Railway between Redcar and Middlesbrough on the 24th ult. my attention was called (about 8.35 A.M.) to a curious phenomenon. The sun was just above the Cleveland Hills, and, although not brilliant, on account of some fog, it was distinct, and of a deep red colour. At a short distance from it, on the left, I could distinctly see a second sun (or the sun's shadow) not quite so large or so distinct as the real sun, but still distinct enough to attract the attention of two gentlemen, who pointed it out to me. J. E.

STRANGE PHENOMENON.

[1147]—Whilst walking in the country last Monday evening, at about 10.20, I saw the following strange phenomenon. The night was dark and calm, and a drizzling misty rain was falling, when suddenly the whole heavens became illuminated for about two seconds, and then all was darkness again. The light was not vivid, but soft, and bright enough to show clearly the surrounding objects.

If any readers of Knowledge will give me an explantion of this curious phenomenon I shall be much obliged.—Yours faithfully,

J. Rogers.

MOCK SUNS AND MOCK MOONS.

[1148]—Thos. H. Amyot, in No. 1128, does not say whether the window from which he observed the figured repetitions of the setting sun was open or shut. If shut, and the "false suns" were seen through the glass, they may have been due to reflection from the inner surfaces of the glass.

About 25 years ago I was summoned from my lofty abode in the Midland Institute by a very enthusiastic observer, to bear witness to his discovery of a double moon, then visible through his window. He was very angry when I suggested the above explanation, but ultimately forgave me on making another discovery, viz., that the moon became single on opening the window.

I have recently made a great astronomical discovery, viz., that Venus has a satellite, subject to remarkable variations of distance from its primary. Any careful observer may confirm this discovery by viewing Venus through a window-pane. The distance between planet and satellite will vary with the thickness of the glass. W. Mattieu Williams.

LETTERS RECEIVED.

L. H. Rudd. Thanks. Your interesting letter marked for insertion.—M. B. Alder. Thanks for cuttings. I will not criticise, beyond saying that I envy you the confidence with which you "hold your views as truth." And yet I do not envy you. For such confidence is never found save with half knowledge. —Jennie Gray. "The public are" is, I think, more frequently met with; but I can give no opinion "as an authority."—G. L. D. P. I feel the utmost confidence in recommending the dearer of the two, not because it is dearer, but because I know that the difference of price is more than counterbalanced by difference in value.—A. G. Thanks for cutting from the Gloucester *Citizen*. As you say, it is unmannerly. The gentleman who arranges my lectures has explained to me how and why the *Citizen* was moved. They wanted to add 50 per cent. to their advertising price because the advertisements were *not* trade-advertisements. Regarding this as decidedly wrong, he declined to advertise at all. The *Citizen* seeks to pay *me* off for this loss of business, by declining to insert my explanation of a change in the order of lecturing, on the plea that the lectures were a trade matter. Paltry,—and innocuous.—The Devil's Advocate. Nay, I spoke of those accessories only as belonging to ghost stories, not to ghosts. Science has not rejected the phenomena of which you speak as impossible. On the contrary, some of them, as the effects "produced by what is called mesmerism" are accepted as real enough. It seems to me science takes a very fair and sensible position in such matters.—W. Poole. Vacuum would make gravity harder yet to explain. What is "the power of vacuum"?—Rich. Hilton. Your instrument ought unquestionably to show what you fail to see with it. I believe F.R.A.S. has an instrument by the same maker and of the same aperture,—which he reduces to 3 in. in making the observations for determining what such a telescope should stand.—Well-wisher and Constant Subscriber. Thanks for your kindly letter. There have been some who have regarded the insertion of matter not interesting to them as a personal offence. But I doubt if there are ten persons in England who take interest in *everything* in any week's Knowledge. Will "listen to Thomas Foster" and "take care of self." Thanks.—J. S. S. Very neat; *but* the moon was not between Venus and *the sun*.—A Common Labourer. Good gracious what an outburst! So, I set myself up as an idol. (Your letter must have upset me, for punning indicates mental disturbance, and the feeble remark occurs to me, "I'm too busy to be an idol, man." But let that pass.) You tell me that as there is less earth under the pole than under an equatorial place, gravity ought to be less at the pole; yet it is not: wherefore you think science all wrong: and the real reason why a body falls more quickly at the pole is that the air there is less dense. Now if I say—as I do—that you are mistaken, you will twit me with idolness, and "decline to worship" (so distressing to an idol, you know). Yet it is not I but science of which I am but a servant which denies utterly the validity of what you regard as reasoning. I feel rather sorry that what you call "my impartiality in attacking the theory of the great Laplace" has pleased you. To say the truth I attacked rather those who discuss as a theory what Laplace himself suggested as a speculation only. However, I hope you may still continue to describe yourself as "one time irascible, but now penitent"—for what is "*ira*" but "*furor brevis?*" That is a recondite quotation! Here is another, which only the widest reading could have instilled into me, *Sera nunquam est ad bonos mores via*.—W. H. Humsen. Thanks for your interesting letter. Rather glad (so mean are we) to hear you have a flat-earth "professor" in Maryland. I fancy we *do* lose a subscriber or two by the replies you refer to. But it is our duty to flatten some of them, and we try to do it. Yes; F.R.A.S. of Knowledge is the F.R.A.S. of the *E. M.* I do not think I have ever been unfair to the *E. M.* But my having written a good deal for it would be no reason one way or the other. I ceased writing for the very reason that paradoxists were allowed an undue amount of space. They are good fun; but one may have too much of a good thing.—London Clay. Understand about star maps. Will give shortly a map of a tolerably large equatorial region —the lively Africa—properly laid down. T. Foster angry with those correspondents! My dear sir, his note is calmness itself. I have seen the letters he referred to, and most certainly I should have spoken about them much more strongly had they been written for *my* benefit. —Gamma. The "g" is silent in "physiognomy" and sounded in "agnostic." I do not know of any definite law; but fancy, from the few words which occur to me that when the accent falls on the vowel preceding the "*g*" the letter is silent.—J. McMaster. Mars has two moons.—W. H. Molton. Thanks.—W. H. France. Fear the subject of cremation is hardly yet held attractive.—J. E. Gore. All the variable stars in my Library Atlas were taken from a list kindly supplied me by Mr. Baxendell, of Manchester.—J. Mabe. May be; but may be not. I think Mr. Cook is nearer the truth. It has been proved unsafe to interpret in scientific way such passages as you refer to. Even if you were right people would say, What a pity the account had not been rightly understood before.—D. B. Cazaux. Those were very strange dreams: but "what do they prove?"—Y. Z. Thanks. Will consider about the easy articles. The dotted lines in map (Knowledge, February 8th) are simply the outlines of constellations.—H. Moody. Certainly moon could be seen at midday in March?—J. O. Lindsay. Many thanks.—A. T. Smith. One convex eye-lens will *do*, but not nearly so well as a properly combined pair, making an eye-piece,—see any treatise on optics.—Reader of Knowledge [(?), anonymous, disguised hand]. Evidently something has troubled you very much; but letters like yours make the kindly and courteous ones even pleasanter than they otherwise would be,—take off the sameness, as it were.—G. A. Ollard.—M. Few (you seem to be one of many). —L. D. Spencer, Jun.—T. S. Etherington.—E. Gamgee.—Glad you are pleased. Thanks for kindly letters.—Sam. Do you recall why they killed the pig? We must consider many not one or two here and there. As for your winking at the Tricycle articles and winking at *me*, you have emphatically hit the pigeon and missed the crow.—Alg. Bray. Yes: your solution of the cribbage problem gives more than the original propounder's. Four correspondents (counting you) have sent 78 as the solution; and it cannot I think be beaten.—R. Bryant. Fear it would only lead to more of the New Principia and many have raised strong objections.—J. Edwards. A purely atmospheric effect.—E. Phillips. With pleasure.—J. F. Clarke. Such large sums that repeatedly the idea of giving the matter up arose. Better go elsewhere,—ill-mannered letters do not help.

Errata.—In last week's Editorial "Gossip" (paragraph 2, last line but one), for "readjust" read "recognise"; and, in paragraph 6, line 27, for "light" read "life." In "Letters Received" (eleventh line from end), for "*iter*" read "*ibis*."

Our Mathematical Column.

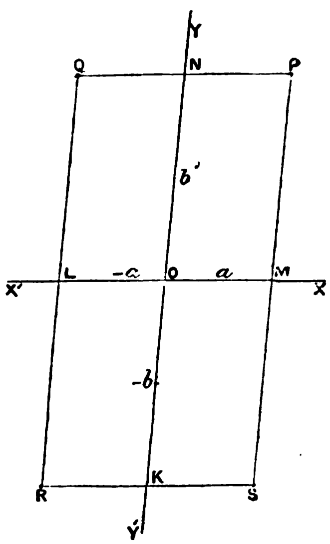
EASY LESSONS IN CO-ORDINATE GEOMETRY.

By Richard A. Proctor.

(1) Co-ordinate Geometry is that science which applies algebraical investigations to geometrical problems. I propose in these lessons to show how this science may be applied to investigate the properties of straight lines, circles, and the curves called Conic Sections. Thus we shall have to deal only with lines and curves lying in one plane.

(2) The position of a point in a plane may be determined in many ways. We shall confine ourselves in the following pages to two methods which are generally used by geometers :—

(I.) Let X O X′ and Y O Y′ be two fixed lines in a plane, intersecting at any angle in the point O. Let P be any point in the plane; and let P M and P N be drawn parallel to O Y and O X respectively, meeting O X and O Y in M and N. Then, if the lengths



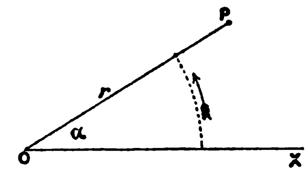
of the lines O M and O N are known, we can determine the position of P. For we have only to measure off O M and O N along O X and O Y, and through the points M and N to draw M P and N P parallel to O Y and O X respectively; the position of P is determined by the intersection of the lines M P and N P.

This method of determining the position of a point in a plane is called the *method of rectilinear co-ordinates;* X O X′ and Y O Y′ are called the *axes of co-ordinates*, and the point O the *origin*. P M and P N are called the co-ordinates of P. It is evident that we might have determined the position of P by *cutting off* O M from O X, and drawing the *parallel* M P of the required length, that is, equal to O N. Hence O M is often called the *abscissa*, and M P the *ordinate* of the point P. O X is called the *axis of x*, and O Y the *axis of y*, and distances measured along O X are generally denoted by x, distances measured along O Y by y. Thus if O M $=a$, and O N $=b$ we should express this by saying that "for the point P, $x=a$, and $y=b$," or "the co-ordinates of P are a and b," or, briefly, we may call P the point (a, b)."

(3) If the angle X O Y is a right angle the axes are said to be *rectangular;* if X O Y is not a right angle they are said to be *oblique*. The angle X O Y is called the angle of ordination, and is usually denoted by ω. Rectangular axes are more convenient than oblique axes in general, and are, therefore, most frequently employed. Occasionally, however, oblique axes may be used with advantage. In the following pages the axes are always supposed rectangular, unless the contrary is expressly stated.

(4) In our figure we have measured O M $=a$ *to the right* of O, and O N $=b$ *above* O. If we measure O L $=a$ *to the left* of O, along O X′, and O K $=b$ *below* O, along O Y′, and draw the parallels P M S, Q L R, P N Q and R K S we shall obtain four points P, Q, R, and S, each of which might be called the point (a, b). As it would obviously be inconvenient to be in doubt which of four points should be taken in this or similar cases, the following conventions as to sign are used to distinguish four such points from each other; (i.) lines measured along O X are considered positive, and lines measured along O X′ negative; (ii.) lines measured along O Y are considered positive, and lines measured along O Y′ negative. With these conventions the co-ordinates of Q are $-a,\ b$; those of S are $a,\ -b$; and those of R are $-a,\ -b$. Again, the co-ordinates of M are $a,\ 0$; of N, $0,\ b$; of K, $-a,\ 0$; and of L, $0, -b$. Finally the co-ordinates of O are $0, 0$.

(5) (II.) We proceed to describe the second method of determining the position of a point in a plane.



Let O X be a fixed line, and O a fixed point in it. Let P be any point in the plane; join P O. Then if the angle P O X be known, and also the length of O P, we can determine the position of the point P. For we have only to draw O P making the required angle with O X, and to take O P of the required length; thus the position of P is determined.

This method of determining the position of a point in a plane is called the *method of polar co-ordinates;* O X is called the *initial line*, and the point O the *pole;* P O and the angle X O P are called the *polar co-ordinates* of P. It is evident that we might have determined the position of P by supposing a line of length P O to be *carried* from coincidence with O X *through* the angle X O P. Hence P O is often called the *radius vector*, and the angle X O P the *vectorial angle*. The radius vector is generally denoted by r, and the vectorial angle by θ. Thus if O P $=a$, and the angle P O X $=\alpha$, we should express this by saying that "for the point P, $r=a$, and $\theta=\alpha$," or "the polar co-ordinates of P are a and α," or, briefly, we may call P the point (r, α).

(6) Conventions as to sign similar to those described in Art. 4, are applied to polar co-ordinates; (i.) angles measured in the direction indicated by the arrow in our figure are considered positive, and angles measured in the opposite direction negative; (ii.) lines measured from O, along O P, the revolving line are considered positive, and lines measured from O, along O P produced backwards through O are considered negative. With these conventions the point P might be determined by the co-ordinates $r,\ \alpha - 2\pi$, or $-r,\ \alpha - \pi$, as well as by the co-ordinates $r,\ \alpha$.

Note.—We shall throughout these papers consider the positive direction for angle measurement to be that here indicated, that is contrary to the motion of the hands of a watch.

EASY RIDERS ON EUCLID'S FIRST BOOK.

With Suggestions for Solution.

Prop. 1.

1. On a given straight line describe an isosceles triangle having each of the sides equal to a given straight line.

Prop. 2.

2. Show that there are in general eight different cases in the solution of this problem; and without drawing in the complete figure for each case show where the different lines will fall.

It will be found that if the given point be connected with either extremity of the given line, or if the equilateral triangle be described on either side of the line thus drawn, or if those sides of the equilateral triangle which pass through the given point be produced either way, a solution results.

3. If the diameter of the smaller circle is the radius of the larger, show that the given point and the vertex of the constructed triangle lie on the circumference of the smaller circle.

Prop. 3.

4. Having drawn two unequal lines, go through the complete construction involved in the method of Proposition 3; shewing that in this construction five circles appear, and that there are two pairs of equal circles.

Prop. 4.

5. If two straight lines bisect each other at right angles, any point in either is equidistant from the extremities of the other.

6. Apply the method of superposition to establish the first case of Prop. 26.

7. The line which bisects the vertical angle of an isosceles triangle also bisects the base.

8. Let A B be a given straight line, and from A let equal straight lines A C, A D be drawn making equal angles with A B on opposite sides of it; show that A B, produced if necessary, bisects C D at right angles.

9. The triangle A B C has equal sides A B and A C, and A D

bisects the angle B A C, the point D not lying in B C. Show that if lines D B E and D C F are drawn so that B E is equal to C F then the triangle A E F is isosceles.

10. The sides A B, A D of a quadrilateral are equal, and the diagonal A C bisects the angle D A B. Show that the sides B C, C D are equal, and that the diagonal A C bisects the angle B C D.

PROP. 5.

11. A B C is an isosceles triangle having each of the angles B and C double of the angle A. B D is drawn bisecting the angle B and meeting A C in D, shew that B D is equal to A D.

12. Two straight lines A B, and C D intersect in E and the lines E A, E B, E C, and E D are all equal. Show that the four angles E A D, E D A, E C B, and E B C are all equal.

Shew that the triangles B A C, D C A *are equal in all respects by* Euc. I. 4.

13. Apply the preceding proposition to show that when two straight lines intersect the vertical angles are equal, without assuming any proposition beyond the fifth.

14. In the quadrilateral A B C D, A B is equal to A D, and B C to C D; show that the angle A D C is equal to the angle A B C.

(*To be continued.*)

Our Whist Column.

BY "FIVE OF CLUBS."

SKILL AT CHESS.

THE following from Cavendish's interesting "Card-table Talk" will be found well worth studying by Whist-players of all classes, good, bad, and indifferent:—

In the latter part of the winter of 1857, during an after-dinner conversation, it was remarked by some of the party that Whist is a mere matter of chance, since no amount of ingenuity can make a King win an Ace, and so on. This produced an argument as to the merits of the game; and, as two of the disputants obstinately maintained the original position, it was proposed to test their powers by matching them against two excellent players in the room. To this match, strange to say, the bad players agreed, and a date was fixed. Before the day arrived, it was proposed to play the match in double, another rubber of two good against two bad players being formed in an adjoining room, and the hands being played over again, the good players having the cards previously held by the bad ones, and *vice versâ*, the order of the play being, of course, in every other respect preserved. The difficulty now was to find two players sufficiently bad for this purpose; but two men were found, on condition of having odds laid them at starting, which was accordingly done.

On the appointed day, a table was formed in room A, and as soon as the first hand was played, the cards were re-sorted and conveyed into room B. There the hand was played over again, the good players in room B having the cards that the bad players had in room A. At the end of the hand, the result was noted for comparison, independently of the score, which was conducted in the usual way. Thirty-three hands were played in each room. In room A, the good players held very good cards, and won four rubbers out of six; in points, a balance of eighteen. In room B, the good players had, of course, the bad cards. They played seven rubbers with the same number of hands that in the other room had played six, and they won three out of the seven, losing seven points on the balance. The difference, therefore, was eleven points, or nearly one point a rubber in favour of skill.

A comparison of tricks only, showed some curious results. In seven of the hands the score by cards in each room was the same. In eighteen hands the balance of the score by cards was in favour of the superior players; in eight hands in favour of the inferior. In one of these hands the bad players won two by cards at one table, and three by cards at the other.

The most important result is, that at both tables the superior players gained a majority of tricks. In room A, they won on the balance nineteen by tricks; in room B, they won two by tricks.

It will be observed that this experiment does not altogether eliminate luck, as bad play sometimes succeeds. But by far the greater part of luck, viz., that due to the superiority of winning cards, is, by the plan described, quite got rid of.

Dr. Pole (the *Field*, June 16, 1866) arrives at a result nearly the same by a statistical method. He writes to this effect:—

"It is very desirable to ascertain the value of skill at Whist.

"The voluntary power we have over results at Whist is compounded of—1. The system of play. 2. The personal skill employed."

The modern system, which combines the hands of the two partners, as against no system (the personal skill of all being pretty equal), is worth—Dr. Pole thinks—about half-a-point a rubber, or rather more. About 900 rubbers played by systematic as against old-fashioned players, gave a balance of nearly 500 points in favour of system.

The personal skill will vary with each individual, and is difficult to estimate; but looking at published statistics, in which Dr. Pole had confidence, he puts the advantage of a very superior player (all using system) at about a quarter of a point a rubber; consequently the advantage due to combined personal skill (*i.e.*, two very skilful against two very unskilful players, all using system), would be more than half-a-point a rubber.

The conclusion arrived at by Dr. Pole is that "the total advantage of both elements of power over results at Whist may, under very favourable circumstances, be expected to amount to as much as one point per rubber."

Now, at play-clubs, nearly all the players adhere more or less closely to system, and the great majority have considerable personal skill. Consequently, only the very skilful player can expect to win anything, and he will only have the best player at the table for a partner on an average once in three times. It follows from this, that the expectation of a very skilful player at a play-club will only average, at the most, say a fifth or a sixth of a point a rubber.

J. R. W.—It is impossible to lay down rules for ruffing second hand; because so many varying circumstances may arise. The rule you mention, "only to ruff your adversary's lead" (meaning doubtless an adversary's suit), is quite insufficient, and also incorrect. In most cases the player to your right leads either his own suit or his partner's; but it by no means follows that you should in most cases ruff. The following is a good general rule, to which however there are numerous exceptions. If the trick is doubtful, that is, if you have no means of knowing whether your partner can take it or not, ruff if you have fewer trumps than four; if you have six trumps or more, ruff and lead trumps; if you have five trumps ruff and either lead from your strong suit or return your partner's: if you have four trumps pass the trick. The cases here to which there are most exceptions are the last two. It is often well to ruff a doubtful card even with the critical four trumps,—and with five trumps it is often desirable to pass the trick. Remember that when you are short in a suit led by an adversary, it is likely enough that your partner is long in it, and may be as much interested in seeing it established as the adversary who originally led it. So even if when you have passed, third in hand holds and plays the master card, your partner may reap the benefit. But of course in the greater number of cases, though a trick may in a sense be doubtful, the previous play has shown enough to make it more or less probable that your partner can take it; so that in reality the question of ruffing is generally a nice problem in probabilities.

NATIONAL HEALTH SOCIETY.—The eleventh annual report of the National Health Society, which was read at the Society's Rooms, 44, Berners-street, W. (Dr. Robert Farquharson, M.P., in the chair), shows that the society has carried on its work during the past year in a most practical manner. Hundreds of lectures on sanitary subjects have been delivered, not only all over the poorer parts of London, but in provincial towns, to large audiences of working men and women, classes of girls, district visitors, and others engaged in work amongst the poor. The society is much encouraged by the practical result of their lectures on "Keeping the House Healthy," "Rearing of Infants," "Prevention of the Spread of Infectious Diseases," "Preparation of Food," and kindred subjects, a knowledge of which is so much needed in our densely-crowded neighbourhoods. The question of poisonous dyes in domestic fabrics, of dust collection, of smoke abatement, and the prevention of cholera, have been investigated and reported upon by special committees appointed for the purpose. The Metropolitan Public Gardens and Playgrounds Association, which is in connection with the National Health Society, has been most active in assisting to open spaces for recreation in crowded parts of London, also in placing seats and planting trees in convenient situations. The Health Exhibition, held by the Society last June, was commented upon; and the Secretary stated that more than 100 members had joined the society during the year

Our Chess Column.

BY MEPHISTO.

A BIT OF MORPHY.

WE recollect an amusing incident which took place some years ago. Mr. Burns, of Liverpool—a very fine player—playing incognito, took the odds of a Knight from a well-known but weak professor, and lost purposely. "I think it is an advantage to play without the Knight," said Mr. Burns to his victorious opponent, "it allows you to Castle early in the game. I think I will try and give you a Knight." So said, so done. Needless to say, Mr. Burns astonished his opponent by winning the game.

This week in playing and giving the odds of a Knight, we have actually had the strange experience of finding that in a certain variation it really is an advantage to be minus the Queen's Knight. This occurred in Morphy's variation of the Muzio Gambit, and came about as follows:

Remove White's Queen's Knight.

1. $\frac{\text{P to K4}}{\text{P to K4}}$ 2. $\frac{\text{P to KB4}}{\text{P takes P}}$ 3. $\frac{\text{Kt to KB3}}{\text{P to KKt4}}$ 4. $\frac{\text{B to B4}}{\text{P to Kt5}}$
5. $\frac{\text{Castles}}{\text{P takes Kt}}$ 6. $\frac{\text{Q takes P}}{\text{Q to B3 (best)}}$ 7. $\frac{\text{P to K5}}{\text{Q takes P}}$ 8. $\frac{\text{B takes P (ch)}}{}$

This gives a very fierce attack, but it is hardly sound against a good defence. In giving the odds of a Knight it leaves White with three pieces to the bad, but in practice we have often succeeded in this bold venture:—8. $\frac{}{\text{K takes B}}$ 9. $\frac{\text{P to Q4}}{\text{Q takes P (ch)}}$ 10. $\frac{\text{B to K3}}{}$

BLACK.

WHITE.

10. $\frac{}{\text{Q to B3.}}$ Q to Kt2 seems better, but even then we think White could obtain a draw, as the following will show:—Q to Kt2. 11. Q to R5 (ch), Q to Kt3. 12. R takes P (ch), Kt to B3. 13. R takes Kt (ch)! K takes R. 14. B to Q4 (ch), K to B2. 15. R to B sq (ch) (this could not be done now but for the fact of White giving the odds of the QKt), K to Kt sq. 16. Q to Q5 (ch), Q to K3. 17. Q to Kt5 (ch), and White can draw by perpetual check if he likes. Also in the actual variation it can be seen that it is of great advantage for White to be able to bring his QR into play, whereby he limits Black's defences very much. After 10. $\frac{}{\text{Q to B3,}}$ the game continued as follows:—11. $\frac{\text{B takes P}}{\text{P to Q3}}$ 12. $\frac{\text{Q to R5 (ch)}}{\text{Q to Kt3}}$ (The Black King could not play to K2 on account of B to Kt5, nor to K3 on account of QR to K sq (ch), threatening to follow up by Q to K8, mate! There is something to be said though in favour of K to Kt2. We think in that case, White would not do well to win the Black Queen by either B to R6 (ch) or B to K5, when, of course, the Queen could not take the B on account of R to B7, mate! but Kt takes B in the former case, and P takes B in the latter case would leave White no alternative but to take the Queen with his Rook, leaving his opponent with too many pieces for the Queen. Instead of either of these moves, White would do better to play B to K3, threatening to win the Queen for the Rook only, and with a better game, as she obviously cannot move to Kt3 on account of B to Q4 (ch). 13. $\frac{\text{B takes P (ch)}}{\text{Kt to B3}}$ 14. $\frac{\text{R takes Kt (ch)}}{\text{K takes R}}$ 15. $\frac{\text{Q to K5 (ch)}}{\text{K to B7}}$ 16. $\frac{\text{R to B sq (ch)}}{}$ which, of course, decides the game in favour of White, strange to say, owing to the fact of White having given the QKt, he is saved from being compelled to play Kt to QB3 in order to develop his Rook, in which case Black would gain time for a developing move which might repel the whole attack. If now K to Kt sq, the Rook mates; the game proceeded as follows:—16. $\frac{}{\text{B to B4}}$ 17. $\frac{\text{R takes B (ch)}}{\text{Q takes R}}$ 18. $\frac{\text{Q takes Q (ch)}}{\text{K to K sq}}$ 19. $\frac{\text{Q to K6 (ch)}}{\text{K to Q sq}}$ 20. $\frac{\text{B to K5}}{}$ and Black resigned.

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CONTENTS OF No. 125.

THE EVOLUTION OF FLOWERS.

By GRANT ALLEN.

IV.—SIDE BRANCHES.

BEFORE we finish with the simplest group of monocotyledons, the Alismas, and proceed to their more developed relations, the true lilies, we must stop awhile to examine a couple of divergent species which present some singular features of their own, showing them to be distinct lateral offsets from the primitive water-plantain ancestor.

The common arrowhead (*Sagittaria sagittifolia*) might fairly claim, for several reasons, to represent the original monocotyledon even more closely than Alisma, were it not for one particular, which I shall mention in due course. It is a pretty and stately perennial, with glossy green, arrow-shaped leaves, rising high out of the water, and bearing a tall bunch of large and striking snow-white flowers. As in Alisma, there are three outer green sepals and three inner and larger white petals. The stamens, instead of being only six in number, are very numerous; and as this is also the case in the buttercups, the simplest and most primitive dicotyledons, we may fairly accept it as a common sign of great antiquity. The carpels are also numerous, small, and one-seeded. All these points show the arrowhead to be at least as primitive in general type as the water-plantain, while the large number of the stamens and carpels seems to betoken a still earlier and simpler organisation.

Why, then, should we not take the rare arrowhead rather than its commoner ally, the Alisma, as the best living representative of the general ancestor from whom the whole existing lily stock is ultimately descended? Simply for this reason; the Sagittarias have all distinct male and female flowers, while in the Alismas, as in the lilies and most other advanced monocotyledons, the stamens and pistils are both found together in the same blossoms. In the arrowhead, the upper flowers are usually males, that is to say, possess stamens alone, while the lower ones are females, with nothing in their middle but a group of very numerous carpels. The flowers, originally hermaphrodite (that is to say, with both stamens and carpels), have become differentiated into two distinct sexes, males above and females below, by the abortion or gradual disappearance of the carpels in one case and the stamens in the other.

Why is this? Well, the change is only one out of the many means commonly adopted by flowers to ensure cross-fertilisation. We may conclude that at some early time some very primitive Alisma-like plant showed some tendency to produce more stamens on some of its blossoms, and more carpels on others. This is a tendency which often occurs in many plants, and as a familiar case we may take the strawberry, which everywhere, but especially in America, has a constant leaning towards the separation of its sexes not only on different flowers, but even on different plants. Now, whenever this tendency is set up, it is pretty sure to prove advantageous to the species (unless, indeed, it interferes with some other and better device for cross-fertilisation) because it renders it absolutely impossible that any blossom can be impregnated in the undesirable fashion with pollen from its own stamens. Accordingly,

Sagittaria sagittifolia.

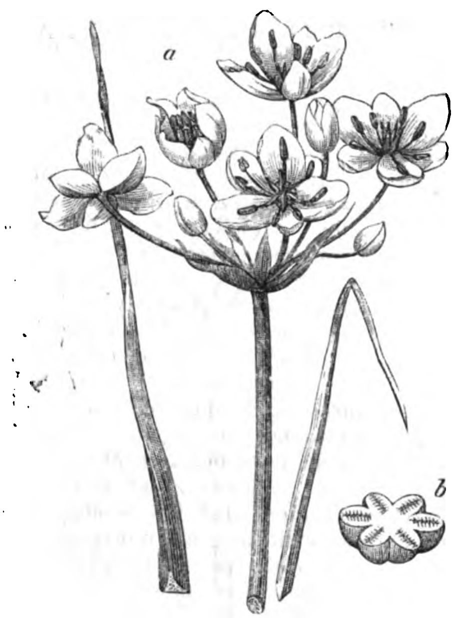
the arrowheads have kept this useful feature ever since, and so have successfully avoided self-fertilisation with its usual concomitant of degeneracy and final extinction. Their chief insect allies are flies of the same sort as those which visit the true Alismas.

Why, again, are the male flowers at the top, while the females are below? For this reason. The flies, like most other insects, always begin their visits to a spike of blossoms at the bottom and proceed slowly upward. In this way they arrive at the female flowers first, and dust them over with pollen from the plant they last visited. Then, as they rise towards the top, they reach the male flowers, and gather from them a fresh store of pollen, which they carry away to the next bunch. By such a simple device nature ensures the fertilisation of each blossom, not merely from another flower, but also from another plant. That is cross-fertilisation in its truest and purest form, and it produces the stoutest and healthiest seedlings of all.

Once more, why doesn't the pollen from the male flowers fall down upon and fertilise the females below? Would it not have been better to put the females on top, so as to

avoid this chance? No, because the flowers open from below upward, and so the females ripen first. They will, therefore, in all probability, get impregnated before the stamens begin to shed their golden dust. But if they don't, then the pollen will fall upon them in due course; and this modified form of cross-fertilisation from the flowers of the same stem is better far than no fertilisation at all, and consequent absolute sterility.

Arrowhead, then, is clearly a close representative of a very primitive form; but it fails to be the closest representative, because in it the pistils of some flowers and the stamens of others have become abortive, and this could not have been the case in the ancestor of the fully hermaphrodite lilies and amaryllids. In short, arrowhead is a very low but still specialised form.



Butomus umbellatus.

Our last British Alismaceous plant is the beautiful rose-coloured flowering rush (*Butomus umbellatus*), whose tall, straight stem and handsome umbel of large bright flowers rise so proudly above the ponds and backwaters of our southern counties. The flowering rush shows some distinct signs of advance in the same general direction as that taken independently by the lilies. The distinction between sepals and petals has here nearly died away, for all six perianth pieces are nearly equally large and brightly coloured, and all equally take part in the attractive display. Nevertheless, if you look carefully you will see that the three outer ones still retain marks of their original character as sepals, for they are not quite so large or so petal-like in structure as the three inner, and they have acted as a covering to the true petals while in the bud. This similarity between petals and sepals is even more marked in the lilies, where the distinction of the two can scarcely ever be perceived in anything else save arrangement on the stem. The stamens are nine in number—three whorls of three each, and the carpels are six, as in the damasonium and the sea arrow grass. But in each carpel—and this is an important point—there are many seeds, instead of one or two only; and we shall find that a similar advance in such respect is almost universal in the lower lilies.

The large rose-coloured flowers of the Butomus attract bees and other higher insects, by whose aid they have no doubt been developed. These trustworthy allies enable it to get fertilised very securely, and so to lessen the number of its carpels to six. But each carpel contains many seeds, and so the plant can afford to do with comparatively few flowers, far fewer than the Alisma or the Triglochins. It has learnt to substitute efficiency for large numbers. This, also, is a step in advance in the direction of the true lilies.

Finally, observe the bracts or small leaves at the base of the bunch of flowers, put there to sheathe them from harm while they are young buds, and to protect them both from cold and from injurious insects. Those bracts are the first indication of what is called a spathe, which becomes so important and conspicuous in several of the higher lily-like plants. Among the true lilies, we get it very marked in the garlics and onions. Among the Amaryllids it is almost equally noticeable in the narcissus and daffodil. Among the palms it assumes the form of a large sheet, completely enclosing the whole mass of blossom. And last of all, in the very degraded arums, it forms the hood or sheath, which is all that can be seen externally of the flower, and within which the real blossoms cluster closely on a long spike or spadix, almost entirely hidden within the large coloured spathe. It is interesting thus to note the first faint beginnings of what rises at last to be so very conspicuous and remarkable an organ. As it occurs in the flowering rush, the spathe is nothing more than three small thin bracts, that is to say, very reduced and simple stem-leaves.

THE MYSTERY OF GRAVITY.

By not a few the idea is erroneously entertained that when Newton established the law of gravity, he had explained the movements of the heavenly bodies. "Nature, and Nature's laws lay hid in night," the poet sings, and then with poetical truth he adds, "God said let Newton be, and all was light." Such, however, was not Newton's own opinion. He saw clearly that behind the law which he had discovered, beyond the veil which he had removed, there was a mystery greater and more solemn than the mystery of those movements in the heavens and on the earth which in a sense he might be said to have explained.

It was a fancy of Kepler's in his earlier days, that the planets are carried along their orbits by angels, moving according to appointed laws, so that although the paths of the planets and their movements in those paths may be described in mathematical terms, yet they are to some degree the result of voluntary action,—a disobedient angel might carry his planet the wrong way if he so willed, unless his wilful disobedience were instantly checked by a superior power (not necessarily the supremest). Newton showed that these intermediary agents are not necessary; that the laws of planetary motion are themselves but parts of a higher and more widely ranging law, the law of universal attraction. But even as Kepler could not explain why the laws which bear his name exist, so Newton who could explain them as results of the law of attraction, could not explain why that law itself exists.

Newton's own words are worth considering by those who imagine he had explained the whole mystery of celestial movements:—"Hitherto," he says, "we have explained the phenomena of the heavens and of our sea by the power of gravity, but have not assigned the cause of this power. This is certain, that it must proceed from a cause that penetrates to the very centres of the sun and planets, without suffering the least diminution of its force; that operates not according to the quantity of the surfaces of the particles upon which it acts (as mechanical causes

used to do) but according to the quantity of the solid matter which they contain, and propagates its virtue on all sides to immense distances, decreasing always in the duplicate proportion of the distances. Gravitation towards the sun is made up out of the gravitations towards the several particles of which the body of the sun is composed; and in receding from the sun decreases accurately in the duplicate proportion of the distances as far as the orb of Saturn—nay, even to the remotest aphelions of the comets (he gives reasons for this belief which can be applied in our own time, even to the whole space over which the range of the telescope has been extended). But hitherto I have not been able to discover the cause of those properties of gravity from phenomena, and I frame no hypotheses" (*hypotheses non fingo*). He explains what he means by hypotheses, in words showing that he used the term in a somewhat restricted sense, for unquestionably in our own time a hypothesis is generally understood to mean a proposition suggested as a possible way of explaining phenomena, and therefore in some degree deduced from them. Newton however defines a hypothesis as "whatever is not deduced from the phenomena"; and hypotheses, he proceeds, "whether metaphysical or physical, whether of occult qualities or mechanical, have no place in experimental philosophy, in which particular propositions are inferred from the phenomena and afterwards rendered general by induction. In this way were discovered the impenetrability, the mobility, and the impulsive force of bodies, and the laws of motion and of gravitation. *To us it is enough* that gravity does really exist, and act according to the laws which we have explained and abundantly serves to account for all the motions of the celestial bodies, and of our sea."

THE EXTRAORDINARY SUNSETS.

By A. COWPER RANYARD.

(*Continued from page* 156.)

THERE seems to be some evidence to show that brilliant sunset phenomena have been observed on former occasions in connection with volcanic eruptions. Gilbert White, of Selborne, in one of his letters to the Hon. Danes Barrington, describes "the amazing and portentous phenomena" observed in the summer of 1783. "The sun at noon looked as blank as a clouded moon, and shed a rust-coloured ferruginous light on the ground, particularly lurid and blood-coloured at rising and setting. The country people began to look with a superstitious awe at the red, lowering aspect of the sun; and, indeed, there was reason for the most enlightened person to be apprehensive, for all the while Calabria and part of Sicily were torn and convulsed with earthquakes, and about that juncture a volcano sprang out of the sea off the coast of Norway." There are many references to the great alarm which was caused all over Europe by the appearance of a "red fog," which lasted during the whole of the summer of 1783. Lalande ascribed it to the natural effect of a hot sun succeeding on a long period of heavy rains. Cowper refers to it in "The Task," Book II., lines 53–65, which were written in the autumn of 1783, and speaks of the eruptions with the unusual appearance of the sky and the dim and sickly appearance of the sun. He also refers to extraordinary winds and tidal waves.

When were the winds
Let slip with such a warrant to destroy?
When did the waves so haughtily o'erleap
Their ancient barriers, deluging the dry?
Fires from beneath, and meteors from above,
Portentous, unexampled, unexplained,
Have kindled beacons in the sky; and the old
And crazy earth has had her shaking fits
More frequent, and foregone her usual rest.
Is it a time to wrangle, when the props
And pillars of our planet seem to fail,
And nature with a dim and sickly eye
To wait the close of all?

Mrs. Somerville, in her "Physical Geography," traces the origin of these atmospheric phenomena of 1783 to the great eruption of Skaptar Jokul, which broke out on May 8, and continued till the end of August, sending forth lava and clouds of dust, which spread over the whole of Europe. Sir Charles Lyell, in his "Principles of Geology," Vol I., p. 372, speaks of this eruption as "more tremendous than any recorded in the modern annals of Iceland." Some idea of the magnitude of the eruption may be derived from a calculation made by Sir John Herschel, mentioned in his "Physical Geography," p. 111, where he states that twenty-one cubic miles of lava were ejected—a quantity equal in volume to the water poured by the Nile into the sea in a year.

Professor Michie Smith, in a letter published in *Nature* of Nov. 8, 1883, after describing the blue sun recently seen in India, says that a similar blue appearance of the sun was observed over Europe and America in 1831. He rejects the Krakatoa theory, as not accounting for the appearances observed in India, and he does not seem to have been aware of the eruption in the Mediterranean which, in July, 1831, gave rise to Graham's Island. Extraordinary sunsets were observed in that year, as appears from a letter of Mrs. E. M. Pitman, published in *Nature* of Dec. 13. She says: "I remember how splendid the colouring of the sky was at Malta, after sunset, in the year that 'Graham's Island' appeared. The great beauty of the sunsets we have been having have forcibly reminded me of the colouring I saw so many years since at Malta."

This evidence is not conclusive, but it lends some probability to the assumption that the recent sunset phenomena are connected with the great Krakatoa eruption. On June 30, 1861, the earth passed through the tail of the great comet of that year, and must have swept clean of dust a cylindrical area 8,000 miles in diameter, and probably 400,000 or 500,000 miles in length, but there were no sunsets which attracted special attention during the following months of that year.

It cannot be doubted that the earth did pass through the tail of the comet of 1861. Mr. Hind predicted that such a transit would take place on the evening of June 30; and the Rev. T. W. Webb, of Hardwick, without knowing of Mr. Hind's prediction, noticed on the evening of June 30 a number of long, faint streamers pointing to the nucleus of the comet. As he watched them, they slowly closed up like a fan. An account of Mr. Webb's observation is given in the *Monthly Notices*, Vol. XXII., p. 311, and leaves no room to doubt that the faint streamers which he saw stretching across the heavens were streams of rather denser matter connected with the nucleus, and that the earth was at the time passing through the tail, as a fly might pass through the feathers of a shuttlecock. Mr. George Williams, of Liverpool, also noticed the rapid angular motion of two of the streamers, while Mr. Tebbutt, in Australia, who was observing on the morning of July 1, 1861—*i.e.*, really in the afternoon before sunset of our English June 30—noticed the widening out of the branches of the tail, showing that the actual passage of the earth through the central part of the tail must have taken place about sunset on the evening of our June 30.

I was aware of this transit of the earth through the tail of the comet of 1861, when I suggested that the

passage of the earth through a cloud of meteoric dust might have given rise to the recent sunset phenomena, but I thought that the matter of a comet's tail is probably too imponderable to cause any sensible effect in the condition of the earth's atmosphere.

If the earth has recently passed through a cloud of meteoric particles, they cannot have been comparable in size with the fragments which on entering the air give rise to the meteors which are seen with the naked eye. Millions of such meteors entering the air at once would cause the heavens to appear all ablaze, and even if only two or three were encountered in every cubic mile of space, the resulting phenomena could not have escaped attention on clear nights. But millions of such little meteors as are only seen with the telescope might enter the atmosphere without increasing the light of the heavens sufficiently to attract attention. We know nothing definitely as to the size of the particles which give rise to meteors which do not reach the earth's surface, or as to the amount of matter melted and driven off from the surface of meteors which do reach the earth's surface.

An ordinary house candle seen at the distance of a mile appears much brighter than a sixth magnitude star, and a small morsel of platinum wire raised to a white incandescent heat, not sufficient to melt the platinum, gives more light than such a candle. The meteors which appear and vanish in the upper air, are no doubt, raised to as high a temperature as the platinum wire, or they would not be broken up and driven into vapour in a few seconds. But it does not necessarily follow that a mass which would give the light of a sixth magnitude star if seen at the distance of a mile, as one side of it is heated and driven into vapour in its passage through the air, is smaller than the minute piece of platinum wire which gives rise to the brilliant light in the laboratory. Such an incandescent mass seen at a hundred miles distance would give one ten-thousandth part of the light of the same mass seen at a mile distant. It is, therefore, conceivable that large quantities of matter may come into the air without being perceived by us.

Observations show that the larger meteors usually become incandescent and attract attention before they have entered the air to a depth of 70 miles above the sea level. It is evident that the resistance of the atmosphere must tend to break up small particles more suddenly than large particles. For other things being similar, the resistance experienced varies as the surface, that is, as the square of the diameter of the moving particle, while the momentum varies as the weight or cube of the diameter. In the case of particles which do not reach the earth's surface, the whole of the momentum is converted into heat, and owing to the relatively larger surface of small particles, the heat will be more rapidly developed the smaller the particle. When a meteoric particle first encounters resistance in the outer air, its temperature is probably many degrees below zero. As the resistance increases, its temperature must rise until the surface of the particle becomes white hot, unless the particle is composed of material which can be driven into vapour at a temperature less than a white heat, or unless the resistance increases so slowly that the radiation of heat from the particle into space, and the loss of heat due to contact with she cold air, more than counterbalances the increase of temperature due to loss of momentum. If the smaller meteoric particles consist of materials similar to the meteoric masses which have been known to reach the earth's surface, the first supposition may be neglected. It will hardly be contended that particles moving with velocities comparable with the earth's velocity in space take so long to enter the earth's atmosphere to a depth where the resistance would cause them to become incandescent that they have time to radiate away the greater part of the heat, which, if developed suddenly, would have driven them into vapour, and it seems hardly probable that contact with the cold air would cause such a loss of heat in a few seconds. It should be remembered that the weight of air encountered by a meteoric mass increases relatively to the weight of the meteoric mass as its diameter decreases. The temperature of the air encountered has probably less to do with the resulting temperature of the meteoric mass than the velocity with which the air is encountered. It is evident that the larger masses become white hot from the blows they receive from the particles of the intensely cold air, and it seems reasonable to suppose that the greater the number of blows delivered on a given mass, in a given time, the greater will be the heat to which the meteoric mass is raised.

The chief part of the light emitted by a meteor is emitted between the level where it is raised to a temperature of white incandescence and the level where the last portion of the solid or melted nucleus is driven into vapour. It is evident that the smaller the meteor the higher will be the level at which the resistance of the air would first raise it to incandescence, and the more rapidly will it pass to the level where the last portion is driven into vapour. If it is broken into fragments by the pressure caused by its motion through the atmosphere, or by expansion due to heat, the area of the resisting surface will be increased, and the solid or liquid parts will be still more quickly converted into vapour. It seems, therefore, improbable that small meteoric particles, which enter the air with velocities comparable with the earth's velocity, should reach the surface without being vaporised. The vapour of iron would, no doubt, rapidly be precipitated, and it would probably reach the surface in the form of very minute particles of black oxide.

(*To be continued.*)

GHOSTS AND GOBLINS.

BY RICHARD A. PROCTOR.

(*Continued from page 122.*)

IT would be easy to fill page after page with the details of the various ideas entertained about ghosts, goblins, and demons. Such ideas extend not only to the appearance of such beings, their apparel, appurtenances, and so on, but to the noises which they make either of themselves or by means of various supernatural objects which they are supposed to carry about with them. Thus—

> The sheeted dead
> Did *squeak* and gibber in the streets of Rome
> A little ere the mightiest Julius fell.

And it is to be noted that as ghosts commonly show no face, so, few have been known to speak with full voice. This may be because the noises heard at the hours when ghosts are seen are not such as can be by any possibility mistaken for the human voice in its ordinary tones, while nevertheless an excited imagination can frame words out of the strange sounds which can be heard in almost every house in the stillness of night. This also serves to account for the notion that ghosts can clank chains, or make other dismal noises. Sounds heard at night are highly deceptive; a small noise close by is taken for a loud noise at a distance (not necessarily a very great distance); and a noise made by objects of one kind will be mistaken for noises made by objects of a different kind altogether. A friend of mine

told me he had been disturbed two nights running by a sound as of an army tramping down a road which passed some 200 yards from his house; he found the third night (I had suggested an experimental test as to the place whence the sound came) that the noise was produced by a clock in the next house, the clock having been newly placed against the party wall. We all know Carlyle's story of the ghostly voice heard each evening by a low-spirited man—a voice as of one, in like doleful dumps, proclaiming "once I was hap-hap-happy, but now I am meeserable,"—and how the ghost resolved itself into a rusty kitchen-jack. There is a case of a lady who began to think herself the victim of some delusion, and perhaps threatened by approaching illness, because each night, about a quarter-of-an-hour after she had gone to bed, she heard a hideous din in the neighbourhood of her house, or else (she was uncertain which) in some distant room. The noise was in reality the slightest possible creak (within a few feet of her pillow, however), and produced by the door of a wardrobe which she closed every night just before getting into bed. This door, about a quarter-of-an-hour after being closed, recovered its position of rest, slightly beyond which it had been pushed in closing. In another case the crawling of a snail across a window produced sounds which were mistaken for the strains of loud but distant music.

It is, perhaps, not going too far to say that our modern spirits, who deal in noise-making as well as in furniture-tilting (of yet more marvellous feats I say nothing), are not unacquainted with the means by which the ear may be deceived as in the cases just considered. Some sounds said to be heard during dark *séances* suggest the suspicion.

It will be seen that the opinion to which I incline—as the best and perhaps only natural interpretation of events supposed to be supernatural—is that real sights and sounds are modified by the imagination, either excited or diseased, into seemingly supernatural occurrences. It does not seem to me likely that in any large proportion of recorded (and presumably veracious) ghost stories, there has been an actual phantom of the brain. Such phantoms are sometimes seen, no doubt, and unreal voices are sometimes heard; but the condition of the brain which leads to such effects must be regarded as altogether exceptional. Certainly it is not common. On the contrary, the play of fancy by which images are formed from objects in no way connected with the picture raised in the mind is a common phenomenon. Although some minds possess the faculty more than others, few actually want it. I suppose there is not one person in a thousand who cannot see "faces in the fire," for instance, though to some the pictures so produced are much more vivid than to others. Dickens tells us that in travelling through a cleared region in America at night, the trees by the roadside seemed to assume the most startling resemblance to different objects—now an old man sitting in a chair, now a funeral urn, and so on. Doubtless, not every traveller along the road under the same circumstances would have found so many fanciful tree-pictures formed for him, or perhaps any formed so distinctly as did Dickens, with his lively imagination and wealth of mind-images. Yet probably very few persons travel along a tree-covered region in the deeper dusk of evening without fancying that the trees shape themselves into strange forms of living or inanimate objects.

But the important point to be noticed is that when the mind is deeply occupied with particular thoughts the imagination is more likely to conjure up pictures connected with those thoughts than such random pictures as are formed when the mind is not so preoccupied. If we admit this—and I conceive that there can be very little doubt on the point—we can dispose very readily of the argument from coincidence, advanced by those who believe that the spirits of the dead sometimes come visibly into the presence of the living. I present this argument as urged in an analogous case (that of visions at the moment of death) by a late eminent mathematician, whose belief in the possibility at least of many things which are commonly regarded as superstitions was so well known that no apology need here be made for touching on the subject. After speaking on the general subject of coincidences, De Morgan thus, in language less simple than he commonly employs, presents the argument for spectral apparitions (at the moment of the death of the person so appearing):—"The great *ghost-paradox* and its theory of coincidence will rise to the surface in the mind of every one. But the use of the word *coincidence* is here at variance with its common meaning. When A is constantly happening, and also B, the occurrence of A and B at the same moment is the mere coincidence which may be casualty." (That is, this is a coincidence of the common kind.) "But the case before us is that A is constantly happening" (*here* by A, De Morgan means a death, as he explains further on, but the explanation should come in at this point) "while B" (the spectral appearance of the person who dies), "when it does happen, almost always happens with A, and very rarely without it. That is to say, such is the phenomenon asserted; and all who rationally refer it to casualty affirm that B is happening very often as well as A, but that it is not thought worthy of being recorded except when A is simultaneous." I must venture to express my dissent from this statement; it seems to me incredible that any person would, as De Morgan asserts, *rationally* affirm that spectral appearances are "very often" seen. "In talking of this subject," he proceeds, "it is necessary to put out of the question all who play fast and loose with their secret convictions: these had better give as a reason, when they feel internal pressure for explanation, that there is no weathercock at Kilve: this would do for all cases. But persons of real inquiry will see that, first, experience does not bear out the asserted frequency of the spectre, without the alleged coincidence of death; and secondly that if the crowd of purely casual spectres were so great that it is no wonder that now and then the person should have died at or near the moment, we ought to expect a much larger proportion of cases in which the spectre should come at the moment of the death of one or another of all the cluster who are closely connected with the original of the spectre." (This is not very distinct: any wrong spectre, with or without close connection with any particular moribund, would seem to serve De Morgan's purpose in this argument equally well. He appears to insist, however, on the fact—undoubtedly such—that if spectres were commonly appearing, without reference to the deaths of individuals, cases would happen pretty frequently where a spectre appeared which was not that of a person then dying, but of some near relative. I feel by no means sure, however, that I have rightly caught De Morgan's meaning.) "But this," he proceeds, "is, we know, almost without example. It remains then, for all who speculate at all, to look upon the asserted phenomenon, think what they may of it, the thing which is to be explained, as a *connection* in time of the death, and the simultaneous appearance of the dead. Any person the least used to the theory of probabilities will see that purely casual coincidence, the *wrong spectre* being comparatively so rare that it may be said never to occur, is not within the rational field of possibility."

I have quoted this argument because it applies equally well to the case of spectral appearances after death. The right spectre is always seen, so far as is known, and it

appears always on a suitable occasion (at least, an occasion as nearly suitable as the case permits).

It must be admitted, however, that this explanation does not cover the facts of all ghost stories. There are some narratives which, if accepted in all their details, appear to admit of no explanation other than that which refers the events described to supernatural causes. But it must not be forgotten that these narratives have come in every instance from believers in ghosts and spirits; and without questioning the veracity of particular narrators, we may yet not unfairly point out that it is not absolutely impossible that at some stage or other, either in the events related or in the handing down of the story, some degree of deception may have come in. Tricks *have* been played in these matters, beyond all possibility of question. Untruths *have* been told also. The person who doubts a narrative of the marvellous is not bound to say *where* he suspects that some mistake has been made, some deception practised, some statement made which is not strictly veracious. He may not wish to say, or he may even be very far from believing, that the narrator is a trifle foolish or not quite honest. He may put faith in the persons cited as authorities for the narrative; and he may even carry his faith, as well in the sense as in the honesty of the persons concerned, a step or two farther. Yet he may still find room for doubt. Or again, he may have very little faith, and very ample room for doubt, and yet may have valid reasons for not wishing to state as much. Persons who tell marvellous stories ought not to press too earnestly for their auditor's opinion. It is neither fair nor wise.

(*To be continued.*)

SCINTILLATION.

By a Fellow of the Royal Astronomical Society.

FROM the time of the early compilers of that curious Chinese astronomical farrago, the Ma-twan-lin, through that of Aristotle, and down to the date of the

Twinkle, twinkle little star,
I often wonder what you are,

of Dr. Watts, and the yet more modern conundrum of "Why are wicked old men like the fixed stars?" (the answer obviously being, "Because they sin 'till late") we have it recorded that the scintillation of the stars has excited the notice and admiration of mankind in all parts of the known world. But familiar as this phenomenon must have been to every one who ever gazed upon the night sky, it was reserved for Arago to give the first scientific explanation of its cause. Aristotle regarded it as an effect of the straining of the eye. Kepler thought it was caused by "moving vapours." Descartes, Hooke, Huyghens, and Newton were scarcely more fortunate in their guesses; while even Young, that great physicist of the early part of the present century, to whom we are mainly indebted for the establishment of the undulatory theory of light, regarded scintillation as inexplicable. Arago, however, as we have previously intimated, did furnish a solution of the difficulty, and his interpretation of the phenomenon amounted to this: that rays of light coming from a point, like a fixed star, and traversing an atmosphere composed of strata of various temperatures, densities, and humidity, must travel with different velocities. If now we collect these rays into a focus either by the lenses of the eye or those of a telescope, the result of the rays reaching us in different phases will be apparent in the alternate brightening and darkening of the image. This can, though, only happen where the rays issue from a *point*. If we consider a large disc like that of Jupiter, the rays would issue from such innumerable points that the effects would neutralise each other, and no twinkling would be apparent. This, too, explains why we only detect the faintest telescopic stars by their scintillation. Their phases of double light alternate with those of darkness, and it is during the former that they are momentarily seen to flash up in the field of view. Scintillation, then, is certainly primarily caused by luminous interferences, and Arago went on to explain the variation in colour of a twinkling star by the assumption that if the red rays were destroyed by interference, the resulting image would be white minus red, *i.e.*, green; while, if we assume that the green rays are similarly destroyed, the remaining light will be white minus green, *i.e.*, red; and so on.

This in effect represented the sum and substance of all that was known on the subject of scintillation up to about ten years ago, at which date M. Ch. Montigny, Professor at the Brussels Athenæum, commenced his exhaustive investigation of the whole subject—an investigation which has resulted in some curious and remarkable discoveries. In order that these may be properly appreciated, we propose in the outset to describe the elegant and ingenious apparatus devised by him for his researches, and which he calls a Scintillometer. We are indebted to M. Montigny himself for the details of this which follow.

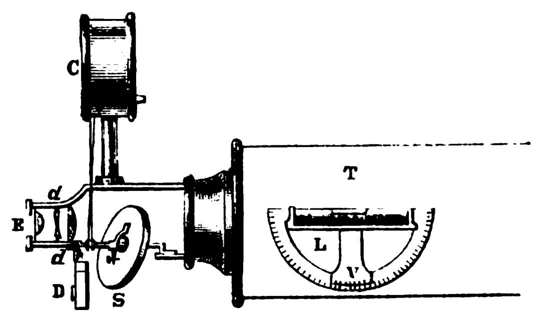


Fig. 1.

The instrument is adapted to a 3-inch telescope which carries a power of 85; and consists essentially of a disc of glass, S, Figs. 1 and 2, 1·85 inches in diameter and a quarter of an inch thick, mounted obliquely at an angle of about 17° on an axis A, close to, and in front of, the eye-piece of the telescope. In this position, the disc must always be traversed by the sheaf of rays converging from the object-glass, as it rotates round its axis, A, Fig. 2, which is parallel to and only 0·87 inch from the optical axis of the telescope. A hole perforated in the middle of the disc carries a brass ring, through which the axis of rotation passes. At that part of the axis immediately within this ring are fixed two pivots, *a*, Fig. 2, perpendicularly to it, whose ends enter two small openings pierced in the brass ring. The inclination of the disc to the axis of rotation is regulated by the brass spring, *s p*, Fig. 2, and the screw, *s c*, which latter passes through an arm tapped to receive it. A glance at our figure will show that while the spring tends to thrust the disc in a direction parallel with that of the axis A, the screw has a tendency to press it into a position at right angles to such axis; so that by the aid of these opposing pressures the disc may be fixed at any given angle of inclination. As we have said above, an inclination of about 17° is that adopted in practice. The disc is rotated by clockwork shown at C, Fig. 1, fixed on a short pedestal on the eye-tube of the telescope. This tube is shown in sec-

tion in our first figure, to exhibit the details of the fitting. The rate of motion of the clock is governed by a species of conical pendulum, or brake. An endless elastic band passes round the driving drum of the clock, and the pulley P, Fig. 2, on the axis of the disc, thus causing the disc to rotate with absolute regularity. What clock-makers call a lantern-pinion, *lp*, Fig. 2, placed in contact with the pulley, transmits the movement to a system of toothed wheels, by which the number of revolutions in a second are registered on the dial-plate D, Fig. 1. M. Montigny himself causes the disc to rotate five times in a second when scintillation is violent, and two and a-half times per second when but little twinkling is perceptible.

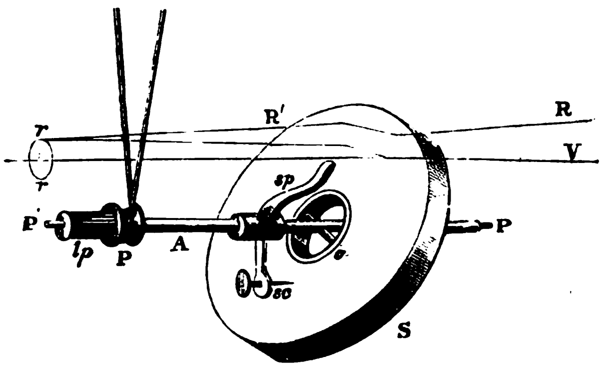


Fig. 2.

Every one knows that a luminous impression is retained for about the tenth of a second by the eye, so that a lighted stick whirled rapidly round produces the effect of a fiery circle. Upon a cognate principle a ray R, Fig. 2, emerging from the second face of the disc parallel to its original direction, will, as the disc rotates, seem to describe a circle *r r'* in the field of the telescope; so that when the telescope is turned towards a star, and the apparatus set in motion, the image of the star will be converted into a ring of light. If the star experiences no change, this ring will be homogeneous, and of an even tint; such tint being the colour of the star. But if the star twinkles, then will the circumference of this circle be divided into arcs of brilliant colours, which are continually changing. To count these arcs with the greatest exactness, M. Montigny employs a specially-devised micrometer, fixed behind the diaphragm *d d*, Fig. 1, in the focus of the eye-glass. This is shown in Figs. 3 and 4. It consists

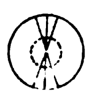


Fig. 3.

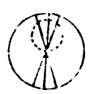


Fig. 4.

of three fine wires crossing diametrically, so as to exhibit four equal sections in the telescopic field, each of such sections being equivalent to $\frac{1}{16}$th of the whole circular space. This micrometer is lighted in the usual way. Its centre may be placed in coincidence either with the centre of the circle described by the star's image, as in Fig. 3, or with a point in its circumference, as in Fig. 4. In the first case the number of colours appearing between two given lines at any instant obviously represent the number of colours into which $\frac{1}{16}$th of the circle is broken up; that circle presenting similar colours through its entire extent. In Fig. 4 *half* the number of colours comprised between two lines gives the number of changes comprised in $\frac{1}{16}$th of the entire circumference. Combining the number of colours displayed through the whole ring with the rate of revolution of the disc of glass, we can calculate the number of colour-changes that the image of a twinkling star experiences in a second of time. Now, the numerical result thus obtained evidently indicates the *intensity* of the scintillation of a star at any given height—a height at once shown by the divided circle with its vernier V and its spirit level L, which is shown attached to the telescope T, in Fig. 1. It is thus that M. Montigny's tables are to be interpreted. So much for his elegant apparatus. Let us see briefly the principal results that he has achieved by its aid.

The intensity of a star's twinkling may vary from three causes. First, its height above the horizon; secondly, the nature of its own intrinsic light; or, thirdly, the state of the atmosphere. Other things being equal, a star twinkles less the nearer it is to the zenith, the law of such diminution being expressible thus:—Save when close to the horizon, scintillation is proportional to the thickness of the stratum of air through which the star's light passes, multiplied by the astronomical refraction at the height at which we are regarding it. This, as may be seen, enables us to convert absolute intensity measured at any given height into relative intensity; such, for example, as it would exhibit when 30° from the horizon or had 60° zenith distance, the altitude to which M. Montigny refers all his observations. This reduction is easily made by means of a table. The star's intrinsic light has considerable influence on the intensity of its scintillation, so that the spectrum of a star* will enable us to say beforehand whether it will twinkle violently or not. M. Montigny expresses this in the form of the following law: "Stars whose spectra are characterised by dark bands and black lines scintillate less than those exhibiting fine and numerous spectral lines, and much less than those which only present a few principal lines. Castor, Vega, and Sirius are examples of stars which belong to this last-named group; Secchi's first type. As a result of an enormous number of observations, the mean scintillation (found as above) of all the chief stars exhibiting this kind of spectrum is 87. Secchi's second type of stars, whose spectra show very fine lines, α Cassiopeiæ, Polaris, α Ursæ Majoris, &c., only give a mean scintillation of 79; while Secchi's third type of red and orange stars, with nebulously banded and black-streaked spectra, such as Aldebaran, α Herculis, and α Orionis, exhibit a mean scintillation of 59. M. Montigny has also observed that in calm, settled weather the trace of the star in the scintillomter is sharp, even, and regular. Under the influence of rain it becomes less sharp, thicker, and diffused. During great atmospheric disturbance, squalls of wind, and the like, the ring of light becomes fringed, broken, or beaded. The stars twinkle most in January and February, and are most quiescent in June and July. The approach of rain and gales is heralded by an alteration in the scintillation; and it is much affected by the aurora. Hence it will be seen that the scintillometer furnishes us with a means of predicting coming atmospheric changes with very considerable accuracy. One of the most remarkable results, however, at which M. Montigny has recently arrived is, that magnetic storms and violent scintillation are absolutely coincident in point of time, a result which seems to open out a new field of investigation.

Thus have we endeavoured to give an elementary idea of the labours of the famous Belgian astronomer.] It has seemed to us that those labours have been carried on in connection with a subject of which little or nothing is popularly known. May we hope that our description of

* The forthcoming papers on "Light Sifting (or Spectroscopic Analysis)," by the Editor, will thoroughly explain the meaning and signification of Star Spectra.

the simple and elegant apparatus of our Belgian *confrère* may stimulate English amateur astronomers in search of special work to provide themselves with it, and pursue the novel and interesting investigation which it was designed to facilitate?

PLEASANT HOURS WITH THE MICROSCOPE.

By Henry J. Slack, F.G.S., F.R.M.S.

Most workers with the microscope attach themselves to a particular class of objects, but no special subject can be properly understood unless general knowledge has been acquired. Some readers may not intend to study botany, but that is no reason why they should not acquaint themselves with some of the most interesting facts concerning the structure and habits of ordinary plants. At this season there are many things worth notice, and some hints may be given concerning a few. For example, while everybody accustomed to rural scenes knows the look of the catkins of the hazel, which are formed early in the autumn for service in the following year, and ripen near London in February or March, comparatively few have seen the female flowers which open at this season. They are tiny things, invisible a few yards off, but extremely pretty, with their bright crimson stigmas protruding like silk threads out of what looks like a leaf-bud, but really belongs to the inflorescence, and contains the parts that are to form the future nuts. A low power shows them well, but the hairs of their scales are best seen with about 100 linear magnification, or more. They are tapering and hollow, and, when separated, look much like some of the simpler worms.

In warm situations the male flowers, or catkins, have lost most of their pollen, but some can still be shaken out on a glass slide. The pollen grains are so small that hundreds of them may lie on the slide without making it look dusty, or being visible, unless held up to the light. When the catkin is in perfection a shake with the wind,

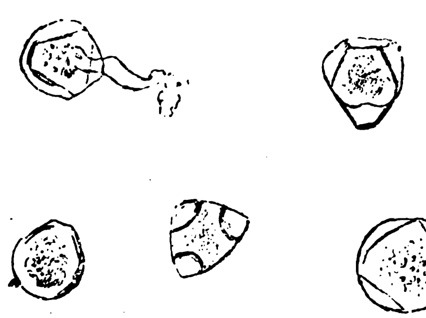


Fig. 1.—Hazel Pollen. $\frac{1}{8}$th × 320.

or with the hand, scatters the little vesicles like smoke. The figures above show some of them magnified 300 linear. The roundest are in the most perfect shape. They cannot be seen well except in some fluid, and easily alter their form. A waterdrop will do for a quick look, and dilute sulphuric acid is pretty sure to make some put forth pollen tubes from one or more of the transparent lid-like patches.

Quite another order of plants should be noticed at this season by the microscopist, namely, the allies of the fir-trees, or conifers. *Thujas*, *Cypresses*, and *Retinosporas* may be specially mentioned. They are *gymnosperms* or plants with naked ovules, and these ovules are very pretty things. Fig. 2 shows a female flower of *Thuja aurea*, a common and beautiful evergreen shrub, very broad in proportion to its height. The little bottles represented in the sketch are the ovules with open mouths. The male plants are produced at the end of other branchlets. The female flowers are now in great abundance, and their yellow, waxy colour throws a golden glow over the plant, contrasting finely with the light green of the leaves. The female flowers vary in the number of ovules; most of those I have looked at bore six, but some eight. The distinction between *gymnosperms* and the angiosperms, which contain most of the common plants, is that the former expose their ovules directly to the pollen, while the latter are provided with a pistil which catches the pollen and allows itself to be traversed by tubes which the pollen grains emit and send down to the ovary.

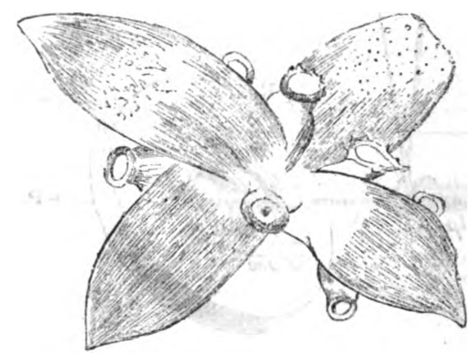


Fig. 2.—Female Flower of Thuja Aurea.

The male flowers of some gymnosperms are particularly ornamental. Those, for example, of the *Cupressus Lawsoniana* (Lawson's Cypress) form, when ripe, a beautiful red coral-looking fringe to the branchlets. Mine are now black; but in warmer places than Ashdown Forest they have probably turned red, as mine will be in a few weeks. The *Retinosporas* are much decorated with these flowers. *R. leptoclodium* is now full of terminal flowers, contrasting with its green leaves. All the conifers and their allies will supply something interesting for the possessor of a microscope to watch for many weeks to come. The enormous quantity of pollen produced by the fir tribe is very striking. Plants depending upon wind fertilization always form vastly more than can be used in the work of impregnation, but we are not entitled to suppose it lost. A continuous change of matter from the inorganic to the organic condition, and back again in a similar circuit, seems necessary to the order of nature.

Among quite other plants interesting to the microscopist are the *Deutzias scabra* and *gracilis*, both common in good gardens south of London. The latter as a greenhouse plant has seen its best days, but in the open border it has only formed young leaves and early buds. The youngest leaves may be soaked for a few days in strong methylated spirit, to dissolve out the chlorophyll, and then, after cutting out the midrib, mounted in Canada balsam. Prepared this way, it has some advantage over the cuticle of *D. scabra*. It should be viewed with polarised light, and selenite or mica, so arranged as to show the starlike hairs on a dark chocolate ground. It is not a good object on most of the light grounds. The very young buds of *D. scabra* are worth looking at. The "Treasury of Botany" especially alludes to *D. Staminea* as sought for on account of stellate hairs, but I have not got it and do not know it.

Darwin's Berberry is in flower, and its anthers should be examined. When the pollen is ripe, two flaps, or trapdoors, open, and stand out from the filament like two arms held away from the body. Early rhododendrons are, or

will be soon, in blossom, and their anthers are elongated bags, with two round openings at the top.

Bay-trees are coming into flower, and their anthers have two lids, and the filaments have wing-like projections.

The oval pollen of the blue squill, *Scilla siberica*, and the spiral fibres attached to the filament, are also good objects.

These are a few of the things to be noticed, but the list might be extended to any length. Young people, Sir John Lubbock says, are fond of natural history, until the taste is educated out of them. Certainly, too much "poring over miserable books," and neglect of using the eye and its optical assistants, leads to stupidity, though it may help to pass Cambridge examinations.

INTELLIGENCE IN A POINTER.

DON was a pointer dog of large experience that I shot with over forty years ago. At that time the pinnated grouse were abundant in our wild prairies. The birds were more frequently found in particular localities in different parts of the day. They affected the low grounds or swales, where the grass was long, in the heat of the day, and in the morning and evening they resorted to the high, rolling prairie. In a cool, cloudy day they were likely to remain on the high grounds. Don had learned this as well as his master, and when taken into the field it was interesting to observe the dog, when on the prairie, deliberately surveying the ground and then start out and range over the same ground his master would have selected.

This was the result of education and observation, and was not peculiar to Don. I have known many old bird dogs do the same. But there was one thing which Don had not been taught in training, but which he had adopted of his own motion, as the result of his own reasoning or reflective powers.

When a young bird gets separated from its companions, or it may be is the last of a covey which has escaped the fowling-piece, it is apt to wander a good deal, and the dog may follow its trail for a considerable distance. When pressed it will seek short cover if convenient, and there the trail is frequently lost. In such cases I have frequently seen Don hunt about rapidly and irregularly for a short time, and if still unsuccessful he would return to the last point where he could detect the scent, and then commence to hunt in a circle, enlarging the circle by perhaps two feet in the radius, and these were made with wonderful regularity till he had covered the ground for many rods around, if the want of success required it, but he generally found the bird within ten or twelve feet of the starting-point. I once knew him to go over the ground a second time in the same way before he was successful.

Invariably the bird was found to have concealed itself in a deep narrow depression in the prairie.

I repeat that the dog had not been taught, but had adopted it voluntarily, or, it was his own invention.

I never knew any other dog to do this, although a book could be filled with accounts of smart things which bird dogs have been known to do.—J. D. CATON, in *The American Naturalist.*

NOTES ON MAPPING.

By RICHARD A. PROCTOR.

IT is hardly necessary to remark, perhaps, that what I have said respecting star-maps is true of geographical maps also. The conical projection described in my former paper is undoubtedly the most satisfactory for presenting regions of the earth of moderate extent; and is far preferable to the constructions employed in our atlases even for showing continents. It is difficult to understand how these preposterous constructions came first to be employed, or why, having once been employed, they have not long since disappeared, making way for one or other of several constructions which are far better suited for the purpose in view.

Let us take for instance the continent of Africa, though this is not by any means the worst case. Africa is always presented on the projection known as Flamsteed's, as also is South America; while Europe, Asia, and North America are presented on a singular mixture of Flamsteed's projection and the conical projection, reflecting more credit on the inventor's ingenuity than on his common sense, since it combines the defects of both systems, and has none of the good qualities of either.

Taking Africa for the application of Flamsteed's system, we proceed as follows:—We first draw a series of equidistant parallels to represent the parallels of latitude ten degrees (say) apart, and a line crossing them at right angles to represent the central meridian of our map. Then from the points in which this line crosses the parallels we measure off along each parallel a series of distances each corresponding to ten degrees of longitude on the parallel we are dealing with. Thus along the equator these distances are equal to that between the parallels first drawn; in latitude 40 north or south they are only equal to 766 thousandths of that distance; and so on for each latitude parallel. Then we join the points thus obtained by a series of curved lines forming the meridians on either side of the central straight one. It is unnecessary to give an illustrative figure, for the simple reason that every atlas map of Africa will serve. If the reader examines the corner spaces in such a map and compares them with the mid-spaces between the same parallels, which are nearly of the right shape, he will see how great the distortion is. Then the distortion is widely variable in different parts of the map—always a serious defect. The scale not only varies in different parts of the map, moreover, but it is widely variable in different directions. A scale of miles is complacently given, which is untrue for large parts of the map—except in measuring directions parallel to the equator.

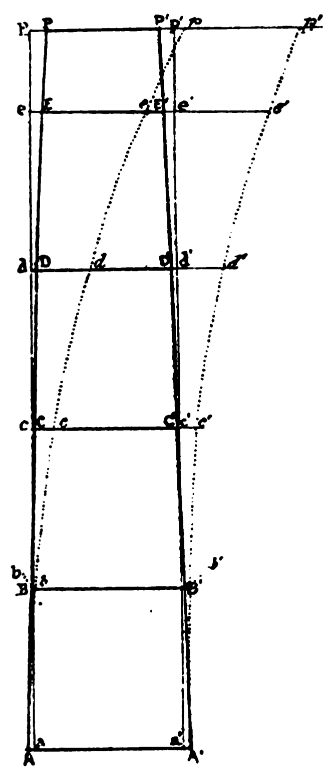


Fig. 9.

The actual distortion near a corner of one of the Atlas maps of Africa is illustrated in Fig. 9. Here P E D C B A and P' E' D' C' B' A' are two meridians, crossed by the equator at A A', and successive parallels of latitude, 10° apart, in B B', C C', D D', and E E': *p e d c b* and *p' e' d' c' b'* are two such meridians near the left side (north of equator) of a map of Africa.

In the same Fig. 9 is shown the shape of the same portion of the map, if the conical construction, which at the

equator becomes cylindrical, is adopted. The meridians here assume the position p e d c b a and p'e'd'c'b'a', whose distance apart is made nearly equal to that corresponding to the mean between the length of ten degrees for the equator and for latitude 35° north. (This is rather better from objection. The areas increase on either side of the equator as shown in Fig. 9; but not much within the limits of Africa itself. Preposterous changes of shape are avoided, though equatorial regions are slightly compressed and regions north of 15° slightly expanded in latitude.

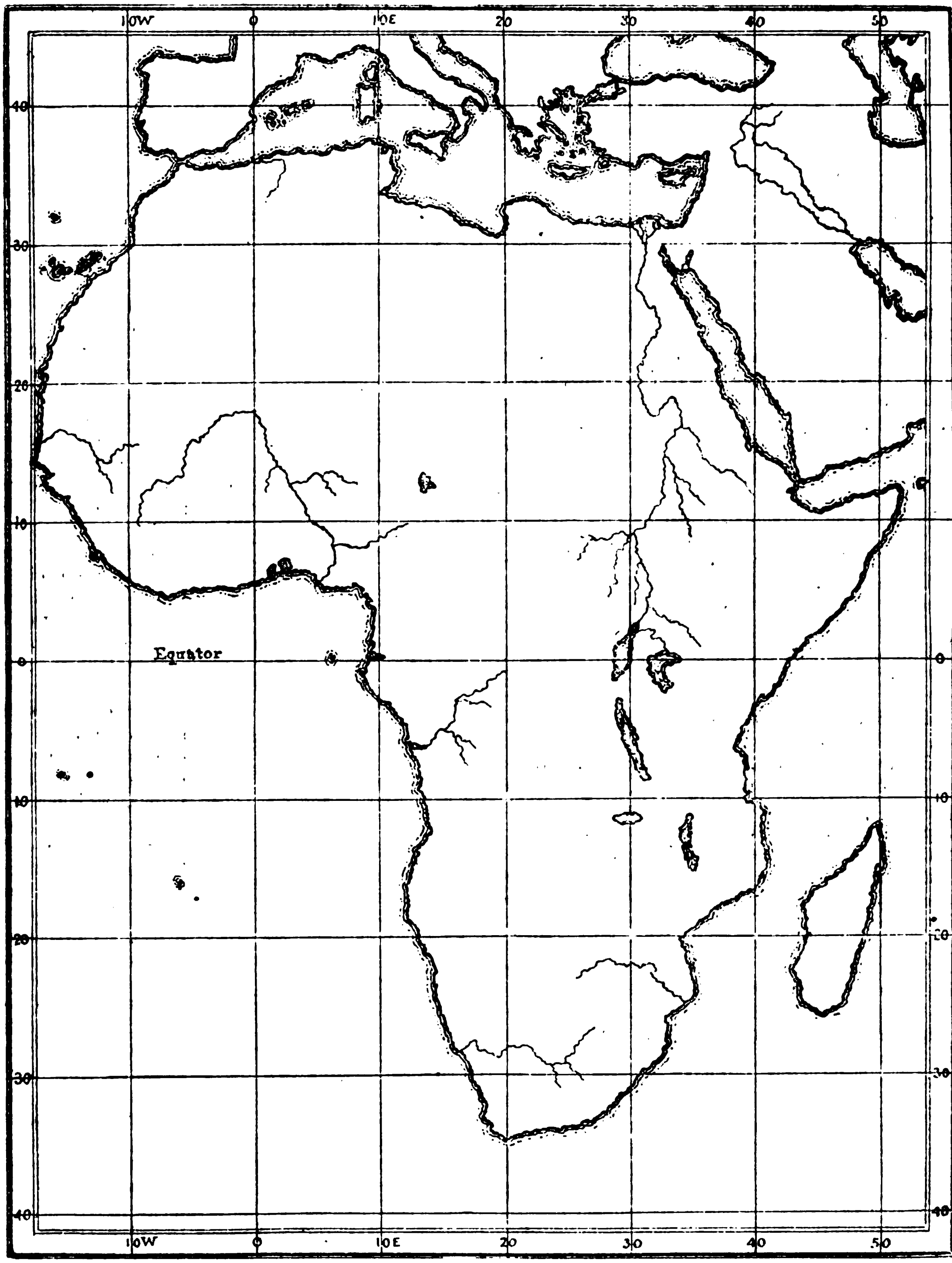


Fig. 10.

than taking the mean between 0° and 40°, because the greater part of the outline of Africa falls between the parallels 35° north and south of the equator.)

The cylindrical projection is far superior to Flamsteed's for a map of Africa, though by no means absolutely free

I have drawn a little map of Africa (Fig. 10) on the cylindrical projection, so that the student may compare the shape of the continent as thus mapped with the shape given in atlas maps. Perhaps the best evidence of the superiority of the cylindrical projection, (which is also far simpler in

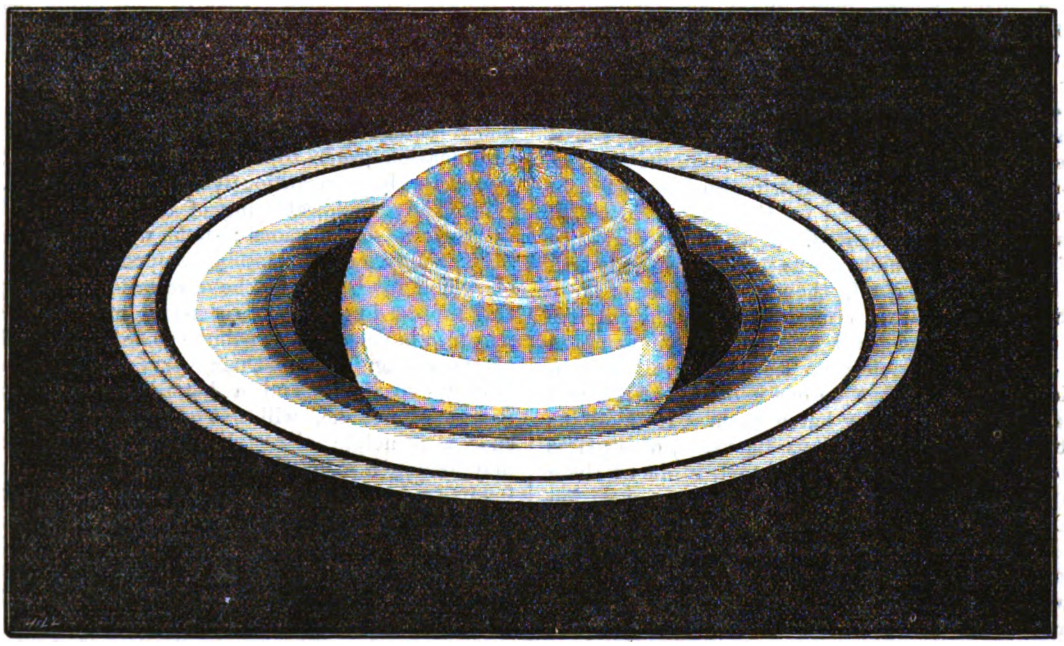
c nstruction) will be found on comparing the figures of Spain, Arabia, and the Caspian Sea with the real shapes on the one hand, and with the distorted figures of these regions in the atlas maps of Africa.

I propose to take an early opportunity of giving a map of Africa on the stereographic projection, and then one on the equidistant construction, both of which are far better for so large a region than even the cylindrical,—though this, as can be seen by Fig. 10 is far superior to Flamsteed's.

(*To be continued.*)

A FEW SATURNINE OBSERVATIONS.*

HERE is a gentleman at our doors, Mr. R. A. Proctor, who has written a book upon that planet Saturn, and he asks us to stroll out in his company, and have a look at the old gentleman. It is a long journey to Saturn, for his little place is nine and a-half times further from the sun than ours, and his is not a little place in comparison with our own tenement, because Saturn House is seven hundred and thirty-five times bigger than Earth Lodge.



The people of Earth Lodge made Saturn's acquaintance very long ago; nobody remembers how long. Venus and Jupiter being brilliant in company, may have obtruded themselves first upon attention in the evening parties of the stars, and Mars, with his red face and his quick movement, couldn't remain long unobserved. Saturn, dull, slow, yellow-faced, might crawl over the floor of heaven like a gouty and bilious nabob, and be overlooked for a very little while, but somebody would soon ask, "Who is that sad-faced fellow with the leaden complexion, who sometimes seems to be standing still or going backwards?"

He was the more noticeable, because those evening parties in the sky differ from like parties on earth in one very remarkable respect as to the behaviour of the company. We hear talk of dancing stars, and the music of the spheres, but, in fact, except a few, all keep their places, with groups as unchanging as those of the guests in the old fabled banquet, whom the sight of the head of Medusa turned to stone. Only they wink, as the stone guests probably could not. In and out among this company of fixtures move but a few privileged stars, as our sister the moon and our neighbours the planets. These alone thread the maze of the company of statues, dancing round their sun, who happens to be one of the fixed company, to the old tune of Sun in the middle and can't get out. Some of the planets run close, and some run in a wide round, some dance round briskly, and some slip slowly along. Once round is a year, and Saturn, dancing in a wide round outside ours, so that in each round he has about nine times as far to go, moves at a pace about three times slower than ours. His year, therefore, is some twenty-seven times longer; in fact, a year in the House of Saturn is as much as twenty-nine years five months and sixteen days in our part of the world. What, therefore, we should consider to be an old man of eighty-eight, would pass with Saturn for a three-year-old.

A hundred and fifty years ago, Bishop Wilkins did not see why some of his posterity should not find out a conveyance to the moon, and if there be inhabitants, have commerce with them. The first twenty miles, he said, is all the difficulty; and why, he asked, writing before balloons had been discovered, may we not get over that? No doubt there are difficulties. The journey, if made at the rate of a thousand miles a day, would take half a year; and there would be much trouble from the want of inns upon the road. Nevertheless, heaviness being a condition of closeness and gravitation to the earth, if one rose but the first twenty miles, that difficulty of our weight would soon begin to vanish, and a man—clear of the influence of gravitation—might presently stand as firmly in the open air as he now does upon the ground. If stand, why not go? With our weight gone from us, walking will be light exercise, cause little fatigue and need little nourish-

* I have often been asked whether this paper, which appeared in *All the Year Round*, on Sept. 2, 1865, was written by Charles Dickens. I have no means of knowing; though it is tolerably easy to form an opinion from internal evidence. Now that Saturn is departing from our skies I have thought readers of KNOWLEDGE might be interested to read this quaint paper, the first notice of importance which my first book received from the public press. It is illustrated by a view of Saturn taken from that work of mine, showing the planet as seen on March 23, 1856, and as he appears at present and will appear again next year.—R. A. PROCTOR.

ment. As to nourishment, perhaps none may be needed, as none is needed by those creatures who, in a long sleep, withdraw themselves from the heavy wear and tear of life. "To this purpose," says Bishop Wilkins, "Mendoza reckons up divers strange relations. As that of Epimenides, who is storied to have slept seventy-five years. And another of a rustic in Germany, who, being accidentally covered with a hayrick, slept there for all autumn and the winter following, without any nourishment." Though, to be sure, the condition of a man free of all weight is imperfectly suggested by the man who had a hayrick laid atop of him. But what then? Why may not smells nourish us as we walk moonward upon space, after escape from all the friction and the sense of burden gravitation brings? Plutarch and Pliny, and divers other ancients, tell us of a nation in India that lived only upon pleasing odours; and Democritus was able for divers days together to feed himself with the mere smell of hot bread. Or, if our stomachs must be filled, may there not be truth in the old Platonic principle, that there is in some part of the world a place where men might be plentifully nourished by the air they breathe, which cannot be so likely to be true of any other place as of the ethereal air above this? We have heard of some creatures, and of the serpent, that they feed only upon one element, namely, earth. Albertus Magnus speaks of a man who lived seven weeks together upon the mere drinking of water. Rondoletius affirms that his wife did keep a fish in a glass of water without any food for three years, in which space it was constantly augmented, till at first it could not come out of the place at which it was put in, and at length was too big for the glass itself, though that were of large capacity. So may it be with man in the ethereal air. Onions will shoot out and grow as they hang in common air. Birds of paradise, having no legs, live constantly in and upon air, laying their eggs on one another's backs, and sitting on each other while they hatch them. Rondoletius tells, from the history of Hermolaus Barbarus, of a priest who lived forty years upon mere air. And, if one of these possibilities be admitted, why, we can take our provision with us. Once up the twenty miles, we could carry any quantity of it the rest of the way, for a ship-load would be lighter than a feather. Sleep, probably, with nothing to fatigue us, we should no longer require; but if we did, we cannot desire a softer bed than the air, where we may repose ourselves firmly and safely as in our chambers.

As for that difficulty of the first twenty miles, it is not impossible to make a flying chariot and give it motion through the air. If possible, it can be made large enough to carry men and stores, for size is nothing if the motive faculty be answerable thereto—the great ship swims as well as the small cork, and an eagle flies in the air as well as a little gnat. Indeed, we might have regular Great Eastern packets plying between London and No Gravitation Point,* to which they might take up houses, cattle, and all stores found necessary to the gradual construction of a town upon the borders of the over-ether route to any of the planets. Stations could be established, if necessary, along the routes to the Moon, Mars, Venus, Saturn, and the rest of the new places of resort; some London Society could create and endow a new Bishop of Jupiter; and daring travellers would bring us home their journals of a Day in Saturn, or Ten Weeks in Mars, while sportsmen might make parties for the hippogriff shooting in Mercury, or bag chimeras on the Mountains of the Moon.

(To be continued.)

* But alas, this point would be not 20, but 150,000 miles away.—R. P.

THE UNIVERSE OF SUNS.

By Richard A. Proctor.

(Continued from page 146.)

It was not however until 1811—nine years later be it noticed, and more than a quarter of a century after the examination of the theory of 1785, that Herschel first propounded a definite theory concerning the milky nebulosity. I have elsewhere had occasion to refer to his analysis of the various orders of nebulæ, in the masterly essay of 1811, and shall here therefore merely give a summary of his ideas. But before doing so it is essential to quote the words with which Herschel opens this memoir, because they will serve to remove any lingering doubts which the reader may still entertain respecting the truth of my assertion that Herschel's ideas underwent a marked change during the later part of his career as an observer.

"I must freely confess," says Herschel, speaking of the disagreement between his new views and those he had formerly entertained, "that by continuing my sweeps of the heavens my opinion of the arrangement of the stars and their magnitudes, and of some other particulars has undergone a gradual change; and indeed when the novelty of the subject is considered, we cannot be surprised that many things formerly taken for granted, should on examination prove to be different from what they were generally but incautiously supposed to be. For instance, an equal scattering of the stars may be admitted in certain calculations; but when we examine the Milky Way, or the closely compressed clusters of stars, of which my catalogues have recorded so many instances, this supposed equality of scattering must be given up. We may also have surmised nebulæ to be no other than clusters of stars disguised by their very great distance; but a longer experience and a better acquaintance with the nature of nebulæ, will not allow a general admission of such a principle, although undoubtedly a cluster of stars may assume a nebulous appearance when it is too remote for us to discern the stars of which it is composed."*

Herschel then proceeds to exhibit his ideas respecting the gradual evolution of stars from truly nebulous matter. He shows that diffused nebulosity exists in great abundance over the heavens, insomuch that notwithstanding the extreme difficulty of detecting it he recognised its presence over more than 150 square degrees of the heavens, "its abundance," as he said, "exceeding all imagination." He shows that this diffused nebulosity is often associated with real nebulæ, citing as a particular instance the great nebula in Orion; and that often detached nebulosities of this kind can be recognised. He points out that such detached nebulosities when small must be classed as nebulæ. Then he mentions instances in which nebulæ of this sort—that is, milky nebulæ—show signs of condensation, either in one place or in several. Regarding these signs as indicating the formation of one or more nebulæ from a region of diffused nebulosity, Sir W. Herschel is careful to show that the idea derives support from the remarkable association of many nebulæ which are thickly spread in cer-

* It will be observed that in this passage Herschel abandons two of the views he had formerly entertained,—the general uniformity of stellar distribution, and the theory that all the nebulæ are systems of stars. Presently I shall have occasion to show that yet a third essential point in the views of 1785 was abandoned by Herschel quite as definitely; but I would invite special attention to the circumstance that *each* of the two views here given up, was of itself essential to the theory of 1785. Two links of the chain of ideas then enumerated were in effect rejected by Herschel, in 1811, as broken (under the strain of observations); so that the chain itself was broken.

tain regions of the heavens, while other regions are as remarkably free from nebulæ. He next considers the form of the discrete nebulæ, describing some which appear to be extended only in one direction, others that have length and breadth but no great thickness, others that are irregular, and others which have an irregular round figure. He infers that these various figures indicate the successive figures which one and the same nebula assumes during the process of condensation, the further effects of this process being illustrated, in order;—by round nebulæ; by nebulæ that are gradually a little brighter in the middle; brighter in the middle; much brighter in the middle; and suddenly much brighter in the middle; by nebulæ increasing gradually in brightness up to a nucleus; by nebulæ that have a well-marked nucleus; by planetary nebulæ which are uniformly bright but whose light "must be considerably more condensed than that of the foregoing sets*"; and lastly by nebulous stars more or less clearly recognisable as such, those only barely to be distinguished from stars being but one stage from the final stellar condition to which the whole series has tended.

Such briefly stated is the famous theory of the formation of stars from nebulous matter. It will be observed that Herschel here extends (and also illustrates from the heavens themselves), the ideas he had first enunciated in 1789. He pictures every stage of the progress of such matter from its most diffused state up to the last state in which the condensing nebula can be distinguished from the star itself; and he remarks that there is perhaps not so much difference between these different states, as there would be in the condition of the human frame from one year to another, were it described "from the birth of a child till he comes to be a man in his prime." "The end I have had in view," he says, "has been to show that the above-mentioned extremes may be connected by such nearly allied intermediate steps as will make it highly probable that every succeeding state of the nebulous matter is the result of the action of gravitation upon it while in a foregoing one, and by such steps the successive condensation of it has been brought up to the planetary condition. From this the transit to the stellar form, it has been shown, requires but a very small additional compression of the nebulous matter, and several instances have been given which connect the planetary to the stellar appearance. The faint stellar nebulæ have also been well connected with all sorts of faint nebulæ of a larger size; and in a number of the smaller sort, their approach to the starry appearance is so advanced, that in my observations of many of them it became doubtful whether they were not stars already."

In the memoir of 1811 Herschel does not discuss the condition of star-clusters of any orders. In fact, as he points out, the paper does not deal at all with the sidereal part of the construction of the heavens. We must pass on to the paper of 1814 to ascertain the direction in which Herschel's ideas on this part of his subject had been tending since he had first seen reason to question the ideas of 1785. It will be well to quote Herschel's own account of his purpose in the memoir of 1814, since in this case as in many others his meaning has been misapprehended.†

"The observations contained in this paper," he says, "are intended to display the sidereal part of the heavens, and also to show the intimate connection between the two opposite extremes, one of which is the immensity of the widely diffused and seemingly chaotic nebulous matter; and the other the highly-complicated and most artificially constructed globular clusters of compressed stars. The proof of an intimate connection between these extremes will greatly support the probability of the conversion of one into the other." It will be observed that Herschel here definitely describes as the extremes of observable creation, the self-luminous nebulous matter on the one hand and the compressed globular clusters on the other.

He then proceeds to show how stars are sometimes "remarkably situated with regard to milky nebulosity," citing specially double stars connected by nebulous matter, stars with nebulous appendages, and those nebulous stars which closed the series considered in the Memoir of 1811. Furthermore there are stars connected with extensive windings of nebulosity, as well as "small patches consisting of stars mixed with nebulosity." "Admitting from what has been said," he remarks, "that stars may be formed of nebulous matter, it may happen that the nebulosity still mixed with them is some remaining unsubsided part of that from which they were formed;" *or* "a union of stars and nebulosity, originally at a distance from each other, may have been effected by the motion of either the stars or the nebulosity." "Every nebulosity which is carried into the region of a small patch of stars will probably be gradually arrested and absorbed by them, and thus the *growth of stars* may be continued." This part of the paper, forming a connecting link, as it were, between the subjects of the Memoirs of 1811 and 1814, concludes with the consideration of objects of an ambiguous construction,—that is, "of such a construction or at such a distance from us that the highest power of penetration which hitherto has been applied to them, leaves it undetermined whether they belong to the class of nebulæ or stars."

For much of what relates to the sidereal heavens, Herschel refers to the paper of 1785, and it may be that this reference has presented most of his commentators from noticing that his ideas as to the distribution of the stars, differ markedly in 1814 from those he had adopted in 1785. He refers back to the earlier paper only with regard to individual suns. So soon as he deals with the aggregation of the stars, we find that though he still refers to the star gauges of 1785 he interprets them on principles altogether different from those he had formerly adopted. He dwells afresh on what he had written in 1802 respecting the clustering condition of portions of the stellar heavens. He explains that his expression "forming clusters," was used "to denote that some peculiar arrangement of stars in lines making different angles, directed to a certain aggregation of a few central stars, suggested the idea that they" (the former) "might be in a state of progressive approach to them" (the latter). "This tendency to clustering seems chiefly to be visible in places extremely rich in stars. In order, therefore, to investigate the existence of a clustering power, we may expect its effects to be most visible in and near the Milky Way." I would invite the reader's special attention to the circumstance that the Milky Way is here pointedly referred to as a stellar region distinct in its characteristics from the region of the stars forming our constellations. In studying Herschel's papers we have to be continually on the watch for indications of the sort; since he does not always judge it necessary to make definite assertions of his views on such points.

He then describes irregular clusterings of stars, noting

* This is the weakest part of Herschel's chain of reasoning. It is not easy to recognise the systematic nature of a process of change, which after converting round nebulæ of uniform lustre into nebulæ with a nucleus leads next to the small round planetary nebulæ of uniform lustre, and thence to nebulæ with a star for nucleus.

† Not from any real indistinctness in his mode of expression but (here as in other cases) because the attempt has been made to reconcile the views expressed at one time with views expressed much earlier, and when as yet the available evidence had not been even collected, far less examined.

in particular that "though they are in general very promiscuously scattered they are yet sufficiently drawn together to show that they form separate groups," while in many places a falling off in the number of stars surrounding the clusters indicates "a tendency to future insulation." "*Those which are in, and very near the Milky Way,*" he says, "*may be looked upon as so many portions of the great mass drawn together by the action of a clustering power, of which they tend to prove the existence.*"* In describing the various orders of irregular clusters, Herschel is particular to notice *rows* and *streams*, *ridges* and *shelvings* of stars, as indications of a preponderating clustering power.

(*To be continued.*)

* The passage I have here emphasised deserves as careful study as the passage similarly emphasised in the paper of 1802. It is as obviously inconsistent with the theory of uniform distribution of stars throughout the galaxy.

TRICYCLES IN 1884.

BY JOHN BROWNING,

Chairman of the London Tricycle Club.

TRICYCLE EXHIBITION AT THE AGRICULTURAL HALL.

THE Lecture at the Royal Institution on "Cycling" was happily timed, coming just at the beginning of the riding season, and during the Exhibition at the Agricultural Hall, which was the largest exhibition of tricycles yet seen. I trust that all my readers who are interested in tricycles will have taken the opportunity of inspecting this great collection of machines, as it was closed on Saturday last the 15th inst. There were many novelties at this show which were not exhibited at the Floral Hall in February, notably three new two-speed gearings—one exhibited by Leni, one by Rucker, and one by Rudge. They are all modifications of the "Sun" and "Planet" gearing, and all of them are good—so good that only a careful trial would enable any one to pronounce an opinion as to which was the best.

I only propose to enumerate here a few of the most novel or important exhibits; very shortly I hope to be able to give more lengthened descriptions of the machines of the greatest value.

First, a few words on bicycles, because any improvement in those machines will almost certainly be found to be applied very soon to tricycles. One exception to this rule I must name, on account of its simplicity, originality, and efficiency—Rucker's "Tandem Bicycle," which is undoubtedly the fastest machine yet contrived. It is a bicycle with two large wheels, only the two riders both steer. At first sight it appears a dangerous machine, but I am assured that it is really safer than an ordinary bicycle, because it is not possible for either rider to be thrown off from it over the handles.

The new "Safety" bicycle introduced by Robinson & Spence (?) is a bicycle in which, by dispensing with the cross-bar handles, it is believed that the liability to headers may be obviated. An open front is gained in a very ingenious manner by carrying the ends of the large hollow forks down under the riders' feet, and then bringing them up in the form of the letter U, the handles being on the outside of the U. The inventors have immediately applied this contrivance to the tricycle, giving us a "Humber," with an open front, though the machine is not so graceful in appearance as the original. The "Safety" bicycle is clumsy in appearance, and must be heavier by several pounds than machines of the usual construction, but its undoubted advantage on the score of safety may lead to its adoption.

Humber's new "Tandem" tricycle is also a light and very fast machine, probably faster than an ordinary bicycle. It is simply the well-known "Humber" tricycle, with a seat for a second rider in front of the handles. This rider drives by a chain a wheel placed on the axle. The machine can very easily be converted into the single form, leaving only the chain wheel, weighing but 1 lb. at the utmost, upon the axle. The machine requires more skilful management than an ordinary "Sociable," in which the two riders sit side by side, but I am told that it is easier to ride and steer than the single "Humber" tricycle. Very soon I shall have an opportunity of riding it, and then I will give an account of my experience as to its performance.

Keen's "Eclipse" tricycle is a central-geared front-steerer, well planned and very light. The double-driving gear is in the centre, and is open so that it can be oiled or cleaned at pleasure.

The "Grosvenor" is a machine introduced by Hart, Son, & Peard, of Regent-street, based on the "Coventry," having only one large driving-wheel and two steering-wheels, both on the same side. One small steering-wheel stands well out in front, and the other is just behind the rider. The steering arrangement is very neat and effective, being entirely contained within the side-bar, and the machine has an open front. The work is good, and the price reasonable. I should be glad to know more of this machine than I could gather at the Show.

I was greatly pleased to see, at last, some sensible tricycles for children. Scantlebury has the cheapest. His machines at fifty shillings have a double-tyre break; while some makers display machines at more than double the price with a break *on one wheel only*. Lloyd had some children's machines of superior make, highly finished, and nickelised, with good brakes, at the price of £5; while Singer's showed a reduced model of their excellent front steerer, the "Apollo," at ten guineas, which would serve for any lad or lass up to the age of fifteen or sixteen.

Before concluding, I would direct attention to Lucas's admirable "King of the Road" lamps, which are the most complete and perfect I have yet seen. They are of great illuminating power, easy to light, difficult to be blown out, and, though small, will burn without refilling for eight hours.

"LET KNOWLEDGE GROW FROM MORE TO MORE."

(*In Memoriam.*)

WHO shall compass Knowledge?
  Who with straining ear shall blend
From minds of men, and wood, and wave,
  The harmony her movements lend?

Time shall bring us Knowledge:
  Time, whose fost'ring formed the earth,
Marked the shores her waters lave,
  Gives the bud the rose's worth.

In the growth of ages
  Knowledge shall reveal her might;
She shall read the universe
  In her twin-born sister's light:

In the radiance wisdom sheds,
Knowledge comes, and Virtue spreads.

HELEN AUXILIUM KING.

Gossip.

WE are glad to see that Dr. Andrew Wilson is to give the four lectures of the course which he delivered last Christmas in Princes' Hall, Piccadilly, at the Guildhall, Plymouth. I have a lively recollection of the welcome extended to me last autumn in that splendid hall, and should be glad to hear that the Editor of *Health* had been as kindly greeted as the Editor of KNOWLEDGE. His subject should prove even more generally interesting. For while we necessarily take but an outside interest in sun, moon, and stars, we are all vitally interested in the structure and mechanism of our own bodies. In these days when every one is learning not only how to take care of his own personal body, but to look after his fellow-creatures' bodily welfare in a scientific way, we cannot but follow with interest the teachings of medical men.—our Richardsons, Carpenters, Wilsons, and so forth. With care and time will come the proper application of the facts which they bring to the knowledge of the lay community.

WE heard a few days since, by the way, of a rather odd development of surgical teaching. A class of ladies, students of the Ambulance Corps, had been carefully taught how, by means of a tied handkerchief, pad, and a ruler (to twist and so tighten the handkerchief), the bleeding of an artery in the arm or leg may be stopped. The remedy, unscientifically applied, is a little dangerous, and we recently heard of a case in which where no artery had been wounded, strong but unnecessary pressure had nearly caused mortification. Be this as it may, the extension of the method to the case next to be considered was certainly unexpected and perhaps mistaken:—"How would you proceed," the ladies were asked afterwards in examination, "in the case of a person bleeding from a bad wound in the head?" "I would tie a handkerchief round the neck," came the answer, "apply a pad to the throat, and with a ruler inserted under the knot at the back of the neck, I would tighten the handkerchief until the bleeding ceased." The remedy would be undoubtedly decisive.

RATHER than have such experiments tried one would almost prefer the spread of such happy ignorance among laymen as was indicated in the reply (really made) to the question, "Describe the circulation of the blood?"—"The blood goes down one leg and up the other."

SIR GEORGE AIRY, by the way, calls attention to the mistake of describing as "arterial drainage" a system which if named by reference to the circulation should be called "venous."

RETURNING to the subject of lectures, I may take this opportunity of expressing my sense of the kindly welcome extended to me in Gloucestershire and Somersetshire during the past few weeks. I shall never forget the splendid audiences in the Greater Colston Hall, at Bristol, or the pleasant warmth of their greeting. At Bath and Cheltenham, again, my whole course of six lectures was attended by crowded and kindly audiences. And at Taunton, one of the brightest and pleasantest places in England, I was similarly welcomed, though naturally, with a much smaller population, the audiences were smaller also. At Gloucester, only, were the audiences below par,—Gloucester in fact gave an audience smaller in proportion than any place except Reading which I have yet visited. Probably the progress of the Gilchrist lectures, with penny admissions, may have exerted a demoralising influence. Certain is it that the manager of the course was asked for half-price on school admissions already reduced to sixpence. But the local press made up for this, I am told, by asking (so at least did the generous *Citizen*) for fifty per cent. above their customary advertising rates. This gave an air of dignity to the proceedings at Gloucester which may be better imagined than described.

WESTON'S walk shows that in the open air a man can continue steady work, not very heavy while it lasts, for ten or twelve hours per day, six days out of seven, in the open air, for seventeen weeks, without taking any alcoholic stimulant. Among our working classes are many who continue steady work, harder while it lasts, for more than twelve hours per day, with less complete rest on Sundays, in badly-ventilated places, for years together, some of them without any alcoholic drinks, and others, again, without suffering appreciably (so far as appears) from a glass of beer, or of spirits and water, daily. Now, if some one would try the following experiment, the real danger of even the moderate use of stimulants would, I fancy, be more clearly made apparent. Let two men of equal strength, and in equally good condition, be for twelve weeks without exercise or with insufficient exercise, and let one of them during that time take daily such an allowance of alcoholic stimulant as men living actively and much in the open air can take without harm; let the other takes none: then let the conditions of these men be tested by medical experts and compared with their condition before the twelve weeks began. I fancy the former will be found to have suffered much more than the latter, though both will no doubt have lost condition.

PEOPLE who know nothing about the phenomena of so-called mesmerism are apt to be as unwise in their incredulity as some who have witnessed the performances of mesmeric charlatans have been in their gullibility. Mr. Langley has done good service in showing at the Royal Institution that certain mesmeric influences are demonstrable, for he mesmerised the unhappy frog (always invited to these entertainments), and, by way of change, a lively alligator. He was probably not far from the truth in saying that human beings cannot be mesmerised against their will, though of course multitudes of instances seem to prove the reverse.

I HAD no idea that the writer who had told Mr. Sala to be silent about matters English because he was not of English blood would ever see the pages of KNOWLEDGE, and still less had I any particle of personal feeling against him. Mr. Sala only gave us one sentence from a series which he described as a perfect string of pearls, and that sentence was so ludicrously pompous and stilted that it seemed natural to imagine the writer to correspond. But I really am sincerely sorry that regarding him in this absolutely impersonal way, I spoke of him as a solemn idiot; for he turns up *in propriâ personâ*, and as a man of science (he says), with a very natural wish to castigate me in bodily conflict, where his "complete victory over" me "would be hallowed by the cheers of assembled thousands gathered to view the tourney, while" he, "doffing his casque to the acclamations of the people would have presented on" his "lance's point" my "bleeding and lacerated head, as a fitting offering to the lady of" his "choice. But alas, we are fallen on degenerate days!" Without doubting that the lady of *his* choice would find a bleeding and lacerated head a cheerful gift, and be able to make pleasing use of

the same, I may remark that I should have declined the contest on the plea that I had no right to call my admirer a "solemn idiot" without better knowledge of him. If, after that admission, and regretful apology, he insisted on the fight, then truly should I have taken heart of grace, and have done my endeavour to keep my head unlacerated and therefore unbleeding, as being in that state more pleasing to the lady of *my* choice. My correspondent, adopting the title of "A Solemn Idiot," which really seems unsuitable for one of such positive character (idiocy being purely negative), recommends that to correct the large proportion of non-English blood in their veins (if royal persons *have* veins) our royal families should immediately begin to "intermarry with families of English blood and noble birth, whose ancestors have fought for their country, and whose modern representatives belong to the right side in politics, that is to say, the Fourth Party." If "his will were paramount," he would perhaps bring this about, but, then, it is not: there is much virtue in your "if."

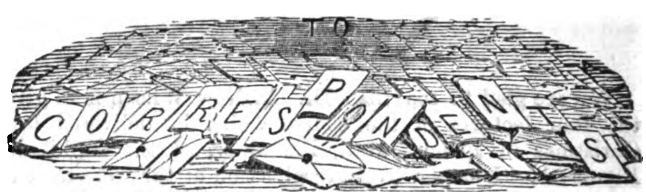
I REJOICE to see from a pamphlet sent me by Mr. Irving that the Lyceum Company has won esteem throughout America. It is pleasant to think that while this is so, the Lyceum is filled nightly by audiences which crowd to do honour to the charming actress (in her own paths incomparable) who has visited us from America.

THE Patents Department of the Bethnal Green Free Library is now open for the free use of the public daily from 11 a.m. till 3 p.m., and from 6 till 10 p.m. G. F. HILCKEN, *Librarian.*

THE following figures give the production of rails in 1883 as estimated :—United States, 1,303,000 tons, about 190,000 tons less than in 1882; Great Britain, 1,097,174 tons, about 140,000 tons less than in 1882. In looking into these figures it has to be borne in mind that the United States absorb the whole of their own produce, whereas Great Britain had to find outlets for 773,509 tons, the difference for home consumption being 323,665 tons.

THE members of the Edinburgh Stock Exchange Association have forwarded a memorial to the Postmaster-General pointing out the inconvenience and loss resulting from the constantly recurring interruption of telegraphic communication caused by the frequent storms, and suggesting that at least the principal lines of communication throughout the country should be protected from atmospheric disturbance by wires being placed underground. It is not proposed that the underground system should supersede the present system. In ordinary circumstances it would act as an auxiliary, and in contingencies only as a substitute.

THE number of railway carriages now being provided with improved means of lighting is larger than generally thought. The Pintsch's Patent Lighting Company has fitted on the Midland Railway 11 carriages, and has 84 in hand; for the Great Western they have fitted 30 carriages; for the South-Eastern 154 are fitted; for the Metropolitan, 257 fitted, and 60 in hand; for the District, 296 fitted, and 54 in hand; for the London and South-Western, 292 fitted, and 61 in hand; for the Great Eastern, 560 fitted, and 32 in hand; for the Caledonian, 102 fitted, and 40 in hand; for the Glasgow and South-Western, 100 fitted, and 100 in hand; for the North British, 2 fitted, and 24 in hand; making a total of 1,804 carriages fitted, and 456 in hand. But, of course, a total of 2,260 is small compared with the total number in use. It is, however, indicative of the spread of a much-needed improvement.



"Let Knowledge grow from more to more."—ALFRED TENNYSON.

Only a small proportion of Letters received can possibly be inserted. Correspondents must not be offended, therefore, should their letters not appear.

All Editorial communications should be addressed to the EDITOR OF KNOWLEDGE; *all Business communications to the* PUBLISHERS, *at the Office, 74, Great Queen-street, W.C.* IF THIS IS NOT ATTENDED TO DELAYS ARISE FOR WHICH THE EDITOR IS NOT RESPONSIBLE.

All Remittances, Cheques, and Post Office Orders should be made payable to MESSRS. WYMAN & SONS.

The Editor is not responsible for the opinions of correspondents.

NO COMMUNICATIONS ARE ANSWERED BY POST, EVEN THOUGH STAMPED AND DIRECTED ENVELOPE BE ENCLOSED.

METEORIC DUST.

[1149]—On Saturday, March 1, after snow had fallen to a depth of 5¼ in., the sky cleared, and then what had previously been a white landscape became a grey or sooty one. All over the peninsula of Roseneath (and apparently the whole valley of the Clyde) a meteoric dust shower had taken place.

On realising this fact, I was careful to try and obtain some. Filtration only resulted in the finely-divided dust passing through with the water. It then occurred to me to melt the snow, and evaporate the resulting water. This I did from half a square foot; then dried and weighed the residue, which turned the scale at two grains, or 8 tons 12 cwt. 3 qr. 17 lb. to the square mile!

On Monday we had another snow-storm, and at midnight a second shower of dust, but very slight. This time the dust had aggregated into little black flakes, which melted their way through the snow at once on falling (there being a general thaw at the time, but no rain), leaving tunnels about the size of crow-quills. Barometer, 30·05 at 90 ft. first fall; 29·6 rising at second. I enclose some of the dust, in which the metallic particles are quite visible.

One of the oldest inhabitants here remembers "the year of the black snow," when it fell black. This occurrence took place on March 20, 22, 1828.

On the evening of the 4th, we had another grand sunset, and the sun appeared to go down much more northerly than due at this time of year. After the clock-light shone out the light was quite red, as if seen through a haze. LEWIS P. MUIRHEAD.

THE EARTH'S DUST ENVELOPE.

[1150]—The article upon "Our Earth's Dust Envelope," by Prof. S. R. Langley, lately published in KNOWLEDGE, reminds me forcibly of the fact that some of the factors that contribute largely, if not principally, to this "Dust Envelope" are almost entirely overlooked.

That Prof. Langley and Mr. King should travel on the wings of the imagination to China for "loess" to beautify the atmosphere around Mount Whitney, while the Inyo desert lay glittering in sunshine at their feet, is to me simply marvellous. But, sir, more marvellous still are the feats attributed to the fierce Krakatoa. According to some of our authorities, that now famous mountain has been supplying us for months with the concrete materials for our magnificent sunsets. Now, Mr. Editor, why should we sweep the regions of space for cosmic dust, and why go the Straits of Sunda and Behring for volcanic ashes to colour our skies, when we can get all we want, and more, too, nearer home? Does not the great African desert suffice for all our wants? Why, then, should it be coldly overlooked in favour of its more sensational rivals? Surely Sahara cannot be so utterly insignificant as to be justly ignored in this matter.

As a landscape painter for the last thirty years I have closely studied atmospheric effects—sunrises and sunsets more particularly—and I can affirm that not a single year passes without similar displays, the only difference having been their less frequency. This being the case, I can have but little faith in cosmic dust, and still less in Krakatoa. Yet I am open to conviction, and I hope you

will kindly bring your greater knowledge to bear upon the more humble phase of a most fascinating subject. E. RAWSTORM.

[I have little to say, the subject being too difficult in my opinion to justify the hasty guesses thrown out. If Krakatoa were the cause of the recent displays, yet one would expect the same features though with less splendour and frequency to have been seen before, and even to be seen every year; for eruptions occur somewhere every year. Has Mr. Rawstorm considered the circumstances that the peculiar ejecta of Krakatoa have been found in European snow-falls? One finds it less easy to understand how Sahara dust could reach the enormous height at which the matter reflecting sunlight certainly has been, than to understand how volcanic dust once sent up there would be carried all over the earth.—R. P.]

"THE GREEN SUN" IN HANKOW.

[1151]—A phenomenon similar to the "green sun in India" (observed at Ceylon from the 9th to the 11th of September inclusive; from various portions of the Indian Ocean on the 10th and 13th; and at Trichinopoly, for some three weeks preceding Oct. 2) has been witnessed several times at Hankow; on Nov. 17 by the Rev. A. W. Nightingale, and on another occasion about the same time (date unrecorded) and again so recently as Dec. 29 by the Rev. G. John and Rev. A. Foster. On these occasions the sun shortly before setting was of a pale green tint, the colour deepening as the orb declined; then followed an exhibition of the glowing redness of the western and southern horizon which since the early part of December last has been observed from the sea-board far into the interior. In due time we shall learn from physicists whether these two remarkable phenomena are correlated or not. Mr. Proctor ascribes the green hue of the sun to absorption of the sun's colour, and Père Decheverens suggests that the fiery glow is due to cosmic dust—to a disintegrated comet. Whatever the cause or causes of these two coincident phenomena may be they present this distinguishing feature. The appearance of a distant conflagration just before sunrise, and after sunset, has now continued for three months; while the solar greenness has been only occasionally observed. Information from other parts of China respecting the "green sun" is a desideratum. D. J. MACGOWAN.

Hankow, 9th Jan.

FLIGHT OF A MISSILE.

[1152]—My friends and self desire to thank you for the trouble and expense you have taken on our behalf in solving the problem relating to the flight of a missile projected vertically, published in No. 88 and several following numbers of KNOWLEDGE. We experienced much difficulty with the problem, and could not arrive at any basis of agreement among ourselves, but now we are perfectly satisfied with your solution; but we would like to see the problem rigidly solved, keeping strictly to the elliptic path, without involving the parabola. I know it will be much more difficult, and that the westerly deviation will be practically the same, even then. There is a small circular segment between the base of the path traced out by the projectile (that is the chord) and the arc of the equator between the two sides of the projectile's path, which ought to be deducted when considering the equality of areas. The height of this segment amounts to about 53 feet. This, for so long a chord, will give a considerable arc to be deducted.

We should like to see the problem strictly solved, but must leave that to you. Already you have done much for us, and for which we thank you.

Wishing you prosperity with your journal, and trusting you are well, on behalf of my friends, N. P. LEE.

Port Adelaide, South Australia,
Feb. 1, 1884.

INVENTION OF THE TELEPHONE.

[1153]—"Cosmopolitan" has gone far away for the origin of the telephone. Fifty years ago I saw the same instrument sold in the streets in Kent by a vendor of children's toys, and bought one for a penny. It consisted of two elongated pill-boxes with a diaphragm of thin writing-paper attached to each other by 20 ft. of sewing cotton passed through the centre of each diaphragm, and then knotted. SERIATIM.

OPTICAL ILLUSION.

[1154]—The following case of persistent self-deception may be of interest:—I have remarkably accurate and clear sight, and a good perception of shades of colour, and yet *italic* printing always conveys to me the impression of being in *dark-green* ink. What makes it more strange is that I have done a good deal as an amateur printer, and yet the italic type which I inked with black ink always seemed to give the same dark-green impression.

I fancy that as a boy I got some notion of coloured ink into my head (probably from the coloured rubrics of the Prayer-book), and my eye still persists in deceiving me. C. W. BOURNE.

WILD FUCHSIA.

[1155]—Can any of your readers inform me if the fuchsia grows wild in any of our southern counties? I came across a specimen of the fuchsia, in bloom, and apparently growing wild, on the eastern bank of Helford River, during a tour round the Cornish coast, as late in the year as the first week in November, 1881.

The flower was of the ordinary red and purple type, but of a very small size. F. W. HALFPENNY.

THE "SPARKBROOK" NATIONAL.

[1156]—Mr. W. H. Watts requests me to say in KNOWLEDGE if I consider the above machine a good one. The *central-geared* "Sparkbrook" I consider one of the best roadsters made, but I do not place any value on the statement that the machine may be driven up a flight of stairs. It would be easy to make a machine which could be driven up a flight of stairs and down one, and yet be an almost worthless machine for general riding purposes.

Only this week I have heard of another frightful accident on one of the boasted stair-climbing machines. JOHN BROWNING.

March 13th.

"A KINDLY THOUGHT" (R. P.).

[1157]—TO MY FELLOW-READERS,—From the tenor of the remarks anent the change of size and price of KNOWLEDGE, it appears the proprietors fear they may lose a certain number of their old subscribers. It has occurred to me, that if all who derive pleasure from a perusal of the work—that is, that if *each one of us* were to purchase an extra copy of the next number issued, and carry it about with us for a week or more, introducing it to our fellow-travellers on the railway, to our friends in the counting-house and at home, we may not only nullify the expected secession from the list of subscribers, but materially increase the number of these, thus giving the Editor greater heart for his work, and ensuring the publishers a more profitable undertaking, as some compensation for the intellectual dish they weekly provide us with. D. C.

(A SUBSCRIBER FROM THE BEGINNING).

LETTERS RECEIVED AND SHORT ANSWERS.

FREE LANCE. Mr. Richards' address was given, but, following our usual custom, was not inserted. It is no case of Jack Robinson, Mr. Richards is well and widely known.—B. MCMILLAN. So sorry, but no deserter.—W. L. Like you I know of no such work. —J. H. COBBETT.—E. ROBINSON.—J. HARDING. They fancied the fire was not hot enough because it looked dull in the sun's light. Have never heard of any experiments by Mrs. Somerville, but Professor Tomlinson made some, and, as might have been expected, they proved decisively that the sun's rays exert no appreciable effect on combustion.—G. H. STYLES.—N. P. LEE.—W. REYNOLDS.—LEOPOLD WAGNER.—THETA.—J. R. CORDER. —S. ALSOP.—R. S. CALLCOTT. Fear there is no prospect of all my books being in the hands of one publisher. Nearly all are advertised in KNOWLEDGE.—J. B. LINDLEY.—W. SWANSTON.—BLACKBROUGH. Thanks.—T. P. BATTERSBY.—J. W.—A. MCD.—STUDENT.— F. W. J. C.—H. SMITH.—J. F. C.—J. FARRAR. There are mistakes in every thing. Patented proposition.—A. GILES.—L. P. MUIRHEAD.— W. F. COLLIS.—A. J. WOOLINER.—L. M.—A. D. TAYLOR. Thanks.— J. W. ROBERTSON. Thanks. Inquire of publishers.—J. H. HAYWOOD. From publishers.—D. C. Thanks for your very kind and encouraging letter.—M. T. H.—M. F. J. MANN.—FACIEBAT. Fear there is little encouragement to lecture where Jupiter is such a novelty. Such slips as you notice may generally be referred to carelessness somewhere; but thanks.—T. R. CLAPHAM. Thanks for pleasant greeting. —M. The ring reflection would not work, for the following reasons, —the rays from a point even on the axis of the cylinder of which the ring-surface is part would not be brought to a focus as in your figure, but diverge after leaving the reflecting surface.—V. CAVAN. Excuse delay in reply to your letter. There is a correction for clock-time. Have drawn a figure showing exactly how clock-time differs from sun-time throughout the year. Will presently find room for it.—HELEN A. KING. Many thanks for the lines on knowledge which shall certainly appear.—W. J. TRONTON. If the diameter of a circle is 1, the area of the circle is represented by one-fourth of the well-known ratio (I quote from memory Loisettically strengthened) 3·14159265358979323846264338 32, &c., &c. If the diagonal of a square is 1, the area of the square is represented by half

the square root of 2, that is half of 1·41421356237309504880168872 &c., &c.; but I see you did not ask for this: the area of a circle containing such a diagonal is as before one-fourth of 3·14159265 &c.—RUPERT SWINDELLS. The comet was surely not very resplendent. Here it was little more than just visible.—J. B. FINDLEY. (1) Fear your question as to the publishers of Dr. Hull's Geographical Charts did not reach Mr. Allen. (ii.) A refractor would suit you best; in fact, I think mirrors for refractors are not sold separately, whereas you can get object-glass and eye-pieces. But if you go in for making the whole telescope, I suppose a reflector would be the thing. Yet what you would want for figuring a mirror would cost more than a small telescope such as you probably require.—IGNORAMUS. The term "parabolic orbit" is perhaps not correct,—parabolic path would be better. For twinkling or scintillation see full answer this week.—THOMSON WALKER. Clay's work is published by Thos. De la Rue; Pole's by Longmans: have forgotten price, but both books are cheap.—E. ROBINSON. The subject does not come within our scope, which includes rather things testable by the telescope and polariscope, spectroscope and microscope.—G. H. STYLES. Have some maps for the southern hemisphere in preparation, but for the present they have to wait. To see such a halo the eyes must be on or very near the line from sun's centre to the object forming the shadow. In the case of your head this was so, but not in the case of your horse's head nor of your raised hat. If you saw the halo round your friend's head there would be a difficulty. But he saw that: you only saw your own halo.—J. W. ROBERTSON. Nearly all the old numbers can be obtained, but four or five are out of print.—M. F. J. MANN. Have you not overlooked the vertical plumb-line above the "bottomless pit" (as Professor Smyth considers the underground chamber to be)? That would give the horizontal north-and-south line.—D. L. A very kindly suggestion.—LEWIS P. MUIRHEAD. Levity was just what Newton so neatly got rid of. Attraction suffices to explain all the movements of the planets.—A. J. WOOLMER. The odds against throwing 2 sixes with 2 dice are 35 to 1. The betting man who after you had chanced to throw 2 sixes twice running, offered you £100 to £5 that you would not throw 2 sixes again, was not only a gambler, which is bad, but a swindler, which is worse. The fair odds were £175 to £5.—A BELIEVER. Thanks; but such narratives numerous.—T. PRESTON BATTERSBY. I should simply say that *obviously* the evil results of any such attempt to remove misery would far exceed the good. The death of one whose life is torture does, no doubt, in itself bring relief; but a system by which such results were deliberately brought about would bring much more misery than it would remove.—GROPER. The principal of Virtual Velocities would be more suitable as the subject of an article than of a short answer: will see about this. Thanks for very kindly letter.—STUDENT. I avoided instances lest I should inadvertently injure Prof. Loisette. I should find no difficulty in applying the system to all the subjects you mention. But I have been especially struck of late by the way in which, apart from its direct application, the system strengthens and develops the mind's retentive power. To give an example of an outside use of the method, I note,—Walking through Richmond four or five times only, I have found it easy to recollect the names of about 150 tradesmen, in the order of their shops, on one side of the road. But I could cite a number of really useful applications of the system as well as this mere illustration of its effectiveness.—T. W. H. Thanks for your thoughtful and suggestive letter, which Mr. Foster has read with extreme interest.—MARTHA HATHERWELL. The construction of telescopes so far as optical principles are concerned is dealt with in my little book "Half-hours with the Telescope," published by (and the property of) Messrs. Allen & Co.—R. H. ROCKE. I think my note of admiration only related to the "shape of an oyster-shell." The quantity of matter falling on the earth from outside can only be roughly estimated from the known mass of shooting stars, their number as observed at any given station and so forth.—A. McD.—E. L. R.—C. W. BOURNE.—E. J. P.—(1.) No dictionary of astronomical, that I know of, gives the pronunciation of star names. (2.) Some animals like some music.—E. L. R. The original "blue stockings" was a gentleman—Mr. Stillingfleet—who showed such marked conversational power at meetings of literary ladies held in Johnson's time that they used to say when he was absent, "We can do nothing without our blue stockings" (he rejoiced in two legs). Hence these meetings came to be called "blue-stocking clubs," and the ladies who attended them were called "blue-stockings." "*A* blue-stocking" is absurd, unless a lady had a wooden leg.—G. H. WILKINSON. Thanks for kindly letter. For fear of giving Mr. N. C. excuse for further entry your quaint letter of denunciation does not appear.

ERRATUM.—I suppose I wrote "Skill at Chess" instead of "Skill at Whist" last week: but it was merciless of compositor and reader to leave it unchanged.

Our Mathematical Column.

NOTES ON EUCLID'S FIRST BOOK.

By RICHARD A. PROCTOR.

(*Continued from page* 153.)

LET us nex examine the particular objects which Euclid appears to have had in view in the first book, and see whether any additions seem to be suggested; noting at the same time those propositions which are of frequent use to the geometrician.

The first proposition shows us how to construct an equilateral triangle. The same method is clearly applicable to the construction of an isosceles triangle on any finite line.

The student is not likely to neglect the application of Prop. 3 (to which Prop. 2 is wholly subsidiary).

Prop. 4 is the first determining the equality of triangles. The others are Props. 8 and 26. We learn from them that the equality (i) of two sides and the included angle; (ii) of the three sides, (iii) of two angles and a side opposite to equal angles in each triangle; or (iiii) of two angles and a side adjacent to them suffices to determine the equality of two triangles *in all respects.* For although Euclid limits the proof in Prop. 8 to the angles contained by the *sides* (as distinguished from the *base*), yet since any side might have been taken for base the equality of each angle of one triangle to the corresponding angle of the other is established.

Now there are six elements in the determination of a triangle,—the three angles and the three sides. It will appear, on consideration, that Euclid has combined these, three and three, in four ways out of six possible ways. It remains, then, only to consider the remaining two.

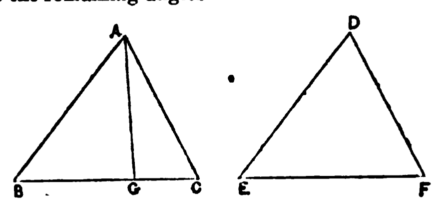
It is obvious that if the three angles of one triangle are equal to the three angles of another, the triangles are not necessarily equal. For, it follows from Euc. I., 29, that if we draw a parallel to one side of a triangle, either within the triangle, or else to meet the other two sides produced, we form another triangle, unequal to the first, but having equal angles.

There remains only the case of two triangles having two sides of the one equal to two sides of the other each to each, and an angle opposite one side of one triangle equal to the angle opposite to the equal side of the other. In this case the two triangles are not *necessarily* equal. We may form the following proposition, which is an important one, as are also its corollaries.

PROP. I.—*If two triangles have two sides of the one equal to two sides of the other, each to each, and the angles opposite to a pair of equal sides equal; then if the angles opposite the remaining sides be both acute, or both obtuse, or if one of them is a right angle, the two triangles are equal in every respect.*

In the two triangles A B C, D E F, let A B be equal to D E, and A C to D F; also let the angle B be equal to the angle E.

First let the remaining angles C and F be acute.



If the angle A be not equal to the angle D, make the angle B A G equal to the angle D. Then the triangles A B G and D E F are equal in all respects (Euc. I., 26), therefore A G is equal to D F, and the angle A G B to the angle F. But since D F is equal to A C, A G is equal to A C, and the angle A G C to the angle A C G; hence A G C is an acute angle and A G B obtuse (Euc. I., 13). Therefore the angle A G B is *not* equal to the angle F; which is absurd. Therefore the angle B A C is not unequal to D, that is, these angles are equal, and (Euc. I., 4) the triangles A B C and and D E F are equal in all respects.

If the angles C and F are both obtuse the proof is similar to the preceding; or if we please, we may adopt a proof resembling that of the following case.

If the angle C is a right angle, we proceed as before until we have proved that the angle A G C is equal to the angle A C G. Thus we have two angles of the triangle A G C equal to two right angles, which is impossible. Therefore, as before, the triangles are equal in all respects.

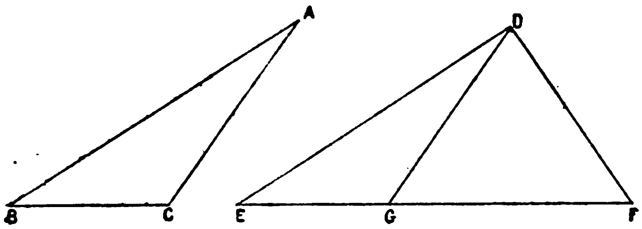
COR. 1.—If the equal angles B and E are right or obtuse, the other angles are necessarily acute and the triangles are equal in all respects.

COR. 2.—If the equal sides A C, D F, are greater than the equal sides A B, D E, the angles C and F are necessarily acute (Euc. I.

18, 17). Hence in this case also the triangles are equal in all respects.

SCHOLIUM.—It appears, then, that the triangles can only differ, when the equal angles B and E are acute, and the pair of sides opposite them less than the other pair of equal sides. In this case a relation holds important enough to form a separate proposition — of which we shall presently have occasion to make use,—

PROP. II.—*If two triangles have two sides of one equal to two sides of the other, each to each, and the angles opposite one pair of equal sides equal; then if the angles opposite the remaining pair of equal sides be unequal their sum is equal to two right angles.*



In the triangles A B C, D E F, let A B, A C, be equal to D E, D F, each to each; the angle B equal to the angle E, but the angle C greater than the angle F,—so that by Prop. I. C is obtuse and F acute. Then (Euc. I. 32) the angle E D F is greater than A. Make the angle E D G equal to A; then the triangle E D G is equal to the triangle A B C in all respects (Euc. I. 26). Hence D G is equal to A C, and therefore to D F; also the angle D G E is equal to the angle C. Now since D G is equal to D F, the angle D G F is equal to the angle D F G. But D G F and D G E together make up two right angles; hence their respective equals F and C together make up two right angles.

COR. 1.—The triangle E D F exceeds the triangle A B C by the isosceles triangle D G F.

There are other elements such as the area, altitude, and so on, which determine triangles. We shall have occasion, as we proceed, to notice how triangles may be constructed when one or more such elements, combined perhaps with one or more of the six elements just considered, are given. But we may consider the relations discussed in Euc. I., 4, 8, 26, and in the above propositions, as the fundamental problems in the determination of triangles.

We proceed, therefore, to discuss the construction of triangles when certain of the six elements above considered are given.

(*To be continued*).

EASY RIDERS ON EUCLID'S FIRST BOOK.

WITH SUGGESTIONS FOR SOLUTION.

PROP. 6.

15. In the figure of I. 5, if F C, B G meet in H, the triangle B H C is isosceles.

16. If, further, F G is drawn, the triangle F H G is isosceles.

17. If, further, A H be drawn, the triangles A B H, A C H, are equal in all respects.

18. With the same construction A H bisects B C at right angles.

19. With the same construction A H produced bisects F C at right angles.

20. With the same construction the triangles B H F, C H G are equal in all respects.

If problems 15–20 be taken in order, the student will find no difficulty in solving them without using any propositions beyond Euc. I., 6.

21. If the angles A B C, A C B at the base of an isosceles triangle be bisected, by the straight lines B D, C D, show that D B C will be an isosceles triangle.

22. In the quadrilateral A B C D, D C is equal to B C, and the angle A B C is equal to the angle A D C. Show that A D is equal to A B.

Join D B and apply Euc. I. 5; the rest is obvious.

(*To be continued.*)

CANADIAN TELEGRAPHS.—A Canadian Blue Book just issued says that Canadian tariffs are probably the cheapest in the world, and will still compare favourably with the reduced rate of sixpence shortly (?) to take effect in England.

Our Whist Column.

BY "FIVE OF CLUBS."

THE following game illustrates the importance of clearing or helping to clear your own or partner's strong suit: *Y* who plays badly throughout negects this rule, *B* follows it and wins thereby five by cards.

THE HANDS.

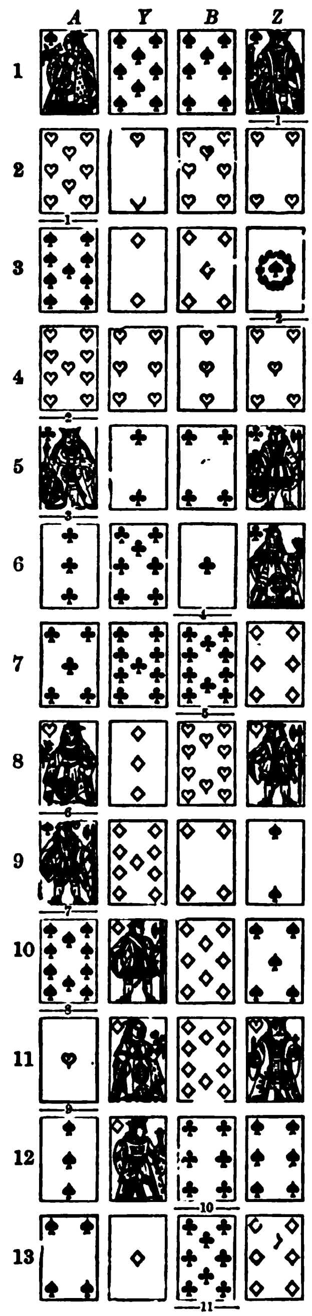
B { *Clubs*—A, 10, 8, 6, 4. Hearts—10, 7, 3. Spades—7. Diamonds—10, 8, 5, 4. }

Y { *Clubs*—9, 7, 2. Hearts—6, 2. Spades—8. Dia.—A, K, Q, Kn, 9, 3, 2. }

A B, 0. **B**
Y *YZ*, 0. Dealer. **Z** *Trump. C. Q.* Leader. **A**

{ Q, Kn—*Clubs*. 4, 5, Kn, K—Hearts. 2, 5, 6, K, A—Spades. 6, 7—Diamonds. } *Z*

A { *Clubs*—K, 5, 3. Hearts—A, Q, 9, 8. Spades—Q, Kn, 10, 9, 4, 3. Diamonds—None. }



NOTES AND INFERENCES.

1. All know that *A* holds Kn, 10. Position of 9 remains unknown to all except *A*. *A* knows that *Y* and *B* have no more Spades, unless one or other is signalling.

2. *A* sees that *Y* has not been signalling, unless—which is unlikely—he held originally but two Spades, and a singleton in Hearts. *B* may be signalling. *B* knows that honours in Hearts are divided between *A* and *Z*.

3. *A* leads the 9 in order that his partner may know what *Z* already knows, that *A* holds quart minor. *Y* should have trumped even if he had been certain *Z* held the Ace. Why, discarding, he selected his splendid Diamond suit it is impossible to divine. He may have had some vague idea of keeping a card wherewith to return his partner's suit. But he should have seen how much better was the chance of bringing in his own suit after ruffing. *B* who *is* certain that *Z* holds the Ace, very properly declines to ruff, that his partner's suit may be cleared. He sees further that *Z* will lead at a disadvantage. He commences another signal.

4. *Z* would probably have led a Diamond, his partner being so weak in Hearts, but for *Y*'s discard. *B* sees that *A* holds Heart Ace. *B* completes his signal. *Y* holds now only Diamonds and trumps.

5, 6, 7. *A* responds to his partner's signal and the enemy's teeth are carefully extracted.

8. *B* gives his partner the lead, and

9, 10, 11, 12, 13, the rest of the hand plays itself, *A-B* making five by tricks.

DOUBLE DUMMY ENDING.

BY MR. F. H. LEWIS.

A.—S. A, 2; D. A, 6, 4; H. K, 10.

Y.—S. 9, Q; D. Q, K; C. 9; H. 6, Q.

B.—S. 10, K; C. 5, 10; H. 3; D. 7, 10.

Z.—S. 3, 8; H. 4, 8; C. Kn; D. 5, 8.

Spades trumps. *B* to lead, and *A* and *B* to make all seven tricks.

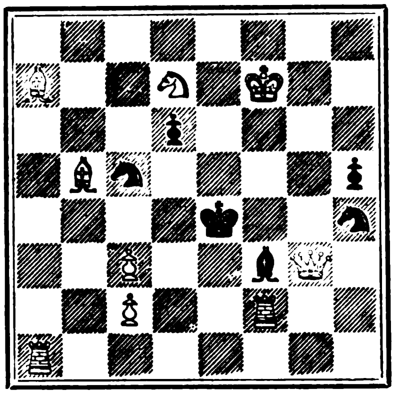
Our Chess Column.

BY MEPHISTO.

PROBLEM No. 115.

BY E. N. FRANKENSTEIN.

BLACK.



WHITE.

White to play and mate in two moves.

NOTE.—We offer a copy of Miles's "Poems and Chess Problems" for the first best solution received of the above problem.

IF we had to write a dissertation on the uses of Chess, we feel confident we should point out many beneficial effects of Chess playing which escape the notice of ordinary observers. We should likewise suggest many occasions when a game of Chess might be played to afford recreation and amusement. We must, however, confess that we should never have thought of advocating Chess play on 'Change.

But truth is stranger than fiction, for that which our imagination would have refused to depict has actually come to pass. Two gentlemen have whiled away time "in the house" by playing Chess *blindfold*.

We cannot tell whether or no blindfold Chess play—as strengthening the mind—was resorted to as an indirect aid to devise clever means to raise the price of "Unified," or whether, by increased powers of analysis, the players hoped to be better able to estimate the proper value of "Grand Trunks" and "American" Rails, in order to avoid the snares, pitfalls, and delusions of the Vanderbilts, Goulds, &c. It is sufficient for our purpose to state that, considering the strength of the players, they played a really good game, notwithstanding the necessarily trying surroundings. We append the score of the game, and although some weak moves occur this is fully condoned by the brilliant finish, where White announces a mate in four moves.

GAME PLAYED BLINDFOLD ON THE STOCK EXCHANGE.

CENTRE GAMBIT.

	White. Mr. H.	Black. Mr. B.
1.	P to K4	P to K4
2.	P to Q4	P takes P
3.	P to QB3	P takes P
4.	B to QB4	P to Q3
5.	Q to Kt3	Q to K2
6.	Kt takes P	Kt to QB3
7.	Kt to B3	B to K3 (?)
8.	B to KKt5	B takes B
9.	Q takes B	Q to Q2
10.	Castles KR	B to K2
11.	P to KR4	B takes B
12.	Kt takes B	Kt to K4.
13.	Q to Kt3	Castles
14.	P to B4	P to KR3
15.	P takes Kt	P takes Kt
16.	P takes QP	Q takes P
17.	QR to Q sq.	Q to K2
18.	Kt to Q5 (!)	Q takes P
19.	KR to K sq. (!)	Q takes RP

White announced mate in four moves.

White. Mr. H. — Black. Mr. B.

BLACK.

WHITE.

	White. Mr. H.	Black. Mr. B.
20.	Kt to Kt6 (ch)	RP tks Kt (*a*)
21.	R tks R (ch)	K takes R
22.	Q to Q5 (ch)	K to B sq.
23.	R to K8 mate.	

(*a*) If 20. BP takes Kt, then 21. Q to B3 (ch) &c.

SOMETHING BETWEEN FOOL'S MATE AND SCHOLAR'S MATE.

EVERYONE knows Fool's Mate in which mate is given by second player on the second move, and everyone knows Scholar's Mate in which mate is given by first player on the fourth move. But perhaps everyone does not know the following mate in three, in which there is a combination of energy in attack and self-sacrifice in defence, entitling us to regard this mate as "something between Fool's and Scholar's."

1. P to K4	1. P to K4
2. Q to R5	2. K to K2
3. Q takes P mate	

[R. P.]

ANSWERS TO CORRESPONDENTS.

*** *Please address Chess Editor.*

J. G. W.—1. The book is out of print. Try Long's. 2. No chance of coming to life again.

H. W. Sherrard.—Received with thanks, but *four* are too many for us.

Tom.—Problem not suitable.

CONTENTS OF No. 124.

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The terms of Annual Subscription to the weekly numbers of KNOWLEDGE will be, commencing No. 123, as follows:—

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3. THE MOON.	6. THE STAR DEPTHS.

See Advt. Pages for full Syllabus.

The following arrangements are complete: the numbers in brackets referring to above list.

LONDON (Brixton Hall) March 28, April 1, 4, 8 (1, 2, 3, 4).
„ (Memorial Hall), March 24, 27, 31, April 3 (1, 2, 3, 4).
CRYSTAL PALACE, April 7, 9 (1, 3).
GRAVESEND, March 25, 26 (1, 2).
ST. LEONARDS, March 29 (Afternoon); April 2 (Afternoon and Evening).
BIRMINGHAM (Town Hall), April 18, 23, 25, 28; May 2 (1, 2, 3, 5, 6).
LEAMINGTON (Royal Music Hall), Four Afternoons, April 17, 19, 24, 26 (1, 3, 5, 6); Two Evenings, April 17, 24 (2, 4).
St. HELEN'S (Lanc.), April 22 (2).
COVENTRY, April 30, May 1 (1, 2).
MALVERN, May 3, 17 (Afternoon) (2, 3).
LLANELLY, May 5 (1).
SWANSEA, May 6, 7 (1, 2).
WORCESTER, May 8, 9, 15, 16.
BANBURY, May 14 (Afternoon and Evening).
OXFORD, May 12, 13, 19, 20.
CAMBRIDGE, May 21, 22, 23.

NOTE.—*All communications respecting Lectures should be addressed to Mr. John Stuart, Royal Concert Hall, St. Leonards.*

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LONDON: FRIDAY, MARCH 28, 1884.

CONTENTS OF No. 126.

COINCIDENCES AND SUPERSTITIONS.

By R. A. PROCTOR.

EVERY one is familiar with the occasional occurrence of coincidences, so strange—considered abstractly—that it appears difficult to regard them as due to mere casualty. The mind is dwelling on some person or event, and suddenly a circumstance happens which is associated in some altogether unexpected, and as it were improbable, manner with that person or event. A scheme has been devised which can only fail if some utterly unlikely series of events should occur, and precisely those events take place. Sometimes a coincidence is utterly trivial, yet attracts attention by the singular improbability of the observed events. We are thinking of some circumstance, let us say, in which two or three persons are concerned, and the first book or paper we turn to shows, in the very first line we look at, the names of those very persons, though really relating to others in no way connected with them; and so on, with many other kinds of coincidence, equally trivial and equally singular. Yet again, there are other coincidences which are rendered striking by their frequent recurrence. It is to such recurring coincidences that common superstitions owe their origin, while the special superstitions thus arising (that is, superstitions entertained by individuals) are innumerable. It is lucky to do this, unlucky to do that, say those who believe in common superstitions; and they can always cite many coincidences in favour of their opinion. But it is amazing how common are the private superstitions entertained by many who smile at the superstitions of the ignorant; we must suppose that all such superstitions have been based upon observed coincidences. Again, there are tricks or habits which have obviously had their origin in private superstitions. Dr. Johnson may not have believed that some misfortune would happen to him if he failed to place his hand on every post which he passed along a certain route; he would certainly not have maintained such an opinion publicly; yet in the first instance that habit of his must have had its origin in some observed coincidences; and when once a habit of the sort is associated with the idea of good luck, even the strongest minds have been found unready to shake off the superstition.

It is to be noticed, indeed, that many who reject the idea that the ordinary superstitions have any real significance, are nevertheless unwilling to run directly counter to them. Thus, a man shall be altogether sceptical as to the evil effects which follow, according to a common superstition, from passing under a ladder; he may be perfectly satisfied that the proper reason for not passing under a ladder is the possibility of its falling, or of something falling from it; yet he will not pass under a ladder, even though it is well secured, and obviously carries nothing which can fall upon him. So with the old superstition that a broken mirror brings seven years of sorrow, which, according to some, dates from the time when a mirror was so costly as to represent seven years' savings—there are those who despise the superstition who would yet be unwilling to tempt fate (as they put it) by wilfully breaking even the most worthless old looking-glass. A story is not unfrequently quoted in defence of such caution. Every one knows that sailors consider it unlucky for a ship to sail on a Friday. A person anxious to destroy this superstition, had a ship's keel laid on a Friday, the ship launched on a Friday, her masts taken in from the sheer-hulk on a Friday, the cargo shipped on a Friday; he found (Heaven knows how, but so the story runs) a Captain Friday to command her; and, lastly, she sailed on a Friday. But the superstition was not destroyed, for the ship never returned to port, nor was the manner of her destruction known. Other instances of the kind might be cited. Thus a feeling is entertained by many persons not otherwise superstitious that bad luck will follow any wilful attempt to run counter to a superstition.

It is somewhat singular that attempts to correct even the more degrading forms of superstition have often been as unsuccessful as those attempts which may perhaps not unfairly be called tempting fate. Let me be understood. To refer to the example already given, it is a manifest absurdity to suppose that the sailing of a ship on a Friday is unfortunate; and it would be a piece of egregious folly to consider such a superstition when one has occasion to take a journey. But the case is different when any one undertakes to *prove* that the superstition is an absurdity; simply because he must assume in the first instance that he will succeed, a result which cannot be certain; and such confidence, apart from all question of superstition, is a mistake. In fact, a person so acting errs in the very same way as those whom he wishes to correct; *they* refrain from a certain act because of a blind fear of bad luck, and *he* proceeds to the act with an equally blind belief in good luck.

But one cannot recognise the same objection in the case of a person who tries to correct some superstition by actions not involving any tempting of fortune. Yet it has not unfrequently happened that such actions have resulted in confirming the superstition. The following instance may be cited. An old woman came to Flamsteed, the first Astronomer Royal, to ask him whereabouts a certain bundle of linen might be, which she had lost. Flamsteed determined to show the folly of that belief in astrology which had led her to Greenwich Observatory (under some misapprehension as to the duties of an Astronomer Royal). He "drew a circle, put a square into it, and gravely pointed out a ditch, near her cottage, in which he said it would be found." He then waited until she should come back disappointed, and in a fit frame of mind to receive the rebuke he intended for her; "but she came back in great delight, with the bundle in her hand, found in the very place."

In connection with this story, though bearing rather on over-hasty scientific theorising than on ordinary superstitions, I quote the following story from De Morgan's "Budget of Paradoxes":—"The late Baron Zach received

a letter from Pons, a successful finder of comets, complaining that for a certain period he had found no comets, though he had searched diligently. Zach, a man of much sly humour, told him that no spots had been seen on the sun for about the same time—which was true—and assured him that when the spots came back the comets would come with them. Some time after he got a letter from Pons, who informed him with great satisfaction that he was quite right; that very large spots had appeared on the sun, and that he had found a comet shortly after. I have the story in Zach's handwriting. It would mend the story exceedingly if some day a real relation should be established between comets and solar spots. Of late years good reason has been shown for advancing a connection between these spots and the earth's magnetism. If the two things had been put to Zach, he would probably have chosen the comets. Here is a hint for a paradox: The solar spots are the dead comets, which have parted with their light and heat to feed the sun, as was once suggested. I should not wonder if I were too late, and the thing had been actually maintained." De Morgan was not far wrong. Something very like his paradox was advocated, before the Royal Astronomical Society, by Commander Ashe, of Canada, earlier, we believe, than the date of De Morgan's remarks. I happen to have striking evidence in favour of De Morgan's opinion about the view which Zach would probably have formed of the theory which connects sun-spots and the earth's magnetism. When the theory was as yet quite new, I referred to it in a company of Cambridge men, mostly high mathematicians, and it was received at first as an excellent joke, and welcomed with laughter. It need hardly be said, however, that when the nature of the evidence was stated, the matter assumed another aspect. Yet it may be mentioned, in passing, that there are those who maintain that, after all, this theory is untrue, the evidence on which it rests being due only to certain strange coincidences.

In many instances, indeed, considerable care is required to determine whether real association or mere casual coincidence is in question. It is surprising how, in some cases, an association can be traced between events seemingly in no way connected. One is reminded of certain cases of derivation. Ninety-nine persons out of a hundred, for instance, would laugh at the notion that the words "hand" and "prize" are connected; yet the connection is seen clearly enough when "prize" is traced back to "prehendo," with the root "hend" obviously related to "hand," "hound," and so on. Equally absurd at a first view is the old joke that the Goodwin Sands were due to the building of a certain church; yet, if moneys which had been devoted to the annual removal of the gathering sand were employed to defray the cost of the church, mischief, afterwards irreparable, might very well have been occasioned. Even the explanation of certain mischances as due to the circumstance that "there was no weathercock at Kiloe," may admit of a not quite unreasonable interpretation. I leave this as an exercise for the ingenious reader.

(To be continued.)

THE CHEMISTRY OF COOKERY.

XXXI.

BY W. MATTIEU WILLIAMS.

HAVING described the changes effected by heat upon starch, and referred to its further conversion into dextrine and sugar, I will now take some practical examples of the cookery of starch foods, beginning with those which are composed of pure, or nearly pure, starch.

When arrowroot is merely stirred in cold water it sinks to the bottom undissolved and unaltered. When cooked in the usual manner to form the well-known mucilaginous or jelly-like food, the change is a simple case of the swelling and breaking up of the granules described as occurring in water at the temperature of 140° Fahr. There appears to be no reason for limiting the temperature, as the same action takes place from 140° upwards, to the boiling point of water.

I may here mention a peculiarity of another form of nearly pure starch food, viz., tapioca, which is obtained by pulping and washing out the starch granules of the root of the *Manihot*, then heating the washed starch in pans and stirring it while hot with iron or wooden paddles. This cooks and breaks up the granules and agglutinates the starch into nodules which, as Mr. James Collins explains (*Journal of Society of Arts*, March 14, 1884,) are thereby coated with dextrine, to which gummy coating some of the peculiarities of tapioca pudding are attributable. It is a curious fact that this *Manihot* root, from which our harmless tapioca is obtained, is terribly poisonous. The plant is one of the large family of nauseous spurgeworts (*Euphorbiaceæ*). The poison resides in the milky juice surrounding the starch granules, but being both soluble in water and volatile, most of it is washed away in separating the starch granules, and any that remains after washing is driven off by the heating and stirring which has to reach 240°, in order to effect the changes above described.

I suspect that the difference between the forms of tapioca and arrowroot has arisen from the necessity of thus driving off the last traces of the poison with which the aboriginal manufacturers were so well acquainted as to combine the industry of poisoning their arrows with that of extracting the starch-food from the same root. No certificate from the public analyst is demanded to establish the absence of the poison from any given sample of tapioca, as the juice of the manihot root, like that of other spurges, is unmistakably acrid and nauseous.

Sago, which is a starch obtained from the pith of the stem of the sago-palm and other plants, is prepared in grains like tapioca, with similar results. Both sago and tapioca contain a little gluten, and therefore have more food-value than arrowroot.

The most familiar of our starch foods is the potato. I place it among the starch foods, as next to water, starch is its prevailing constituent, as the following statement of average compositions will show:—Water, 75 per cent.; starch, 18·8; nitrogenous materials, 2; sugar, 3; fat, 0·2; salts, 1. The salts vary considerably with the kind and age of the potato, from 0·8 to 1·3 in full-grown. Young potatoes contain more. In boiling potatoes, the change effected appears to be simply a breaking up or bursting of the starch granules, and a conversion of the nitrogenous gluten into a more soluble form, probably by a certain degree of hydration. As we all know, there are great differences among potatoes, some are waxy, others floury; and these, again, vary according to the manner and degree of cooking. I cannot find any published account of the chemistry of these differences, and must, therefore, endeavour to explain them in my own way.

As an experiment, take two potatoes of the floury kind; boil or steam them together until they are just softened throughout, or, as we say, "well done." Now leave one of them in the saucepan or steamer, and very much overcook it. Its floury character will have disappeared, it will have become soft and gummy. The reader can explain

this by simply remembering what has already been explained concerning the formation of dextrine. It is due to the conversion of some of the starch into dextrine. My explanation of the difference between the waxy and floury potato is that the latter is so constituted that all the starch granules may be disintegrated by heat in the manner already described before any considerable proportion of the starch is converted into dextrine, while the starch of the waxy potatoes for some reason, probably a larger supply of diastase, is so much more readily convertible into dextrine, that a considerable proportion becomes gummy before the whole of the granules are broken up, *i.e.*, before the potato is cooked or softened throughout.

I must here throw myself into the great controversy of jackets or no jackets. Should potatoes be peeled before cooking, or should they be boiled in their jackets? I say most decidedly in jackets, and will state my reasons. From 53 to 56 per cent. of the above-stated saline constituents of the potato is potash, and potash is an important constituent of blood—so important that in Norway, where scurvy once prevailed very seriously, it has been banished since the introduction of the potato, and according to Lang and other good authorities, it is owing to the use of this vegetable by a people who formerly were insufficiently supplied with saline vegetable food.

Potash salts are freely soluble in water, and I find that the water in which potatoes have been boiled contains potash, as may be proved by boiling it down to concentrate, then filtering and adding the usual potash test, platinum chloride.

It is evident that the skin of the potato must resist this passage of the potash into the water, though it may not fully prevent it. The bursting of the skin only occurs at quite the latter stage of the cookery. The greatest practical authorities on the potato, Irishmen, appear to be unanimous. I do not remember to have seen a pre-peeled potato in Ireland. I find that I can at once detect by the difference of flavour whether a potato has been boiled with or without its jacket, and this difference is evidently saline.

These considerations lead to another conclusion, viz., that baked potatoes, and fried potatoes, or potatoes cooked in such a manner so as to be eaten with their own broth, as in Irish stew (in which cases the previous peeling does no mischief), are preferable to boiled potatoes. Steamed potatoes probably lose less of their potash juices than when boiled; but this is uncertain, as the modicum of distilled water condensed upon the potato and continually renewed may wash away as much as the larger quantity of hard water in which the boiled potato is immersed.

Those who eat an abundance of fruit, of raw salads, and other vegetables supplying a sufficiency of potash to the blood, may peel and boil their potatoes; but the poor Irish peasant who depends upon the potato for all his sustenance requires that they shall supply him with potash.

When travelling in Ireland (I explored that country rather exhaustively when editing the Fourth Edition of "Murray's Handbook), I was surprised at the absence of fruit-trees in the small farms where one might expect them to abound. On speaking of this the reason given was that all trees are the landlord's property; that if a tenant should plant them they would suggest luxury and prosperity, and therefore a rise of rent; or otherwise stated, the tenant would be fined for thus improving the value of his holding. This was before the passing of the Land Act, which we may hope will put an end to such legalised brigandage. With the abolition of rack-renting the Irish peasant may grow and eat fruit; may even taste jam without fear and trembling; may grow rhubarb and make pies and puddings in defiance of the agent. When this is the case, his craving for potato-potash will probably diminish, and his children may actually feed on bread.

As regards the nutritive value of the potato, it is well to understand that the common notion concerning its cheapness as an article of food is a fallacy. Taking Dr. Edward Smith's figures, 760 grains of carbon and 24 grains of nitrogen are contained in 1 lb. of potatoes; $2\frac{1}{2}$ lb. of potatoes are required to supply the amount of carbon contained in 1 lb. of bread; and $3\frac{1}{2}$ lb. of potatoes are necessary for supplying the nitrogen of 1 lb. of bread. With bread at $1\frac{1}{2}$d. per lb., potatoes should cost less than $\frac{1}{2}$d. per lb. in order to be as cheap as bread for the hard-working man who requires an abundance of nitrogenous food.

My own observations in Ireland have fully convinced me of the wisdom of William Cobbett's denunciation of the potato as a staple article of food. The bulk that has to be eaten, and is eaten, in order to sustain life, converts the potato feeder into a mere assimilating machine during a large part of the day, and renders him unfit for any kind of vigorous mental or bodily exertion. If I were the autocratic Czar of Ireland my first step towards the regeneration of the Irish people would be the introduction, acclimatising, and dissemination of the Colorado beetle, in order to produce a complete and permanent potato famine. The effect of potato feeding may be studied by watching the work of a potato-fed Irish mower or reaper who comes across to work upon an English farm where the harvestmen are fed in the farmhouse and the supply of beer is not excessive. The improvement of his working powers after two or three weeks of English feeding is comparable to that of a horse when fed upon corn, beans, and hay, after feeding for a year on grass only.

The reader may have observed that the starch foods already described are all derived from the roots or stems of plants. Many others might be named that are used in tropical climates where little labour is demanded or done, and but little nitrogenous food required. Having treated the cookery of the chief constituents of these parts of the plant, the fibre and the starch, I now come to food obtained from the seeds and the leaves.

Taking the seeds first, as the more important, it becomes necessary to describe the nitrogenous constituents which are more abundant in them than in any other part of the plant, though they also contain the starch and cell material, or woody fibre, as already stated.

In No. 29 of this series, page 123, I described a method of separating starch from flour by washing a piece of dough in water, and thereby removing the starch granules, which fall to the bottom of the water. If this washing is continued until no further milkiness of the water is produced, the piece of dough will be much reduced in dimensions, and changed into a grey, tough, elastic, and viscous or glutinous substance, which has been compared to bird-lime, and has received the appropriate name of *gluten*. When dried, it becomes a hard, horny, transparent mass. It is insoluble in cold water, and partly soluble in hot water. It is soluble in strong vinegar, and in weak solutions of potash or soda. If the alkaline solution is neutralised by an acid, the gluten is precipitated.

If crude gluten obtained as above is subjected to the action of hot alcohol, it is separated into two distinct substances, one soluble and the other insoluble. As the solution cools, a further separation takes place of a substance soluble in hot alcohol but not in cold, and another soluble in either hot or cold alcohol. The first—viz., that insoluble in either hot or cold alcohol has been named *gluten-fibrin*; that soluble in hot alcohol, but not in cold, *gluten-casein*; and that soluble in either hot or cold alcohol, *glutin*. I

give these names and explain them, as my readers may be otherwise puzzled by meeting them in books where they are used without explanation, especially as there is another substance presently to be described, to which the name of vegetable casein has also been applied. The gluten-fibrin is supposed to correspond with blood fibrin, gluten-casein with animal casein, and glutin with albumen.

EFFECTS OF THE GLACIAL PERIOD.

I.—INORGANIC EFFECTS.

WHEN we dealt with the "Evidences of the Glacial Period," we enumerated, under the head of "Inorganic Evidences," moraines, polished and striated rock surfaces, erratic blocks, perched boulders, scored and rounded mountains, and dome-shaped rocks. Of course, all these might, with equal propriety, be treated as Effects of the Glacial Period; but, having already briefly described them, I do not purpose to make any further comment thereon now, with the single exception of observing that probably the most striking of such effects may be seen in the rounded mountain summits of Scotland, Wales, and the Lake District. No evidence or effect of the Glacial Period can be more obvious to the ordinary eye, not specially trained in the observation of geological scenery, than these widely distributed and boldly rounded mountain summits.

In addition to the marks in the scenery of the country already enumerated, may be mentioned, as a similar effect of the Glacial Period, the formation of that mass of unstratified *débris* known as the *Boulder Clay*. This Boulder Clay is a mixture of all kinds of rock material, including fragments of all sizes and shapes (many being angular, and polished and striated by the action of ice) imbedded in earth composed in different districts of different materials. For some time the origin of this mass of heterogeneous *débris* was much disputed; but now all the most eminent geologists agree in regarding it as an accumulation formed chiefly under a great ice sheet which once enveloped the whole of the British Isles. Accumulations of *débris* on a very much smaller scale are formed underneath existing glaciers, and have received from the French geologists the name of *moraines profondes;* but owing to the Boulder Clay being so extensively distributed throughout Great Britain, and occurring in such large quantities (the district of Holderness, in Yorkshire, is formed entirely of it), it is considered that the British Boulder Clay forms the vast effect of a mighty power of erosion which was once exerted upon the whole country by an ice sheet which entirely buried it, and ploughed up the surface of the land, grinding and mixing together into a mass of confused rubbish the materials of which such surface was composed. A fact which is confirmatory of this explanation of the origin of Boulder Clay is that that formation, wherever found, consists chiefly of fragments of the neighbouring rocks. For example, in the neighbourhood of the chalk we have the "Great Chalky Boulder Clay," which is absent from districts remote from the chalk; in the neighbourhood of the Lias the Boulder Clay teems with worn fossils derived from that formation, and in like manner elsewhere, the great glacial sheet having in every case formed its *débris* of the materials over which it passed.

A very important, and at the same time very interesting, effect of the Glacial Period, is the great part that period took in the formation of numerous *lakes*. To Professor Ramsey is due the credit of having first discovered the interesting connection between the marks of glacial action found so widely distributed in the north temperate zone and the occurrence of a great number of lakes in that part of the globe. When we look at the map of the world we cannot fail to be struck by the remarkable number of lakes found in all those regions where evidences of ice action are found. In the American continent we find the whole territory of British North America (a region containing abundant evidences of a former glacial condition) covered with lakes too numerous to count. In the British Isles, where, as we have seen, the Glacial Period left such striking marks upon the scenery of the country, we find Scotland, Wales, and the north of England (all severely glaciated districts) teeming with lakes, while the similar regions of Switzerland, Scandinavia, and Finland furnish a no less remarkable number. The occurrence of such a great number of lakes in regions bearing such evident marks of glacial action could not be without meaning, and Professor Ramsay was led by the remarkable coincidence of the two phenomena to ascribe both to the same cause, and so to formulate a theory of the origin of a large class of lakes by the eroding power of glaciers. Taking the Lake of Geneva as an object of special study, he was brought, by a due appreciation of the great grinding power which a moving mass of ice 4,000 ft. or 5,000 ft. in thickness (with its numerous imbedded sharp and angular stones) must necessarily have exerted upon underlying rocks of soft material, to consider a large number of lakes found in countries bearing marks of glacial action, as the effects of the Glacial Period. As facts confirmatory of this theory, it may be noted that nearly all such lakes occur in the lines of old glaciers, and that in a great number of them the rock basin of the lake is much deeper than the outlet channel, a fact which entirely precludes their formation by running water, and is only explicable by the greater eroding power of a glacier when of great thickness that when, lower in its course, it gradually becomes thinner as it approaches the melting point. Very many lakes in the regions we have mentioned lie in rock basins of this form, and it is easy to see that such form is just what we might expect to result from the grinding power exerted upon the underlying rocks by a descending glacier; such power being greatest where the ice was thickest, and where its pressure was most nearly vertical.

But it is very interesting to know that rock basins of this form occur on the coasts of countries once exposed to ice action *beneath the sea-level*, and are then known as *fiords* or *lochs*, the characteristic features of which are that they are shallower at their entrances than further up, the inland recesses of the fiord being generally much deeper than the mouth. As this construction is identical with the construction of that class of lakes we have been considering it becomes obvious that fiords are simply rock-bound, glacial-formed lakes, the outlets of which, having become submerged, have admitted the waters of the sea. These fiords or lochs form a grand and beautiful characteristic of the scenery of Norway and Scotland, two countries in which, perhaps, the effects of the Glacial Period have been greater than anywhere else.

Although the great majority of lakes due to glacial action in the north temperate zone lie in *deep rock basins*, and were probably formed by the *scooping out* power of glaciers, some few lakes in that region, also the effect of the Glacial Period, have been caused by the *damming up of valleys by terminal moraines*. Such lakes, however, are generally small, and are often known as *tarns*. They occur plentifully among the mountains of Wales and the Italian Alps.

Lakes, or rather ponds, of another class which may also be included indirectly in the Effects of the Glacial Period, have been formed in hollows or *inequalities in the boulder clay.* Examples of such ponds are easy to find in Yorkshire or anywhere else where boulder clay exists in any considerable quantity. Being generally shallow, they have a great tendency to get filled up with vegetation and become converted into peat mosses or bogs.

The Glacial Period has also played an important part in the formation and modification of *valleys.* It is easy to see how, in districts where the rocks are composed of very soft material, great hollows or valleys might be scooped out by the eroding power of glaciers, and to this agency has been attributed the formation of the beautiful dales of Yorkshire and other valleys. But, generally speaking, the effect of the Glacial Period with regard to valleys has been more to modify and deepen than to form them, as in all probability the principal valleys were formed before the Glacial Period, and then determined the flow of the glaciers, which have, in almost every case, considerably modified them, and left indisputable marks of an Ice Age in the northern part of the world.

It has been thought, not without some degree of plausibility, that one of the most immediate effects of the Glacial Period would be the *occurrence of great floods* and the *submergence of large tracts of land,* as the glacial condition of things approached its termination. For it has been calculated that during the Glacial Period such vast masses of water would be locked up as snow and ice, that the average level of the sea must have been at least 1,000 feet lower than at present. This would cause considerable tracts of land to be laid bare, which, as the ice melted and the average level of the ocean rose, would be subjected to floods, and ultimately to submergence. This supposition receives support from the great number of traditions current among mankind concerning floods, and also concerning the former existence of countries now submerged. Such traditions are not without their value, for it is now an acknowledged fact that man existed during the Glacial Period (the discovery of his tools and implements in glacial formations having settled the question), and, such being the case, it is only reasonable for us to expect to find in his descendants dim traditions of those great events and changes which must have occurred towards the close of that period, and of which he was doubtless an interested spectator.

We have now dealt with what may be arranged under the head—Inorganic Effects of the Glacial Period. In our next article we propose to consider the Organic Effects of the same.

ROBERT B. COOK.

URANUS AND NEPTUNE IN A THREE-INCH TELESCOPE.

BY A FELLOW OF THE ROYAL ASTRONOMICAL SOCIETY.

IN one sense this series of essays would be incomplete were no reference made in them to the aspect in our instrument of the (as far as is at present known) two outermost members of our solar system. A three-inch telescope is hardly the one which the observer would select for the scrutiny of these dim and distant orbs: but, if we are to view them at all, we must employ the optical means at our disposal, and make the most of what we possess. As will be seen from the Zodiacal Map on p. 165, Uranus is now coming into a favourable position for examination. He is so close to β Virginis as to be well within a tolerably low power field together with that star. Even with a power that will include the star and the planet in the same field a very notable difference in their aspect is perceptible; but we must use all the magnification that our telescope will admit of to see Uranus to the greatest advantage; and under such a power he was absolutely isolated in the field of view when the subjoined drawing was made.

Uranus, March 16, 1884, 9h. 15m. G.M.T., power 250.

It will be seen that the planet exhibits the appearance of a small greyish-blue disc, seemingly perfectly uniform in tint and without markings of any kind. That the disc, however, is planetary and not stellar is evident enough with the power we are using, and may be rendered even more apparent by turning the telescope on to β Virginis, and comparing the two images. The difference between the pale diffused disc of Uranus and the sharp and brilliant one of the star, with its single diffraction ring (wholly wanting, of course, in the former,) will instantly strike the eye. That, however, much more will even be found out as to the physical aspect of the planet, is—in the existing state of practical optics—doubtful. Light itself, travelling at the rate of nearly 187,000 miles in a second, takes more than 2 hours and 28 minutes to pass across the stupendous interval which separates us from this remote world when he is in opposition to the sun. The student will have read that Uranus is attended by four satellites; but it is quite needless to add that they are utterly beyond the power of the telescope we are employing. Probably no human eye, save one, has ever seen these extremely minute objects with less than about 7 inches of aperture, the solitary observer to whom we refer being that excellent and almost supernaturally keen-sighted one, Mr. J. W. Ward, of Belfast, who did actually glimpse the two outer satellites with only 4·3 in. of aperture on many occasions during the early part of the year 1876! The reality of this quasi-miraculous feat was placed beyond doubt by the subsequent comparison of Mr. Ward's diagrams made at the telescope, with Mr. Mark's calculated ephemerides of the satellites. Uranus, we may add, is just visible to the naked eye. Of Neptune little need be said. He is at present lost in the glare of the sun's light. He will be well placed next November for the observer in the confines of Aries and Taurus, about 5° south-west of the Pleiades. In a three-inch telescope with a power of 250, he looks something like an eighth magnitude star; but, as in the case of Uranus, he exhibits no diffraction rings and is dimmer than an ordinary fixed star. It, however, requires a large and powerful telescope to exhibit Neptune with an unmistakably planetary disc, and the observer with an instrument of the size of that whose use is pre-supposed in these papers may be contented if he can fairly satisfy himself that it is not a star that he is looking at.

THE production of the Lake Superior copper-mines for 1883 was sixty million pounds of copper.

TELEGRAPH OFFICES IN CANADA.—The total number of telegraph offices in Canada is 2,259, the highest number *pro ratâ* to the population of any country in the world.—*Electrician.*

HOW TO GET STRONG.

(*Continued from page 159.*)

TO STRENGTHEN THE LOIN-MUSCLES.

WE have attended sufficiently to the muscles of the abdomen. The loin-muscles must now have their turn; then the side muscles of the waist.

Probably there is no set of muscles telling more on the strength of the body as a whole than those of the loins, or the muscles in the small of the back along either side of the spine. In nearly every form of exercise these muscles are taxed; so that if they are weak a man cannot do well in rowing, leaping, running, lifting, or any exercises akin to them. But perhaps the most important consideration in connection with these loin-muscles is that at any time we may be called on to use them in such a way that if they are weak they may be strained and perhaps permanently injured. Some one falls in a swoon, perhaps, and must be lifted; but in the effort to lift even the light form of a delicate girl the muscles of the loins, if at all weak, are severely taxed. Or you may be obliged in travelling to haul a heavily-loaded valise into a railway carriage or out of it or across a platform or up steps, no porter being about who will do the work for you. I have known a man apparently strong disabled for four or five days by a strain of the loin-muscles caused by unwonted labour of this sort. On another occasion a friend of mine (who certainly was tolerably weak all round) "ricked his back" as he called it in trying to pull off the top of a wooden case: it is true that in this case the effort would have been futile if his strength had been thrice what it was; but had his loin-muscles been in reasonably good condition he would not have hurt them even by trying to do more than they were equal to. One of the greatest advantages of all training for the muscles, indeed, resides in this, that they as it were learn their own strength, and are not easily persuaded to overtax themselves. Ladies for instance who seldom take pains to strengthen their wrist-muscles, though these are at least as useful to the fair sex as to us of the rougher sort, are much more apt to sprain their wrists in simple every day work than men who are constantly taxing their wrists' strength. Yet men are not commonly troubled with any thought of the fitness of their wrist-muscles to do the work they want from them; they will strain these muscles to their full strength and get no harm. The weaker-wristed carefully refrain from anything likely to tax their wrists, yet quite often make mistakes and get a sprain.

Probably the best steady exercise for the loins is one which most of us have "handy by"—gardening. Digging especially is splendid work for the loins, though trying if they are weak. Best begin with lighter work,—raking, hoeing, dibbling, planting, any work in the garden almost, for nearly all garden work involves leaning over and moving that which one has to stoop more or less to reach. After awhile, digging will not overtax the loins: and then nothing better can be recommended for giving strength to the muscles of the loins and the small of the back, than a good spell with the spade. Not necessarily every day,—once or twice a week will do very well. The exercise may be made interesting by studying floriculture a little; and skill in gardening exercises generally gives by no means slight pleasure. To be able to take an effective turn at digging, hoeing, and so forth, is as pleasant as to be able to pull an efficient oar. It is useful to boot. If there is no garden for flowers, one can nearly always find digging-work which will not be wasted. For instance one can level a piece of ground which had been irregular; or dig out trenches; or make ridges; or otherwise alter the contour of the ground as one may find convenient. Even if nothing is wanted in this way, the strengthening of the loins will be well worth the trouble. Then legs, arms, and wrists gain strength during the work. The chest is not benefited; and would indeed probably suffer in the long run, if such exercise were not counterbalanced by such others as I have already described for developing the upper part of the trunk. This perhaps is the reason why so few care for garden work of the heavier sort: those who get too much of it are always round-shouldered and thin-chested. But almost every exercise is open to a similar objection. Tricycling is not good for the chest, and does harm if uncorrected; yet tricycling is splendid exercise. Rowing is equally harmful alone, yet rowing deserves the esteem in which it is held. So with digging. It is splendid work for the loins, and ninety-nine men out of a hundred want their loins strengthened, and go about at their proper peril with the relatively weak loins they have. Take then this or some kindred exercise, being careful however to get all the good it can give, and avoid any harm, by duly expanding the chest in over-head work.

Failing gardening or like work out of doors, how can the loins be strengthened by indoor exercises? Very readily. *There* is a bedroom chair with a rail half a foot from the ground,—the very thing for loin work. Standing before the chair, its back towards you, lean over (legs straight) and grasp the lowest rail of the back with the right hand: Then raise the chair steadily at arm's length (the hand and wrist getting plenty of work); then steadily lower to the ground, raise, lower, raise again,—until you have had enough. Set the chair down again, square the shoulders, and draw a full breath or two. Now repeat the process, using the left hand instead of the right.

Fill a small hand-bag pretty heavily. Now standing with the legs straight but pretty far apart and the bag just in front of you, raise it with both hands, swing it between the legs, and so soon as it has gone as far back as you find convenient, sway it forwards so as just to clear the ground and continue the sweep upwards till you are holding it out at arms' length in front of you. Lower it (arms straight) steadily till it passes again, just clearing the ground, between your legs; and continue the process till you have had enough to satisfy your loin-muscles for the time. Then, leaving the bag on the ground in front of you, go through some of the chest exercises, drawing long breaths, squaring the shoulders, and striking out to right and left. This arrests any tendency the other exercise may have had to contract the chest. So soon as the loin-muscles are ready again, give them another spell of the bag exercise. That will be enough for one day. And so much work done twice a week or so, will soon make your loin-muscles feel and know their strength. You may not be able, perhaps, as every fairly stalwart man should be, to lift a prostrate person of average weight from the ground (a fainting lady, for instance, or one who has fallen in the way of some vehicle); but at any rate you will no longer be apt to sprain your back in trying. There are in truth however no muscles better worth training (and tuning even to athletic pitch) than the commonly neglected muscles of the lumbar regions.

In the gymnasium, or in any convenient open space, another form of exercise is available and excellent. Set in a row a number of weights,—say each a stone weight to begin with, but you may easily go on till you can substitute 56 lb. weights. Standing at one end of the row, which extends straight in front of you, stoop to the first weight and seizing it with the right hand swing it forward, upward, and over your head, pitching it as far behind you as you

can. Do the like with the second weight, using your left hand and arm instead of the right. Then the third with the right; the fourth with the left; and so forth; till you have Deucalionised the lot. (It is not considered essential that any one should watch this exercise close behind to see that it is correctly done,—in the gymnasium the master, however anxious he may be to see that the exercise is symmetrically accomplished, selects a different position.)

As Mr. Blackie, who is rather silent about specific exercises for the loin-muscles, says, "in many of the heavier grades of manual labour these muscles have a large share of work to do. All stooping over, when lifting is done with a spade, or fork, or bar, whether the knees are held straight or bent, or lifting any weight directly in the hands, horizontal pulling on a pulley-weight rope or oar,—in short, nearly every sort of work where the back is thoroughly employed, keeps these muscles active. You cannot bend over without using them. Weed awhile, and unless already strong in the loins, they will ache."

Among more familiar forms of exercise, cricketing and lawn-tennis may be mentioned as excellent for the loins, though rather by way of limbering the loin-muscles than of adding greatly to their strength, though such exercises do undoubtedly strengthen them too.

Specially for limbering the loin-muscles, the toe-touching exercise common in school drilling is among the best. Every one ought to be able to touch his toes with the fingers without bending the legs; yet very few men can do this comfortably. A few minutes given to dips towards the toes every morning and evening will wonderfully limber the loin-muscles. Don't strain; the power to dip to the toes themselves, or even (as I find my boys can easily manage) to touch the ground with the knuckles or with the palm of the hand, will come gradually. Every day you advance a little. But straining does harm. Just keep steadily dipping, each time just so far as the loins allow you to go, and you will find after awhile the power to dip to the toes will come. To measure your rate of progress in this direction, you may set a book or two on the ground, which you can just dip to,—then day after day put thinner books, until at last you will (probably) be able to dip to the ground without inconvenience.

So may the loin-muscles, strengthened by the former exercises, be rendered limber and elastic.

(To be continued.)

ELECTRO-PLATING.

II.

By W. SLINGO.

IN the first article of this series (KNOWLEDGE, No. 124) it was remarked that experiment has demonstrated it to be a fact that when a current passes through a liquid containing metals in solution the metallic compound is split up more or less completely into its constituents, and the metal deposited or separated at the negative pole or electrode—that is to say, at the electrode in connection with the zinc or negative pole of the battery. Leaving for a time the separation of hydrogen (which, by the way, possesses many attributes, both chemical and electrical, characteristic of the metals), let us direct our attention to the deposition of copper.

There are several soluble compounds of copper, of which, however, two only need claim attention. Chloride of copper ($CuCl_2$) is an easily soluble compound, each molecule or particle of which contains one volumetric equivalent of copper (Cu) to two of chlorine (Cl), as indicated by the symbol. If two platinum electrodes be connected to the poles of a battery and immersed in a solution of the salt* a current passes through it which separates the chloride into its constituents, the copper being deposited as a fine metallic film on the negative platinum, while the chlorine, which is electrically separated at the positive electrode, enters into combination with it if it be a metal, and forms a chloride of that metal. Thus in the case of platinum, chloride of platinum would result, chloride of zinc or iron being formed if either of those metals are employed. The great cost of platinum is a sufficient cause to preclude the use of it for such a purpose. Iron and zinc are, however, cheap; but their use is not permissible, because their chlorides would enter into solution and impurify it. Carbon is not assailed by chlorine, but even the use of carbon is not commendable, because the solution becomes weaker as the separation of the salt goes on, unless a reservoir is provided to maintain the strength of the electrolyte (a term introduced by Faraday as a distinguishing name for any solution undergoing electrical decomposition). Furthermore, the carbon becomes partially enveloped in a film of chlorine gas, which, however, in time decomposes the water, forming hydrochloric acid, by combining with its hydrogen, and setting free its oxygen. If the positive electrode be of copper, the freed chlorine combines with it, and produces fresh quantities of chloride of copper. It is evident that if two atoms of chlorine combine with one of copper to form a molecule or particle of the chloride, the decomposition of the molecule must result in the deposition of one atom of copper and the freeing of two atoms of chlorine, which, combining with its equivalent (one atom) of copper at the positive electrode, forms a new molecule of the salt. Thus for every molecule decomposed a new one is formed, and the strength of the solution kept constant. It is eminently essential that this constancy should be ensured if a regular or continued deposit is desired.

The other salt of copper to which reference is necessary is the sulphate (SO_4Cu), or the salt which on the substitution of hydrogen for its copper becomes sulphuric acid (SO_4H_2). If two strips of platinum in connection with the poles of a battery be immersed in a solution of this salt it is decomposed by the current—copper, as before, being deposited on the negative plate, while the other portion of the molecule (SO_4) is disengaged at the positive electrode. SO_4, which has been called sulphion, is, however, incapable of a separate existence, and as it is unable to assail the platinum it decomposes a molecule of water (OH_2), and combines with the hydrogen to form a molecule of sulphuric acid, the oxygen escaping into the air as a gas. The same thing happens if carbon be substituted for platinum, but copper being assailable, the substitution of that metal for the positive platinum serves, as in the case of chloride of copper, to maintain the strength of the solution—one atom of copper being dissolved from the positive plate for every atom deposited on the negative plate.

An important question now presents itself. What relation does the amount of solution affected, or of metal deposited, bear to the current? A reference to the articles on Electrical Measurement, which appeared in the second volume of KNOWLEDGE, will demonstrate the fact that the current passing through a circuit depends directly upon its

* A salt is a compound which results from the action of an acid upon a metal, or which becomes an acid by hydrogen being substituted for the metal. Thus $CuCl_2$ is a salt resulting from the action of hydrochloric acid (HCl) on copper, hydrogen being set free in the process. It is manifest that if the copper be displaced by hydrogen hydrochloric acid (frequently called muriatic acid) would again be formed. Of this we shall see more later on, but the recollection of the definition of a salt is essential.

electro-motive force (which may be rudely described as the power the current possesses to overcome resistance or obstacles presented to its passage) and inversely upon the total amount of resistance in the circuit. In other words, if we double the electro-motive force (E M F), the resistance remaining constant, or if we halve the resistance while the E M F remains constant, we in each case double the current. Thus, if we have a current of an E M F of 2 volts,* and cause it to traverse a circuit of 2 ohms † resistance we shall get a current of $\frac{2 \text{ volts}}{2 \text{ ohms}} = 1$ ampère.‡ Doubling the E M F in one case, and halving the resistance in the other, we get the same result thus :—

$$\frac{4 \text{ volts}}{2 \text{ ohms}} = \frac{2 \text{ volts}}{1 \text{ ohm}} = 2 \text{ ampères.}$$

Roughly, to increase the E M F, we increase the number of cells, and, to reduce the resistance, we increase the size of the cells or shorten the length of wire connecting the pole of the battery. These points are thus briefly touched upon here because a consideration of them will be involved later on.

It does not require much mental exertion to see that if we have a gas or water-pipe of varying section, the quantity of gas or water passing each and every point in a given time will be identical, providing there be no intermediate outlets. Similarly, if we join up an electrical circuit, the amount of current passing each point of the circuit in a given time will be equal; in other words, the strength of the current is the same in all parts of the circuit. The sequence is that if we have fifty similar decomposition (or electrolytic) cells in circuit with a battery of commensurate proportions, the amount of metal deposited will be the same in each cell. It does not follow, however, that the same battery-power which will deposit a grain of copper in one cell will deposit a similar quantity in each of fifty cells, else we should be a long way on the road to perpetual motion. It is obvious that the resistance to decomposition offered by the salts is a factor to be considered, as well as the electrical resistance of the solutions. Fifty electrolytic cells will offer fifty times the resistance of one cell; and, were there practically no other resistance in the circuit, or, if that other resistance were negligibly small, the current would be reduced to $\frac{1}{50}$ of its strength with one cell in circuit, and supposing one grain to be deposited in the single cell in a given time, $\frac{1}{50}$ grain would be deposited in each of the fifty cells.

SPEED OF WAVES IN WATER.—M. Erington de la Croix, of Thaipeng-Pérak, has calculated the velocity of propagation of the tidal wave caused by the volcanic eruption of Krakatoa. The final moment of the explosion was about twelve minutes to noon, when a gigantic wave was formed in the Straits of Sunda. But the same day, at 1.30 p.m., a tidal wave was felt on the coasts of Ceylon, notably at Point de Galle. Assuming that this was the same disturbance propagated across the 3,000 kilomètres of sea dividing Point de Galle from Sunda, M. de la Croix calculates the velocity of propagation to be 2,000 kilomètres per hour, or 550 mètres per second. This rate is 210 mètres higher than the speed of sound in air. Further data from the island of Mauritius affords a check on this result. Here the distance is 5,500 kilomètres from Sunda, and the tidal ebb was felt at 2·15 p.m. Hence the speed per second works out as before, 550 mètres.—*Engineering.*

* The volt is the unit of E M F. See KNOWLEDGE, No. 44.

† The ohm is the unit of resistance. See KNOWLEDGE, No. 46.

‡ The ampère is the unit of current strength. See KNOWLEDGE, No. 51.

A FEW SATURNINE OBSERVATIONS.

(*Continued from page* 186.)

WELL, in whatever way we may get there, we are off now for a stroll to Saturn, with Mr. R. A. Proctor for comrade and cicerone, but turning a deaf ear to him whenever, as often occurs, he is too learned for us, and asks us to "let N P′ P″ N′ represent the northern half of Saturn's orbit (viewed in perspective), *n* E n′ E′ the earth's orbit, and N p p′ p″ N′ the projection of Saturn's orbit on the plane of the earth's orbit. Let N S N′ be the line of Saturn's nodes on this plane, and let S P′ be at right angles to N S N′, so that when at P′ Saturn is at its greatest distance from the ecliptic on the northern side." When of such things we are asked to let them be, we let them be, and are, in the denseness of our ignorance, only too glad to be allowed, not to say asked, to do so. We attend only, like most of our neighbours, to what is easy to us. Sun is gold, and moon is silver; Mars is iron, Mercury quicksilver, which we, in fact, rather like still to call Mercury, thinking nothing at all of the imprisoned god with the winged heels, when we ask how is the mercury in the thermometer. Jove is tin, Yes, by Jove, tin is the chief among the gods, says little Swizzle, who, by a miracle, remembers one thing that he learnt at school—Jove's chieftainship among the heathen deities. Venus is copper, for the Cyprian is Cuprian; and as for Saturn, he is lead. A miserable old fellow they made Saturn out in the days of the star-decipherers. Mine, Chaucer makes Saturn say, is the drowning in wan waters, the dark prison, the strangling and hanging, murmur of discontent, and the rebellion of churls; I am the poisoner and the housebreaker, I topple down the high halls and make towers fall upon their builders, earth upon its miners; I sent the temple roof down upon Samson; I give you all your treasons, and your cold diseases, and your pestilence. This is the sort of estimation in which our forefathers held the respectable old gentleman we are now going out to see.

When Galileo's eyes went out towards Saturn through his largest telescope—which, great as were the discoveries it made, was clumsier and weaker than the sort of telescope now to be got for a few shillings at any optician's shop—he noticed a peculiarity in the appearance of Saturn which caused him to suppose that Saturn consisted of three stars in contact with one another. A year and a-half later he looked again, and there was the planet round and single as the disc of Mars or Jupiter. He cleaned his glasses, looked to his telescope, and looked again at the perplexing planet. Triform it was not. "Is it possible," he asked, "that some mocking demon has deluded me?" Afterwards the perplexity increased. The two lesser orbs reappeared, and grew and varied in form strangely; finally they lost their globular appearance altogether, and seemed each to have mighty two arms stretched towards and encompassing the planet. A drawing in one of his manuscripts would suggest that Galileo discovered the key to the mystery, for it shows Saturn as a globe resting upon a ring. But this drawing is thought to be a later addition to the manuscript. It was only after many perplexities of others, about half a century later, that Huyghens, in the year 1659, announced to his contemporaries that Saturn is girdled about by a thin, flat ring, inclined to the ecliptic, and not touching the body of the planet. He showed that all variations in the appearance of the ring are due to the varying inclinations of its plane towards us, and that being very thin, it becomes invisible when its edge is turned to the spectator or the sun. He found the diameter of the ring to be as nine to four to the diameter of Saturn's body,

and its breadth about equal to the breadth of vacant space between it and the surface of the planet.

The same observer, Huyghens, four years earlier, discovered one of Saturn's satellites. Had he looked for more he could have found them. But six was the number of known planets, five had been the number of known satellites (our moon, and the four moons of Jupiter, which Galileo had discovered); one moon more made the number of the planets and of the satellites to be alike, six, and this arrangement was assumed to be exact and final. But in sixteen-'seventy-one another satellite of Saturn was discovered by Cassini, who observed that it disappears regularly during one-half of its seventy-nine days' journey round its principal. Whence it is inferred that this moon has one of its sides less capable than the other of reflecting light, and that it turns round on its own axis once during its seventy-nine days' journey; Saturn itself spinning once round on its axis in as short a time as ten hours and a half. Cassini afterwards discovered three more satellites, and called his four the Sidera Lodoicea, Ludovickian Stars, in honour of his patron, Louis XIV. Huyghens had discovered, also, belts on Saturn's disc. Various lesser observations on rings, belts, and moons of Saturn continued to be made until the time of the elder Herschel, who, at the close of the last century, discovered two more satellites, established the relation of the belts to the rotation of the planet, and developed, after ten years' careful watching, his faith in the double character of its ring. "There is not, perhaps," said this great and sound astronomer, "another object in the heavens that presents us with such a variety of extraordinary phenomena as the planet Saturn; a magnificent globe encompassed by a stupendous double ring; attended by seven satellites; ornamented with equatorial belts; compressed at the poles; turning on its axis; mutually eclipsing its rings and satellites, and eclipsed by them; the most distant of the rings also turning on its axis, and the same taking place with the furthest of the satellites; all the parts of the system of Saturn occasionally reflecting light to each other—the rings and moons illuminating the nights of the Saturnian, the globe and moons enlightening the dark parts of the rings, and the planet and rings throwing back the sun's beams upon the moons when they are deprived of them at the time of their conjunctions." During the present century, other observers have detected more divisions of the ring, one separating the outer ring into two rings of equal breadth seems to be permanent. It is to be seen only by the best telescopes, under the most favourable conditions. Many other and lesser indications of divisions have also at different times been observed. Seventeen years ago an eighth satellite of Saturn was discovered by Mr. Bond in America, and by Mr. Lassell in England. Two years later—that is to say, in November, eighteen 'fifty, a third ring of singular appearance was discovered inside the two others by Mr. Bond, and, a few days later, but independently, by Mr. Dawes and by Mr. Lassell in England. It is not bright like the others, but dusky, almost purple, and it is transparent, not even distorting the outline of the body of the planet seen through it. This ring was very easily seen by good telescopes, and presently became visible through telescopes of only four-inch aperture. In Herschel's time it was so dim that it was figured as a belt upon the body of the planet. Now it is not only distinct, but it has been increasing in width since the time of its discovery.

These were not all the marvels. One of the chief of the wonders since discovered was a faint overlapping light, differing much in colour from the ordinary light of the ring, which light, a year and a half ago [this was written in 1865. R. P.], Mr. Wray saw distinctly stretched on either side from the dark shade on the ball overlapping the fine line of light by the edge of the ring to the extent of about one-third of its length, and so as to give the impression that it was the dusky ring, very much thicker than the bright rings, and seen edge-wise, projected on the sky. Well may we be told by our guide, Mr. Proctor, that no object in the heavens presents so beautiful an appearance as Saturn, viewed with an instrument of adequate power. "The golden disc, faintly striped with silver-tinted belts; the circling rings, with their various shades of brilliancy and colour; and the perfect symmetry of the system as it sweeps across the dark background of the field of view, combine to form a picture as charming as it is sublime and impressive."

But what does it all mean? What is the use of this strange furniture in the House of Saturn, which is like nothing else among the known things of the universe? Maupertuis thought that Saturn's ring was a comet's tail cut off by the attraction of the planet as it passed, and compelled to circle round it thenceforth and for ever. Buffon thought the ring was the equatorial region of the planet which had been thrown off and left revolving while the globe to which it had belonged contracted to its present size. Other theories also went upon the assumption that the rings are solid. But if they are solid, how is it that they exhibit traces of varying division and re-union, and and what are we to think of certain mottled or dusky stripes concentric with the rings, which stripes, appearing to indicate that the ring where they occur is semi-transparent, also are not permanent? Then, again, what are we to think of the growth within the last seventy years of the transparent dark ring which does not, as even air would, refract the image of that which is seen through it, and that is becoming more opaque every year? Then, again, how is it that the immense width of the rings has been steadily increasing by the approach of their inner edge to the body of the planet? The bright ring once twenty-three thousand miles wide, was five thousand miles wider in Herschel's time, and has now a width of twenty-eight thousand three hundred on a surface of more than twelve thousand millions of square miles, while the thickness is only a hundred miles or less. In 1857, Mr. J. Clerk Maxwell obtained the Adams prize of the University of Cambridge for an essay upon Saturn's rings, which showed that if they were solid there would be necessary to stability an appearance altogether different from that of the actual system. But if not solid are they fluid, are they a great isolated ocean poised in the Saturnian mid air? If there were such an ocean, it is shown that it would be exposed to influences forming waves that would be broken up into fluid satellites.

But possibly the rings are formed of flights of disconnected satellites, so small and so closely packed that, at the immense distance to which Saturn is removed, they appear to form a continuous mass, while the dark inner mass may have been recently formed of satellites drawn by disturbing attractions or collisions out of the bright outer ring, and so thinly scattered that they give to us only a sense of darkness without obscuring, and of course without refracting, the surface before which they spin. This is, in our guide's opinion, the true solution of the problem, and to the bulging of Saturn's equator, which determines the line of superior attraction, he ascribes the thinness of the system of satellites in which each is compelled to travel near the plane of the great planet's equator.

Whatever be the truth about these vast provisions for the wants of Saturn, surely there must be living inhabitants there to whose needs they are wisely adapted. Travel among the other planets would have its inconveniences to

us of the earth. Light walking as it might be across the fields of ether, we should have half our weight given to us again in Mars or Mercury, while in Jupiter our weight would be doubled, and we should drag our limbs with pain. In Saturn, owing to the compression of the vast light globe and its rapid rotation, a man who weighs twelve stone at the equator weighs fourteen stone at the pole. Though vast in size, the density of the planet is small, for which reason we should not find ourselves very much heavier by change of ground from Earth to Saturn. We should be cold, for Saturn gets only a ninetieth part of the earth's allowance of light and heat. But then there is no lack of blanket in the House of Saturn, for there is a thick atmosphere to keep the warmth in the old gentleman's body and to lengthen the Saturnian twilights. As for the abatement of light, we know how much light yet remains to us when less than a ninetieth part of the sun escapes eclipse. We see in its brightness, as a star, though a pale one, the reflection of the sunshine Saturn gets, which, if but a ninetieth part of our share, yet leaves the Sun of Saturn able to give five hundred and sixty times more light than our own brightest moonshine. And then what long summers! The day in Saturn is only ten and a half hours long, so that the nights are short, and there are twenty-four thousand six hundred and eighteen and a half of its own days to the Saturnian year." But the long winters! And the Saturnian winter has its gloom increased by eclipses of the sun's light by the rings. At Saturn's equator these eclipses occur near the equinoxes and last but a little while, but in the regions corresponding to our temperate zone, they are of long duration. Apart from eclipses the rings lighten for Saturn the short summer nights, and lie, perhaps, as a halo under the sun during the short winter days.

THE MORALITY OF HAPPINESS.

By Thomas Foster.

CARE FOR SELF AS A DUTY.

(*Continued from page* 165.)

It has been shown that care of self necessarily precedes care of others, because we must ourselves live if we are to benefit others. It has been shown further that if there is to be progress and improvement in the race, the superior must profit by their superiority, and so develop in numbers and influence, while the inferior because inferior become less and less predominant in the community. Further it has appeared that while a society improves as it becomes constituted more and more largely of the better sort, this improvement depends in large part on those qualities of the individual members of society which depend on due care of self. In like manner it appears that in a society whose members are not duly regardful of self misery arises from the excess of self-denial which ends by making those who practise it burdens on the rest of the community. Lastly, we have seen that due care of self is desirable, and neglect of the just rights of self injurious to the social body, because that undue care of self which is properly called selfishness and leads either to negative or positive forms of wrong-doing, thrives and multiplies in a community where the better sort allow evil and oppression to pass unchecked by the due assertion of self-rights.

But now it is worth remarking that the line of reasoning which has been followed does not in reality indicate changed conduct. It reconciles the actual conduct of the better sorts of men with rules derived from observed facts and laws in regard to the development of conduct, and would tend to reconcile their conduct with their words, if men in general would but recognise the folly and danger of a system by which they have one set of rules on their lips and another for their actual guidance. As Mr. Herbert Spencer well puts it, the general conclusion to which we have been led, "though at variance with nominally accepted beliefs is not at variance with actually accepted beliefs; while opposed to the doctrine which men are taught should be acted upon, it is in harmony with the doctrine which they do act upon and dimly see must be acted upon." "The labourer looking for wages in return for work done, no less than the merchant who sells goods at a profit, the doctor who expects fees for advice, or the priest who calls the scene of his ministrations a 'living,' assumes as beyond question the truth that selfishness, carried to the extent of enforcing his claims and enjoying the returns his efforts bring, is not only legitimate but essential. Even persons who avow a contrary conviction prove by their acts that it is inoperative. Those who repeat with emphasis the maxim—'Love your neighbour as yourself,' do not render up what they possess so as to satisfy the desires of all as much as they satisfy their own desires. Nor do those whose extreme maxim is—'Live for others,' differ appreciably from people around in their regards for personal welfare, or fail to appropriate their shares of life's pleasures. In short, that which is set forth above as the belief to which scientific ethics lead us, is that which men do really believe, as distinguished from that which they believe they believe—or pretend they believe."

Which is better?—to proclaim with our lips rules of conduct which none of us really follow and to denounce those who show that the rules which the best-minded among us really strive to follow are such as tend most to improve the condition of the body social, or frankly to recognise the just and equitable rules of conduct which after all are the real guides of the actions of all well-meaning men? Is it well or wise to discredit these fair and proper rules by setting up others which seem more self-sacrificing, but which none except a few abnormally-minded persons of no influence (objects of ill-concealed contempt among those who applaud such rules) actually strive to follow, rules moreover which if widely followed would inevitably bring misery on the community? For my own part I believe that the system by which rules no sane man follows are set up as the real laws of conduct, works most serious mischief, by discouraging many from the attempt to be consistently fair and just to those around them as well as to themselves. Of what use, they feel (rather than consciously think) is any attempt to be merely just and considerate, when still we fall far short of the standard set up for our guidance? Apart from this lies the direct mischief to character which necessarily arises from the confident expression of acceptance of rules which every man (except the few abnormal creatures I have mentioned) knows well that he does not follow, has never attempted to follow, and never intends to follow. Many are led, through their honest unwillingness thus to falsify their words by their actions, into an error of the opposite kind; preferring rather to maintain rules of conduct which have a selfish aspect, while their actual conduct is unselfish, than to ape a degree of disinterestedness which they do not possess and which would (they know) be mischievous if really possessed and acted upon by any large proportion of the community.*

* It is, by the way, rather remarkable that in proportion to the apparent zeal with which some maintain the doctrine of universal love is the intensity of hate which they express and doubtless feel (being in this at least, let us hope, honest) for those who differ from them. If the Honeythunder School of Philanthropists act

But lastly, let it be noticed that just care for self does not imply necessarily less care for others, but often more. As a mere matter of fact, men who carefully consider their own just claims are found to be more considerate, as a rule, of the claims of others, than those who assert that men ought not to be careful to consider what their just claims are. Horace long since, in his famous ode beginning *Justum ac tenacem propositi virum*, drew attention to the connection commonly existing between justice and firm maintenance of what is due to self. Of course there are men who are unduly regardful of self, not being content with the maintenance of their own rights but wilfully infringing the rights of others. Equally are there some who while negligent of their own rights are considerate of those of others. But these are the exceptions. As a rule one may recognise in *due* regard for self-rights the same principle which displays itself otherwise in care for the rights of others. Considering social as distinguished from individual opinions, assuredly Mr. Spencer is justified in what he says on the egoistic excesses which often accompany excessive altruism :—"A society in which the most exalted principles of self-sacrifice for the benefit of neighbours are enunciated, may be a society in which unscrupulous sacrifice of alien fellow-creatures is not only tolerated but applauded. Along with professed anxiety to spread these exalted opinions among heathens, there may go the deliberate fastening of a quarrel upon them with a view to annexing their territory. Men who every Sunday have listened approvingly to injunctions carrying the regard for other men to an impracticable extent, may yet hire themselves out to slay, at the word of command, any people in any part of the world, utterly indifferent to the right or wrong of the matter fought about. And as in these cases transcendent altruism in theory coexists with brutal egoism in practice, so conversely a more qualified altruism may have for its concomitant a greatly moderated egoism. For, asserting the due claims of self is, by implication, drawing a limit beyond which the claims are undue; and is, by consequence, bringing into greater clearness the claims of others."

We have next to consider the duty of caring for others, as it presents itself in connection with the Morality of Happiness. *(To be continued.)*

WEEVILS.

By S. A. Butler, B.A., B.Sc.

(Continued from page 161.)

ANOTHER creature, black and stumpy, and almost as broad as it is long, infests cabbages and other cruciferous plants. It has a short but slender rostrum, which it is able to bend completely under its body; this is rendered possible, not by any articulation of the rostrum to the rest of the head, but by the extreme mobility of the head itself, which fits into the front of the thorax by a kind of ball and socket arrangement. When the insect has thus tucked its rostrum under its body, it folds up its legs and packs them close to the body, and then looks more like a seed or a little lump of earth than anything animate. In this condition it may, like a ladybird, be handled in any way without manifesting the slightest sign of life. Feigning death is a habit commonly indulged in by beetles, and though not confined to that order of insects, is of more frequent occurrence amongst them than in other groups. Several genera of weevils manifest this habit in a most striking degree, and the apparent enlargement of the insect as it stretches out its legs again, brings forward its rostrum and opens out its antennæ, is most surprising. The black weevil referred to above is only ⅛-inch long and belongs to a large genus called *Ceuthorhynchus*, containing about forty British species, which affect various plants. The female beetle lays her eggs on the stems of cabbages near the root, in small perforations which she makes with the jaws at the end of her rostrum. Around the grubs which hatch from these, gall-like excrescences are formed; the grubs feed on the interior, and the galls increase in size with the growth of the cabbage, thus nourishing the grub at the expense of the plant. The maggot, when fully grown, nibbles its way through the rind of the gall, goes into the earth and there forms a cell in which it changes to a pupa, which, in the course of about two months, yields the perfect insect.

The nut-weevil, *Balaninus nucum*, which is remarkable as having almost the longest and slenderest rostrum to be found amongst British weevils, is covered with greyish or yellowish-brown scales, except the rostrum, which is almost bare. With the tiny mandibles at the end of her long rostrum, the female gnaws a hole through the shell of a young nut, while it is still soft, and then deposits therein an egg, which hatches in about ten days. As the nut grows, the weevil grub gradually devours the kernel, and finally eats its own way through the nutshell, either after the nut has (somewhat prematurely) fallen, or before this takes place. The grub falls to the ground, buries itself, and snugly ensconces itself in the soil in a cell of its own manufacture. In this condition it frequently passes the winter, changing in the following spring to a pupa, whence in due course issues the perfect beetle.

Another insect of a reddish colour, with white bands on its back, treats the flower-buds of apple trees in a similar way, devouring all the central organs of the buds and so rendering them abortive. Instead of falling to the ground, however, it undergoes all its metamorphoses cradled in the bud itself.

A species found commonly on lucerne, makes of a gummy substance it secretes, a delicate oval cocoon of open network, on a leaf of its food-plant; in this it encloses itself, as in a cage, in order to pass through its pupahood.

One of the largest of our British species, the Pine Weevil, *Hylobius abietis*, attacks various trees of the pine family. It is a blackish-brown creature, with yellowish markings. The beetle itself devours the tender bark of young twigs, sometimes completely stripping them. The eggs are deposited in various situations where the larvæ will be able to find easy access to the wood of the trees. The grubs, on hatching, begin to tunnel under the bark, forming galleries, the dimensions of which are increased as the insect grows. In course of time, after many months of mining and depredation, these galleries become partially filled with what is called "worm-meal," the remains of the wood-gnawing of the larvæ, and if the burrow be traced to its end, there will be found a snug little cavity hollowed out amongst the chips, where the pupa nestles. The beetles pass about a month in the pupa state, and then issue as perfect insects to pursue their devastations upon the vegetable products of another season. Many wild plants are particularly liable to the attacks of weevils. The figworts (*Scrophularia*) may sometimes be seen in July and August with their leaves all brown and withered, and riddled through and through with tiny holes, as though they had received charge upon charge of miniature shot. This is the work of some very beautiful insects, the genus *Cionus*, which have a squarish body, handsomely adorned with variegated, hair-like scales.

This paper must not be concluded without a reference to

seemingly on the principle, "Curse your souls and bodies come here and be blessed," these seem to adopt as their rule, "Let us hate with all our might those who will not allow us to love every one better than ourselves."

two insects, for the presence of which in Britain we are indebted to our extensive commerce—the corn and rice weevils, *Calandra granaria* and *oryzæ*. These are not truly British insects, but were originally accidentally introduced from abroad with foreign wheat. They feed, not upon growing plants, but upon grain, after it has been stored up. They are small, reddish-brown beetles, about ⅛ in. long. The parent deposits a single egg in each grain; the larva developed from this lives inside the grain, gradually consuming all the interior, and leaving only the husk, which, however, remains intact, so that no trace of the depredator is noticeable. Inside the husk the little being pupates, and when fully developed, it easily ruptures its prison walls and makes its exit, a perfect beetle, on the look-out for a mate, and prepared to renew, probably to a much greater extent than before, the work of destruction. Though individually minute, and making no further call upon the corn-merchant's stores than the contents of a single grain for the nourishment of each during its larvadom, these insects are so prolific, and occur in such enormous numbers as to be terribly destructive, and sometimes to entail serious loss upon the owners of the grain.

Fig. 3.
Corn Weevil.

Owing, however, to improved appliances, and greater care in the shipping and storage of the grain, the damage done now-a-days is much less than formerly. I am informed by a friend who has had much experience in these matters, that the beetles do not readily breed in this country, and that most of the damage wrought by the larvæ is effected before the grain arrives here, the chief depredators after it is actually stored being the perfect insects themselves. These, however, occasionally occur in immense swarms, and there are on record instances of such vast numbers having been screened out of infested corn that they could be weighed only by the hundredweight! Weevils are not the only beetles that attack corn in this way. On one occasion, no less than twenty species belonging to several families were obtained in varying numbers from a small collection of granary-sweepings, which yielded in all six ounces of insects, containing over 2,000,000 specimens. Facts like these go far to show the economic importance of entomology, an aspect of the science that, curiously enough, has not until recently attracted much attention in this country; one reason for this may, no doubt, be found in the fact that unfortunately agricultural and entomological tendencies do not often co-exist in the same individual.

ALMANAC LESSONS.

By Richard A. Proctor.

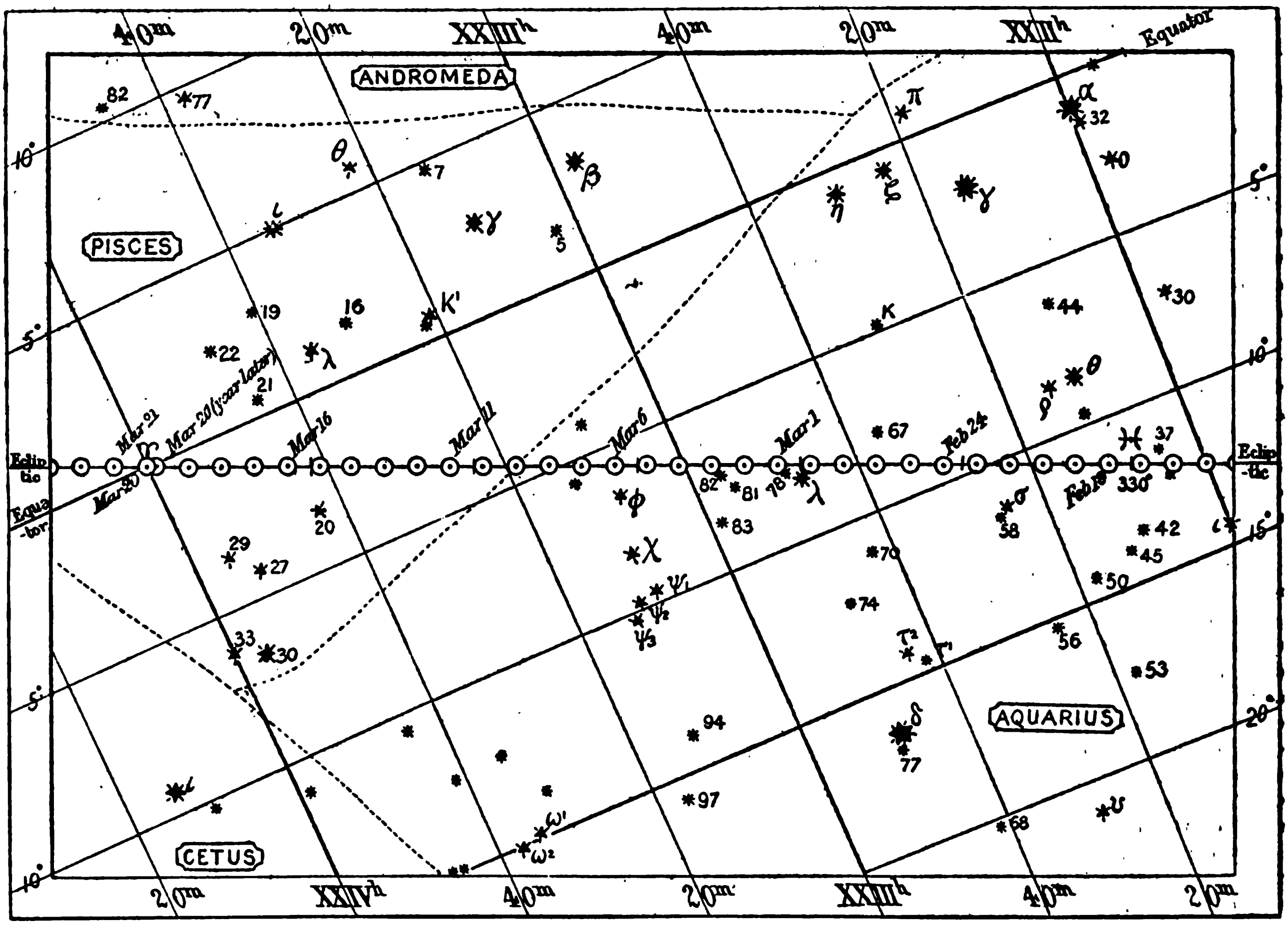
During the months of February and March, the sun's hour of rising ranges from 7h. 42m. to 5h. 38m., his hour of setting from 4h. 46m. to 6h. 30m. The time of southing ranges from 13m. 43s. p.m. at the beginning of February to 14m. 28s. p.m. on the 11th, and thence to 4m. 4s. p.m. at the end of March. The following table represents these changes as they actually occur, the last column showing how the equation of time, or the interval between clock noon and solar noon, varies throughout the two months.

If the student remembers that the earth reaches her mean distance—on her way towards aphelion—near the end of March, so that all this time the sun moves with rather more than his mean apparent velocity eastwards, he will see that so far as the equation of time depends on this relation it would be increasing to the end of March. But as during the last half of the quarter ending March 20th, the sun is crossing the meridianal circles aslant, and where they are farther apart, so that he is passing westwards less quickly than the average, the equation of time more rapidly diminishes. Hence after attaining a maximum in the second week of February, the equation of time diminishes steadily to and beyond the end of March.

A study of the Day-Sign Maps for last month and this (the latter appears this week) will make it plain that about February 11 the sun has his average eastwardly motion, and thereafter less than the average, so that each day he loses more and more of that easting which makes him come to the meridian, or due south, after clock noon.

On February 19 the sun enters the sign Pisces, on March 20 he enters the sign Aries.

	The Sun Rises. H. M.	The Sun Sets. H. M.	After Clock. M. S.	Hourly Variation of Equation of Time. S.
Feb. 1	7 42	4 46	13 47	+0·34
„ 2	7 40	4 48	13 55	0·31
„ 3	7 38	4 50	14 2	0·27
„ 4	7 37	4 51	14 8	0·24
„ 5	7 35	4 53	14 13	0·20
„ 6	7 33	4 55	14 18	0·17
„ 7	7 32	4 57	14 21	0·13
„ 8	7 30	4 59	14 24	0·10
„ 9	7 28	5 1	14 26	0·07
„ 10	7 26	5 2	14 27	+0·03
„ 11	7 25	5 4	14 28	±0·00
„ 12	7 23	5 6	14 27	−0·03
„ 13	7 21	5 8	14 26	0·06
„ 14	7 19	5 10	14 24	0·09
„ 15	7 17	5 12	14 22	0·12
„ 16	7 15	5 13	14 19	0·15
„ 17	7 13	5 15	14 15	0·18
„ 18	7 11	5 17	14 10	0·21
„ 19	7 9	5 19	14 4	0·24
„ 20	7 7	5 21	13 58	0·27
„ 21	7 5	5 23	13 52	0·29
„ 22	7 3	5 24	13 45	0·32
„ 23	7 1	5 26	13 37	0·34
„ 24	6 59	5 28	13 28	0·37
„ 25	6 57	5 30	13 19	0·39
„ 26	6 55	5 32	13 9	0·42
„ 27	6 53	5 33	12 59	0·44
„ 28	6 50	5 35	12 48	0·46
„ 29	6 48	5 37	12 37	0·48
Mar. 1	6 46	5 39	12 25	0·51
„ 2	6 44	5 40	12 13	0·53
„ 3	6 42	5 42	12 0	0·55
„ 4	6 40	5 44	11 46	0·57
„ 5	6 37	5 46	11 32	0·59
„ 6	6 35	5 47	11 18	0·60
„ 7	6 33	5 49	11 3	0·62
„ 8	6 31	5 51	10 48	0·64
„ 9	6 29	5 53	10 33	0·65
„ 10	6 26	5 54	10 17	0·67
„ 11	6 24	5 56	10 1	0·68
„ 12	6 22	5 58	9 45	0·69
„ 13	6 20	5 59	9 28	0·70
„ 14	6 17	6 1	9 11	0·71
„ 15	6 15	6 3	8 54	0·72
„ 16	6 13	6 5	8 36	0·73
„ 17	6 10	6 6	8 19	0·74
„ 18	6 8	6 8	8 1	0.74
„ 19	6 6	6 10	7 43	0·75
„ 20	6 4	6 11	7 25	0·75
„ 21	6 1	6 13	7 7	0·76
„ 22	5 59	6 15	6 49	0·76
„ 23	5 57	6 16	6 30	0·76
„ 24	5 54	6 18	6 12	0·76
„ 25	5 52	6 20	5 54	0·77
„ 26	5 50	6 21	5 35	0·77
„ 27	5 47	6 23	5 17	0·76
„ 28	5 45	6 25	4 59	0·76
„ 29	5 43	6 26	4 40	0·76
„ 30	5 41	6 28	4 22	0·76
„ 31	5 38	6 30	4 4	−0·76



THE DAY-SIGN FOR MARCH.

WE give this week the day-sign for the month, with the sun's course thereon,—not as given in the almanac for this year, but as it would be during the first 11 days of a year beginning on March 20th, and during the last 20 days of such a year—so that, we have for March 20 *two* positions of the sun, corresponding to the extra quarter day in the solar year of 365¼ days.

COLLEGE EIGHT-OARED RACES AT CAMBRIDGE.

THE principal college races are the eight-oared bumping races. Every club sends in one or more boats to take part in these races, and the result is that some forty or fifty boats contend in them. It will readily be conceived that this number would be largely diminished if the object of the races were merely to determine which club could supply the best eight. Thirty or forty boats, having no chance from the beginning, would be withdrawn. But the races are so managed that every boat goes in with a fair chance of victory. The boats are arranged in two divisions, and each division starts some twenty or twenty-five boats, one behind the other, in each race. A boat that can draw up to and touch the boat in front has gained a victory; the coxswain hoists the club-flag in the stern of the boat, and the men row rejoicingly home. Next day both the vanquished boat and the victorious one again take part in the race, but they exchange places. Perhaps the vanquished boat may now be "bumped" by the boat next behind; or they may retrieve their defeat by bumping their former victors; or they may remain, as Mr. Micawber would say, *in statu quo.* So the victorious boat of the day before may make a new conquest (or "rise another place," as it is technically expressed), or be "bumped back again," or remain where they were. This process goes on all along the line of boats for eight days, by which time the boats have commonly begun to find their proper places. It often happens, however, that a good boat which happens to be low down will make bump after bump all through the eight days of the races. There is a notion that when this happens the victorious boat may challenge any boat above it to a time-race over the racing-course, and if victorious take that boat's place. But this is a myth. Year after year the eight-oar races are renewed, the boats commencing each set of races in the places they occupied at the end of the preceding year's races. There are, of course, continual changes of fortune. Sometimes a college which has managed to become "head of the river," that is, to get the first place of all, will maintain that place for several years, and then the loss of two or three good oarsmen will give the boats behind a chance. Then the opening day of the races is looked forward to with intense excitement. Whether Jesus, Trinity, or John's, as the case may be, will keep the headship or not becomes the great question with all the rowing men of the University. The practice of the best boats is watched and timed, bets are made for and against the leading clubs, and all sorts of reports get abroad as to the condition of the crews. Usually the struggle for pride of place is a close one. Sometimes the head boat will escape bumping as by a miracle for several days, and at last be run into close by the winning-post. At others, the first

day of the races settles the question, and then frequently the late "head of the river" loses a place day after day almost to the end of the eight days.

Gossip.

The recent fatal explosions with lime-light apparatus have naturally excited a good deal of attention. They have shown at once the danger which arises from the ignorant use of explosive mixtures, and the singular extent of ignorance about very simple matters. A collection of the various blunders made in the newspaper accounts of the explosions at Oldham and Chedderton would be amusing if the matter were not so serious. Unfortunately the ignorance thus displayed by those who could hardly be expected to know much about explosive gases, is displayed also by some of those who, as they use such gases in magic-lantern entertainments, ought to know something about them.

The following ideas are prevalent, it would seem:—(1) Oxygen is an explosive gas: it will certainly explode if a light is brought near it, but it *may* explode if the temperature around is unduly raised,—as for instance in a crowded hall on a warm day. (2) Hydrogen also is an explosive gas: it will certainly, &c., &c. (3) Oxygen and hydrogen must both be kept under pressure or they will explode anyhow. (4) Oxygen escaping into a room, through any hole which a member of an audience may prod through the pressure bag, will poison those around. (5) The intense lustre of the oxyhydrogen flame (*sic*) shows what an intensely dangerous mixture we have to deal with. (6) The explosive gases used to obtain the electric light are still more dangerous. Hence (7) "no person" (says the coroner's jury at Chedderton) "should be permitted to give public exhibitions in which explosive gases are employed, without a licence." And (8) lastly, "we" says the *Globe*, "would go a little further by adding, 'and the State should refuse licences for that purpose.'"

The ideas numbered above are to some slight degree erroneous from the first even unto the last.

Let us consider under what circumstances explosion can arise, and we shall be able to see, first how safety may be secured by due care suggested by knowledge, and secondly, how safety may be secured without any care at all, which is the kind of safety people want when they have no means of knowing whether the lantern manager is a person of experience or not.

In the ordinary form of an oxyhydrogen lamp we have such an arrangement as is shown in the accompanying figure. L is a cylinder of lime; F (Fig. 1) is a flame produced by igniting a mixture of oxygen and hydrogen issuing from a jet J; O is the pipe by which the oxygen is conveyed to the jet, H that by which the hydrogen is brought there, each gas being forced by pressure towards J. (Instead of hydrogen the ordinary lighting gas—carburetted hydrogen—is often employed, in which case the pressure is applied at the gas-works.) Now so long as the pressure is maintained equally on the two gases, this arrangement is perfectly safe. But suppose the pressure much reduced on either the oxygen or the hydrogen, then it is obvious that the gas which is under greater pressure will force its way into the vessel intended only to hold the other gas. When the mixture of oxygen and hydrogen thus formed attains a particular proportion, explosion occurs when a light is applied, the two gases being transformed into aqueous vapour or steam at high temperature, and the vessel which had contained the explosive mixture being burst into fragments.

At Indianapolis I gave a lecture in 1876, where the lanternist used strong iron cylinders to hold his gases; his careless haphazard way of working caused me more annoyance than anxiety, because I supposed that careless as he was he would never use his two gas cylinders without making sure that in each the gas was adequately compressed to ensure safety. But I was really lecturing, unwittingly, at the risk of my life; for, only a short time after, this man so mismanaged matters that the gases got mixed in explosive proportion in one of the cylinders, the result being that the cylinder was burst like a shell, and two men were killed. The lanternist survived, and in 1879 I had the pleasure of being assisted by him again, but not at Indianapolis. I was not so comfortable on the platform as I like to feel.

I have now always my own lantern, and it is provided with the arrangement which is safe even though the lantern manager should not only be inexperienced but homicidally disposed. F (Fig. 2) is a flame produced by igniting a mixture of oxygen and hydrogen issuing from *two* jets, one

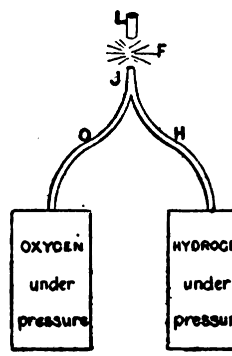


Fig. 1.—Arrangement which is safe with due care.

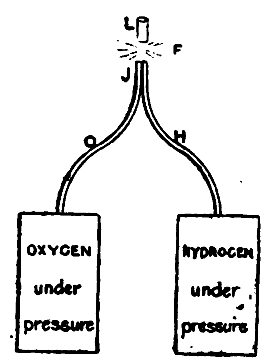


Fig. 2.—Arrangement which is safe with or without care.

supplied with oxygen by pipe O, the other with hydrogen by pipe H. Then it is obvious that if the pressure is not properly maintained the oxyhydrogen flame will wax faint, but neither gas can get into the compartment holding the other gas, for there is no connection between the two compartments.

I luckily have the cleverest and coolest assistant,—though young,—that I have ever known. But if, unfortunately, I had to employ on any occasion one of the most nervous and excitable temperament, or as careless as my actual assistant is careful, no harm could happen either to my audience or myself,—except delay and bad lighting.

I have made another calculation of the rotation-period of Mars, using as before old Hooke's observation of March, 1666, at the further end, and a recent observation at the nearer. It is hardly necessary to say that no correction has been introduced, or could be, through the extension of the long period by a few years only. The period comes out as before—24 h. 37 m. 22·7s. I never felt any doubt about this value except when I found that Kaiser, of Leyden, made the period 24 h. 37 m. 22·6 s., using like myself some

200 years' observations. Then, knowing his accuracy, I expected to find I had made some mistake somewhere. But detecting none, I went carefully through his work, and found an excess of two days in all his intervals between the seventeenth and the nineteenth centuries. I have no doubt this arose from his counting the years 1700 and 1800 as leap-years, whereas, these numbers not being divisible by 400, those years were common years.

IF any observer, armed with a good telescope and suitable micrometric appliances, will send me any times of the central passage of Kaiser's Sea, or—perhaps preferably—of the cape from which Beer and Mädler counted Martian longitudes (it has been named, but not by me, Proctor Cape) I will compute the rotation-period starting both from Hooke's observation in 1666 and from those observations by Huyghens which Kaiser employed, though as a matter of fact they give appreciably the same result.

THE rotation-period of Mars thus determined serves to test time-observations of Mars' rotation extending over any period from a few months to twenty or thirty years. It cannot (as some seem to imagine) be tested by them—any more than a clock rated within five seconds per month could be corrected by a chronometer only rated within a second per day.

AT last we have settled how our Chess columns will in future be arranged. It is meant for home chess-players, just as the hints and suggestions of "Five of Clubs" are meant for home whist-players, though many club-players show lamentable ignorance even of the simple principles which our Whist columns expound. As I am myself only a home chess-player (having never taken part in any public chess or been a member of any chess club) I know what they want better than the professional player can be expected to do. Their usual shortcoming is found in a lamentable ignorance not only of the best openings but of the principles on which the openings at chess depend. But they do not need elaborate analysis of the openings, twenty moves deep. They will not follow lines of play which seem to require so much study. But on the other hand the discussion of elementary principles is not needed. I propose therefore to submit fortnightly to the supervision of the brilliant "Mephisto" such simple expositions of the more familiar lines of opening as may serve, if followed, to change the haphazard opening play of the average home player to such sound, well-considered lines of play as can alone make chess interesting. For chess, as it is too commonly played in the home circle, is little better than a chance game, and no more deserves to be regarded as a scientific recreation, really valuable because requiring and exercising skill, than does that preposterous four-handed card game which many of the "folks at home" imagine to be whist.

IN response to numerous requests, "Five of Clubs" will take an early opportunity of quoting the half column in which he summarised all the leads which have commonly to be considered at whist. Ten minutes devoted to the study of this very short and simple summary would save bewildered partners much anguish and tribulation of spirit.

I MAY remind my readers that the third and fourth of my Series of Lectures in the Memorial Hall, Farringdon-street, will take place on Monday and Thursday of next week, and that in the same week I shall lecture at Brixton Hall. It is gratifying to receive as warm a welcome in the metropolis as was accorded me in the provinces.

POETRY OF SCIENCE.—There are some who seem to think that science cannot be truly called science if clothed in poetic garb. And on the other hand there are those who seem to fear lest the grace and beauty of the universe should be lost, lest a glory should depart from the face of nature, if science should scrutinize the mysteries of nature too closely. These fears are alike uncalled for. Science need not be less exact though poetic fancies underlie its teachings; while beautiful and glorious though the aspect of nature, as all can see her, may be, a deeper poetry, a more solemn significance, a greater beauty and a nobler glory, exist in the aspect of nature as she is seen when science raises the veil which hides her features from ordinary vision. It may indeed be truly said that no one who studies aright the teachings of the profoundest students of nature, will fail to perceive that the Galileos, the Keplers, and the Newtons, the Priestleys, the Faradays, and the Tyndalls, have been guided in no small degree by poetic instincts—nay that their best scientific work has owed as much to their imagination as to their reasoning and perceptive faculties. On the other hand, from the time when Shakespeare penned that noble apostrophe to the stars:

> There's not an orb that thou behold'st
> But in his motion like an angel sings,
> Still quiring to the young-eyed Cherubim,

and later, as the true theory of the universe dawned on mankind, our great epic poet presented in a grand passage of the "Paradise Lost" the theories of Galileo, we have again and again had occasion to perceive that the truths of science are even more impressive to the true poet than the more direct and obvious teachings of nature. Certainly if we consider how largely the universe, as disclosed under the scrutiny of science, surpasses in splendour the aspect of nature as seen by the ordinary eye, we cannot but feel that this should be the case; and that though the eye hath not seen, nor can ever see, the real glories of the universe, yet it may enter the heart of man to conceive something of the significance of those results which science is bringing to light.—R.P.

TELEGRAPHIC COMMUNICATION BETWEEN ENGLAND AND SCOTLAND.—In the House of Commons, recently, in reply to Mr. Buchanan, Mr. Fawcett said interruptions of telegraphic communication between England and Scotland have been very frequently under consideration. They have not occurred in any particular locality, but have been experienced in various parts of the country traversed by the several main lines of telegraph between London and Scotland. The placing of any one section of the lines underground would not, therefore, insure immunity from interruption, while the cost of substituting underground wires for overground throughout the United Kingdom, or even between the principal towns, would be very great, probably not less than a million and three quarters.

A WIRE FENCE TELEGRAPH.—On the Milwaukee and St. Paul Railway and the Brandon branch, experiments have been made of late to see whether or not the barbed wire fence on either side of the line could be used for telegraphing through. The wire was run under the surface at level crossings to make the line continuous, and trials were made which showed that telegraphing could be done through the fence. Mr. Simpson, superintendent of telegraphs on the railway, is, however, of opinion that in winter the snow wreaths covering the fence would prevent working, and thus render the line useless for every-day use; but Mr. Simpson appears to forget that dry snow insulates, and is probably not aware that in some parts of Norway the winter lines are laid on the surface of the snow. A thaw would perhaps prevent working; but even then the telephone might serve.—*Engineering.*

THE FACE OF THE SKY.

From March 28 to April 11.

By F.R.A.S.

THE usual watch will be kept on the Sun for spots, faculæ, &c. The Zodiacal light may still be seen during the earlier part of the next fortnight. The night sky is shown in Map IV. of "The Stars in their Seasons." Mercury, emerging from behind the Sun, is coming into a favourable position for the observer, and may be detected by the naked eye over the west-by-northern part of the horizon after sunset, about the time these notes terminate. Venus is now a most brilliant and conspicuous object in the evening sky, and even at the beginning of April does not set until 10h. 48m. p.m. By the 11th it is between 11 and 12 o'clock at night ere she disappears below the horizon. Mars is visible all night long in Cancer (Zodiacal Map, p. 70), but is gradually diminishing in apparent diameter. On the night of April 6th he will be less than $\frac{1}{2}°$ south of γ Cancri. Jupiter is also visible during the working part of the night, but being nearer the west than Mars, will be best placed for the observer between sunset and 9h. 30m. p.m. He is in the confines of Gemini and Cancer. (Zodiacal Map, p. 40.) The phenomena of his satellites, visible before 1 a.m. during the period covered by our notes, are both numerous and interesting. Beginning with to-morrow night (29th), the transit of Satellite II. will begin at 10h. 11m., and the shadow of Satellite IV. will enter on to the planet's disc at 11h. 56m. p.m. The shadow of Satellite II. will not commence its transit until 38 minutes after midnight. On March 31st, Satellite II. will reappear from eclipse at 10h. 31m. 26s. p.m.; and the transit of Satellite I. begin at 11h. 52m. On April 1, Satellite I. will be occulted at 9h. 1m.; to reappear from eclipse at 12h. 34m. 13s. p.m. On the 2nd, the ingress of the shadow of Satellite I. happens at 7h. 35m.; the satellite casting it passing off Jupiter's opposite limb at 8h. 39m., and being followed by the shadow at 9h. 55m. p.m. On the 3rd, Satellite III. passes off Jupiter's preceding limb at 8h. 50m. p.m.; the shadow which it casts actually not entering on to the opposite limb of the planet until 10h. 21m. On the 5th, the transit of Satellite II. commences 45m. after midnight. On the 6th, Satellite IV. will be occulted at 9h. 45m. p.m. On the 7th, Satellite II. will be occulted at 7h. 43m.; as will Satellite I. at 10h. 55m. p.m. on the 8th. On the 9th, the transit of this same Satellite begins at 8h. 13m. p.m.; followed by that of its shadow at 9h. 30m. The satellite leaves Jupiter's opposite limb at 10h. 33m.; the shadow at 11h. 50m. p.m. On the 10th, Satellite I. will reappear from eclipse at 8h. 58m. 36s. p.m.; and Satellite III. begins its transit at 9h. 12m, the egress of this last named satellite occurring at 12h. 46m. p.m. Saturn now souths in bright sunlight, but may be seen low down in the west to the north of ϵ Tauri ("The Stars in their Seasons," Map I.) as soon as it is dark enough. Uranus is now in Virgo, in an excellent position for the observer. On the night of April 1st he will be only 8′ north, and just to the west of β Virginis ("The Stars in their Seasons," Map V.). Neptune is invisible. The moon's age at noon to-day is 1·3 days; and, pretty obviously, it will be 15·3 days at the same hour on April 11th. No occultations will be visible during the succeeding fourteen days. At noon to-day the moon will be in Pisces; but at 3 o'clock in the afternoon will pass into that little corner of Cetus intruding between parts of Pisces and Aries, which latter constellation she will enter at 11 o'clock to-night. She will not quit Aries for Taurus until 6 a.m. on the 30th. Travelling all next day through Taurus, she will, at 4 p.m. on April 1, enter the extreme northern part of Orion, which she will traverse in, as nearly as may be, twelve hours, emerging in Gemini at 4 o'clock on the morning of April 2. Between 7 and 8 p.m. on the 3rd she crosses the boundary of Gemini into Cancer, which constellation she leaves for Leo at 11 a.m. on the 5th. At noon on the 8th she descends into Sextans, across which she travels until, at 5 o'clock in the morning of the 7th, she re-emerges in Leo. She occupies until 7 a.m. on the 8th in crossing Leo, and at the hour just named enters Virgo. Her journey across this great constellation takes her until 5 p.m. on April 11 to accomplish, and she then crosses the boundary into Libra. We there leave her.

The first enterprising Londoner who introduced conduit water to his premises was—the *Builder* says—a tradesman of Fleet-street. In a record of 1478 it is mentioned that "a wex-chandler in Flete-strete had by crafte perced a pipe of the condit withynne the ground, and so conveied the water into his selar; wherefore he was judged to ride through the citie with a condit uppon his hedde," and the City Crier was to walk before him proclaiming his offence.

"Let Knowledge grow from more to more."—Alfred Tennyson.

Only a small proportion of Letters received can possibly be inserted. Correspondents must not be offended, therefore, should their letters not appear.

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DAY GLOW AROUND SUN.

[1158]—The above effect does not appear to have attracted the notice of your astronomical correspondents. We seem here to have lost the wonderful "afterglow" and "early glow," but on several occasions the sun has had surrounding him—some 15° each way from him—a very beautiful salmon-red ring of soft, diffused light.

I find on blocking or blotting the sun out with any opaque object held at arm's length, or, better, when he is hid behind a building or between cumulus cloud, the circle and contrast with the blue sky outside and the silvery light inside it form a very beautiful effect.

Has this any connection with recent displays—the "dust-cloud" theory, &c.? or, may the cloud, of whatever matter it may consist, be departing—condensing, and thus be only visible in the sun's immediate vicinity? Thos. Radmore.

142, North-road, Plymouth, *March* 21, 1884.

THE FRETFUL PORCUPINE—KRAKATOA AND THE COFFEE PLANT.

[1159]—One of your correspondents writing about porcupines a short time ago, referred to the popular notion of the manner in which these animals used to be supposed to discharge their quills at an adversary in self-defence. I have seen porcupines hunted many a time, in India, by dogs, and noticed a curious fact, which I fancy is not at all known. Dogs, of course, get the quills in their necks and faces in attempting to seize the porcupine, but the latter has a very curious trick of suddenly charging backwards when a man or dog comes close behind it, so that the points of the quills act most effectively as offensive weapons. I remember a tame porcupine which was chained up to a kennel, and it often astonished people who approached within reach—especially coolies with bare legs—by this trick.

Referring to the article at p. 80, there is a curious circumstance bearing on the question of the dust being volcanic, which I believe has not been made known yet. Some of your readers may, perhaps, have heard of the extensive damage done to coffee in Ceylon and South India during the last half-dozen years by a fungus. For this disease the only remedy has been thought to be the application of sulphur, but to be effective this must be universal, and it was impossible to arrange that the cure should be applied to a whole district simultaneously. Now from a private letter from India, I hear that after the eruption in Java last year, the peculiar colour of the atmosphere and the greenish appearance of the sun were very marked for some months, and at the same time a decided improvement in the coffee was noticed. As to the disease, could this have been a universal application of sulphur vapour?

There was, by-the-by, a considerable glow in the sky last evening after sunset, and this morning (February 11), we have had a heavy hailstorm. I carefully collected some of the hailstones, and, after they melted, I examined the result in the microscope. They contained a considerable quantity of green plant-cells and spores of fungi; a few diatoms, one or two fragments of spiral vessels of plants, a few particles apparently mineral, and finally, what I take to be several specimens of either the egg of rotifera, or else the young rotifers in the oval form in which they are sometimes seen.

F.R.C.S.

INFINITY.

[1160]—Many persons appear not to understand that it is impossible for a *finite* being to comprehend infinity; and hence, I imagine, has arisen the "Edinburgh Reviewer's" attack on Mr. Spencer. There is no "continuity" between the finite and the infinite. The former cannot be either a multiple or sub-multiple of the latter. Thus, it is no wonder that "we cannot by searching find out God," when, even as regards "space," we find ourselves baffled. I had a talk once with the late Professor Clifford on the subject of infinitely small and infinitely great space. On my saying I *could* form a conception of the former, I replied to his question that "a *point* was infinitely small space"—it existed in space, but did not fill any space (being without extension), and was, therefore, in fact, "zero space." He would, therefore, I concluded, admit that there might be an infinite number of points between any two points, and yet he could not assert that there was *more* than an infinite number of points in the whole universe. These contradictory admissions clearly prove OUR utter incapability of understanding "infinity" even as regarded space. H.

[Yes: yet strangely enough men find more comfort in talking about infinity as if they understood it than in accepting the finite which can alone be comprehended.—R. P.]

THE MORALITY OF HAPPINESS.

[1161]—If any proof of the truth of your remark that "there can be no manner of doubt that rules of conduct are regarded by an immense number of persons as essentially associated with religious doctrines," were needed, it may be found in the fact that many people will but half-heartedly admit that a man may be capable of good conduct if he does not profess their own peculiar creed, but will stoutly deny that such conduct is impossible to him who professes no creed at all. The reason for this position is, I think, not far to seek. That conduct conduces to happiness is, perhaps, more conclusively insisted upon in the Bible than in any other book that is equally read; and those who regard the Bible as the inspired fount of their theology, cannot admit that a man may by his life prove this and yet not give his adhesion to their own or some kindred doctrine which they insist is built upon Biblical teaching. But that this proposition—"Conduct conduces to happiness"—is true, most people, indeed, I should say, all people, may prove to themselves by a little thoughtful introspection. Who without any reference whatever to religious sentiments, has not felt the pangs of remorse, when suffering from a sense of wrong-doing? Who has not felt a thrill of the most real and satisfying pleasure when, by the exercise of self-denial, he has conferred some benefit on a fellow-creature—thus receiving from his own conscience the direct assurance that the proposition is true? Yet conscience existed before the Bible, and before the Bible must have been susceptible of the same emotions that influence it now. It so happened that the Jews made the discovery some centuries ago, that "conduct conduces to happiness," and insisted upon it in their literature; and it further happened that upon Jewish literature the whole fabric of Christian theology was built up: but the truth and proof of the proposition is a matter of purely worldly wisdom, the outcome of experience, and has nothing whatever to do with theological dogmas. A. McD.

THE SENSES IN INFANTS.

[1162]—Having only just seen the series of four remarkable and highly interesting articles by "A. M. H. B." on Infant Psychology, in KNOWLEDGE, and accepting the invitation generally given to mothers to observe the development of their children, I should be glad to give a few facts connected with this subject which have come under my observation.

As regards the sense of smell, I noticed that my little girl was very susceptible, about the age of nine months, to the smell of oranges, frequently given her to play with, and would always sniff them before commencing to throw them about. When she was one year and four months, she was shown a pig-stye with pigs, and at once detected the bad smell; and always after, when spoken to about pigs, she would wrinkle her nose and make a wry face to show that a bad smell was connected with them. She early associated smell with flowers, but would smell a painted flower in the same way as a real one.

As regards the sense of colour, I found that the colour she noticed first was yellow, and at the age of three months she would always be appeased when crying if I took her to look at some large yellow brass plates which hung on the wall. She also, at the same age, particularly noticed a yellow silk handkerchief I used to wear, though taking no notice of a blue one.

The appreciation for music developed early. Before the age of three months she would be quite quiet when crying if I sang Tyrolese Jödlers to her (these songs have very high notes), and she would screw up her mouth and attempt to sing with me by making sounds like gu-gu (a faint imitation of the sound of a Jödler). At the age of eleven months she often pitched on the right note in attempting to sing with me. This delight in bright music with high notes continued for some time; but when, at the age of one year and three months, I sang a low song in the minor key she became very uneasy and began to cry. A few hours before I had sung quick, high music, and she laughed and was delighted with it. The piano, however, whether accompanied with or without a voice, always made her cry up to the age of one year and three months. Since then she has learnt to tolerate it, and within the last five months, if put near it, likes to play the piano. The value of A. M. H. B.'s articles is very great, and the readers of KNOWLEDGE ought to be much indebted to it for publishing such interesting, and yet popular, science, for they not only teach mothers how to understand and what to observe in their children, but are also a contribution to the early history of the race. A MOTHER.

EFFECT PRODUCED BY GROWING A COLOURED HYACINTH IN DARKNESS.

[1163]—In October, 1882, I placed a bulb of Hyacinth in a pot of mould and allowed it to remain in the light until it showed signs of sprouting. It was then removed to a photographic tent, erected in a room. Light was perfectly excluded, but there was complete ventilation. In March, 1883, the bulb bore a stem of dark purple flowers. The leaves were *totally devoid of colour*. The main flower-stem and footstalk of the flowers were also totally colourless. The intensity and brilliancy of the colour in the flower could not, I think, have been exceeded, had they grown in daylight. The same bulb has flowered again this year (planted in Oct., 1883,) having been grown in the light. The cluster of flowers is not so large as that of last year, and the colour is not quite so deep.

JOHN STONE.

VEGETABLE DIET.

[1164]—Recently Mr. T. R. Allanson, in a paper entitled "Cheap and Good Food," broached a subject which, while being of the first importance to all mankind, is, owing to the peculiarities of their position, year by year forcing itself, with special and increased acuteness, on the attention of the rapidly-growing populations of these islands.

Mr. Allanson strongly recommends vegetarianism as the remedy for dear food, as well as for certain other evils. In support of his views he makes certain statements of facts. Presuming on the correctness of those statements, the question occurs, do they necessarily or conclusively support his views?

If it is granted that certain vegetables contain more nutriment, weight for weight, than meat does, is the problem, then, any nearer to its solution? Which is of the chief consideration, the amount of nutriment contained in a food, or the amount that may be extracted from it? Do we always make use of the whole quantity contained in any kind? Do the digestive organs of necessity extract the most nutriment, or extract it with the greatest ease, from a food which contains an exceptional amount? From reasons similar to those which render it necessary to dilute spirit with water, may not, in some cases, the addition of a quantity of non-nutritious matter be in reality a great advantage? Also, letting alone the question of proportional quantity, may not the non-nutritious matter, in some cases, assist the digestion of that which is nutritious?

In fact, is it not quite possible for a table, showing the percentages of nutriment, to be a completely fallacious guide as to the choice of food? Might not a food, ranking low in such scale, in reality supply more nutriment than some other food, ranking exceptionally high, would do?

Again, is a man bound to be either healthier or stronger the more he weighs? Is it absolutely demonstrated that the diseases mentioned by Mr. Allanson are caused or increased by the usual mixed flesh and vegetable diet, or that vegetarianism would not favour other diseases equally dangerous? Are the inhabitants of vegetarian countries healthier, physically stronger, or mentally superior to the inhabitants of those countries where meat forms a part of the diet?

Finally, as a matter of common experience, is not a man very likely to really feel better and stronger at a time when he is quite convinced that he is so? J. BINDON CARTER.

STRANGE FISH.

[1165]—Anent your correspondent Mr. John Hann's letter in a recent number of KNOWLEDGE, on "Strange Resuscitation" of a fish, perhaps the accompanying extract from *Daily News* of 21st ult.

may help; although the resuscitation of the frozen fish mentioned in it is by warm water, and not by being struck:

"An astounding fish—we had almost written 'fishy'—story comes from the Dominion of Canada. We have read of singing fish and of fish, not to speak of oysters, which climb up a tree and and go to roost like birds, and of the salmon which follows his master like a dog, but verily we have not heard or seen them. The majority of these stories come from the East, but the great West is not without ichthyological marvels. A winter fishery has recently been established at Birch Lake, 150 miles north of the Canada Pacific Railroad. The fishermen 'claim' to have caught with hook and line during the last month 250,000 pickerel, which have been distributed through the Middle and Eastern States of the Union, many of them getting as far as New York. These pickerel are said to be of finer flavour than those of southern waters. They are taken from the water alive and frozen in a few minutes. '*It is also said' that some of them will come to life if placed in a tank of cold water for a few hours to 'draw the frost out of them gradually*.' Now, if these animals had been snakes we should have hesitated to repeat the story. The serpent is not only the arch liar himself, but the cause of lies in others. To say that a tale is snaky is to hint a doubt at once. But the pickerel is not a reptile, but an honest fish, and of pleasant association, for is it not written that 'Hops and turkeys, pickerel and beer, Came into England all in one year?' Wherefore we merely echo the remark made by the Irish gentleman when a process-server crawled out of a pond into which he had been hurled after being beaten nearly to death with sticks, 'The tenacity of life among the lower animals is wonderful!'"

Medicus.

[Before ridiculing the account, the *Daily News* writer might with advantage have studied what Dr. B. W. Richardson wrote on the subject of suspended animation a few years ago.—R. P.]

GHOSTLY VISITANTS.

[1166]—About four or five years ago, Mr. E. T. Smith, a well-known London impresario, died suddenly at his house at Kennington.

Knowing the old gentleman well, I called to condole with his young widow the second evening after his death. I found her and the two children and the maid servant in a very hysterical condition. They solemnly affirmed that, about an hour before, the corpse had knocked at the street door. The servant opened it, and instantly fainted. The widow heard her dead husband's voice most distinctly; so did the two children and a lady visitor, an old friend. The whole party were in a terrible state of nervous fright—quite unmistakable as to its reality. It appeared that the ghost had knocked, shown himself to the servant, said something, then vanished. All agreed on these points. I went upstairs alone, no one volunteering to accompany me. As I, of course, knew would be the case, there was the coffin with its contents intact. Evidently nothing had been disturbed since my old friend had been comfortably prepared for his long sleep.

I reported this fact to the family (the visitor had gone home), but this made no difference in their belief. The corpse certainly *had* knocked at the hall-door. The evidence of the wife, two children, the friend, and the servant, was not to be argued down by any theory that I could suggest, and I began at last almost to believe the story myself, so certain were they one and all.

As a ghost story, it was quite complete. No evidence could be possibly more truly given, or by more trustworthy persons. None of them were at all of a dreamy, imaginative, poetical temperament. On the contrary, every one was about as commonplace as a Cheshire cheese. Nevertheless all were genuinely in a very excited state. I suggested fetching their medical man, and was about going for him myself when there came a tap, tap, at the front door, which was recognised at once by Mrs. Smith as her late husband's old familiar knock. The maid came rushing into the room saying, "Oh, mum, there he is again! Whatever is to be done? I am sure I can't go and face it." Just then another solemn double tap was echoing through the house, which I undertook to answer.

It was about six o'clock on a misty autumnal evening, so, of course, quite dusk.

I opened the door, and sure enough there stood the dead man on the step, dressed about the same as I had seen him for years.

I didn't faint, but candidly admit that I wanted some brandy badly.

"Walk in, Mr. Smith," said I at last, fully prepared to believe in solid ghosts as long as I lived.

"Oh! I may come in now, may I?" said Mr. Smith. "When I was here last night you all seemed out of your senses, so I sloped. Pray, when do you bury my brother?"

This simple question at once explained the mystery.

The ghost was a twin brother, who had been out of England for some years. He was totally unknown to the widow, who, in fact, had never heard of his existence, the brothers having quarrelled and parted a long time ago. Having seen a notice of the death in the *Times*, he had resolved to attend the funeral as chief mourner. This he did, and every one was struck by his likeness to his late brother, both in voice and person.

Now, if the old gentleman (nearly so) had taken offence at what he might have considered the scurvy treatment received on his first visit, "sloped," as he called it, and never shown himself any more, what a perfect case of supernatural appearance would have been ready for record in the next edition of Mr. Crowe's "Night Side of Nature," or Mr. Newton Crosland's "Queer Spirit Book."

No doubt every unexplained ghost story has a twin brother in it if you could only find him.

J. H. Cobbett.

GHOSTLY GARMENTS.

[1167]—The doubt which troubles you [Scarcely!—R. P.] as to ghostly vestments is one which apparently occurs to Spiritualists themselves. I find, in a report of a conference of Spiritualists, taken from the *Standard* of Aug. 7, 1874, that "Mr. Harrison stated that the materialised drapery of spirits was sometimes of a fabric not easily procurable in this country." "Mr. Rogers remarked that a piece of cloth cut off by a female spirit from her materialised skirt was found to have been dressed with lime in the Manchester fashion, and he admitted that this presented a difficulty to Spiritualists which had not yet been surmounted."

Surely these acute reasoners must since then have attained the solution of the question.

L. M.

COINCIDENCE.

[1168]—The other day a party of about eight persons were discussing the merits of the late Mr. T. Todhunter, some praising him and others criticising him on his mathematical works; what brought Mr. Todhunter's name into the conversation no one knows; but it is a fact that when he was spoken of they all joined in the conversation that ensued. Nothing more happened; but a few days afterwards they heard that Mr. Todhunter had died about the time that they were talking of him. This is a striking coincidence, but it may be more than a coincidence—who knows?

C. T. S.

LETTERS RECEIVED AND SHORT ANSWERS.

E. Keelan.—J. Nilson.—No reward has been offered for squaring the circle. There would be no value in the solution of the problem, even were it possible—J. Carey. Yes, if the well is deep enough, the opening small, and the star one of the brighter sort.—J. N. A. wishes to know the name of a good work on dyeing applied to leather, or, failing that, of a work on dyeing applied to cotton, silk, &c.—Newton Crosland. There is *no* opposing force. That is where you have been wrong from the outset—thanks to Joyce & Co. Hence no *tertium quid* is wanted. If your difficulties are to be fought out to the death, it must be with some other opponent.—A Beginner. (1) A string carried round from any point on the Arctic circle to the opposite point on the Antarctic circle (180° in longitude east or west of the former) and so back to the starting point, will give the great circle required. But it is better to use the wooden horizon circle. (2) You do not say what radius the cylinder is to have; if the radius is greater than that of the sphere there will be no intersection. If less, the intersection will lie on two equal small circles having the intersection of the axis of cylinder with the sphere as poles.—Birkenbaum. Fear we have no space for the suggested subject. Thanks, however.—Student. Do not know where that method is given or established: what text-book is supposed to be used by those examined?—Wm. Poole. The sun's light and heat do not increase the earth's substance. Science does not recognise combustion as going on in the sun. Heat results rather from mechanical causes.—Ed. Woolley. I judge by what you tell me of Mr. H. Grattan Guinness that he knows nothing of astronomy. As to astrology, you are *bound* to meet with all manner of singular experiences, if only enough predictions are made, and you sift out those which are not fulfilled. I can predict—given enough trials—the winner of this year's Derby to a dead certainty. Yet I am no prophet.—T. Radmore. Inserted. Thanks.—W. J. Paul. Comet long since gone. That is Venus.—Tyro. Electricity might be obtained by utilising the heat of water applied to the junction of two metals, but the current produced by a temperature of 160° Fahr.

at one junction, and say 60° Fahr. at the other, would be too small to be commercially successful.—W. H. MOTTON. No wire or addressed envelope accompanies your request. Will, however, forward you an ohm length.—R. ELMORE. Many thanks. Will certainly take great interest in examining your picture.—W. GREATHEED. Many thanks; but the lines are not quite suitable to our columns.

Our Paradox Column.

[We have been requested by the former secretary of the Zetetic Society to insert a series of papers in which he promises to show the nature of the deceptions practised by some at least among the advocates of the flat-earth theory.—ED.]

THE FLAT EARTH AND HER MOULDER.

BY H. OSSIPOFF WOOFSON.

I.

THERE is always a deep meaning concealed behind common sayings; and though generally of but few words, they often embrace a whole philosophy. By themselves they may express one thing, but if followed by another sentence, they will often embrace a whole moral teaching which, when applied to experience, strikingly illustrate facts of indisputable value.

My sojourn in England has been but of short duration, yet I have managed to learn much; and in the present case I have strictly, though unconsciously followed the advice of the Scotchman who maintained that "Honesty was the best policy" (in the negative, it would mean that dishonesty was the worst policy). Questioned as to how he knew it, his answer was "'Cause I've tried 'em both." As a matter of fact the adage and saying are bad from a moral point of view. Honesty should be a virtue, and not a necessity. But if we apply the whole to matters of investigation and of a speculative nature a better saying could scarcely be found. For it is by all means essential to *try both* sides of a question before we can say which is the right one. This I was led—quite inadvertently, I confess—to do, and it enabled me to investigate a teaching, which, for its audacity and ingenuity, hardly has an equal in the whole array of paradoxies and scientific frauds. Nay, not only was I led to investigate, but I was brought to regard it for a time as an accomplished fact, as a truth, persecuted and opposed by unscrupulous philosophers, whose object in resisting it was to postpone that punishment at the hands of universal judgment which would surely overtake them sooner or later.

Every falsehood must have a semblance of truth in it, and some plausibility, otherwise it could not gain converts; and as long as you believe the expounder of it to be sincere, you are sure to credit his statements. Simple-minded, credulous, inexperienced people do not, as a rule, require great inducement. For them a small amount of ingenuity and plausibility is sufficient to gain their fancy and to excite their imagination. To gain the credulity of people of higher culture the expounder of a pseudo theory must of necessity possess in proportion greater skill. Such hypocrites, we are bound to admit, must possess no ordinary perception of human failings and a vast psychological discernment.

There are *quacks* for all classes, and to suit all tastes. There is the street quack, who makes a good living by the sale of water and bread pills, supposed to effect marvellous cures; there are the impostors who amass fortunes by their self-denying generosity to benefit humanity by means of a bottle of stuff, for which they condescendingly accept a goodly fee. There are traps for everybody, the highest and the lowest.

Experience and good lessons are, however, essential elements for our future guidance, and, if these are obtained in our early lives, they really recompensate us in the long run for our mistakes and light-mindedness.

Now circumstances have led me to embrace the so-called Zetetic Philosophy, and, although far from being very much elated at the short-lived honour, I yet feel a kind of recompensation by the fact that my experience may be of service to others. Mr. Proctor, to whom my thanks are due for his generous offer to allow space in his valuable journal for my account of the Zetetic Philosophy, has, indeed, not missed an opportunity to expose its falsehood; but he could have been scarcely expected to strike at the root of it for want of such information which I was enabled to collect.

I propose giving a detailed history, in short weekly articles, of the whole of what I shall allow myself to call the humbug catchpenny philosophy, concocted by a man who styles himself Parallax, otherwise Rowbotham, alias Tryon, Goulden, Dr. Birley, &c.

Our Mathematical Column.

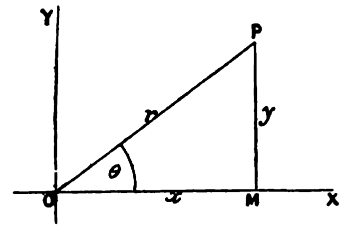
EASY LESSONS IN CO-ORDINATE GEOMETRY.

BY RICHARD A. PROCTOR.

(*Continued from p.* 173.)

TRANSFORMING CO-ORDINATES.

7. When the rectilinear co-ordinates of a point are given we can readily determine its polar co-ordinates, and *vice versâ*.



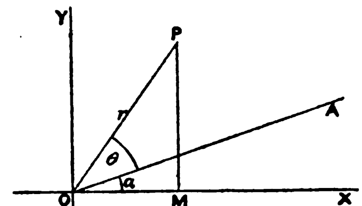
(i) Let the initial line coincide with O X, the axis of x, and the pole with O, the origin: Let P be a point in the plane; x, y, its rectilinear, and r, α, its polar co-ordinates. Join O P and draw P M perpendicular to O X; then, evidently

$$x = r\cos\theta, \quad \text{and } y = r\sin\theta$$

also $$r = \sqrt{x^2+y^2}, \text{ and } \tan\theta = \frac{y}{x}$$

Thus the rectilinear are expressed in terms of the polar co-ordinates, and *vice versâ*.

(ii) The pole O still coinciding with O, let the initial line O A,



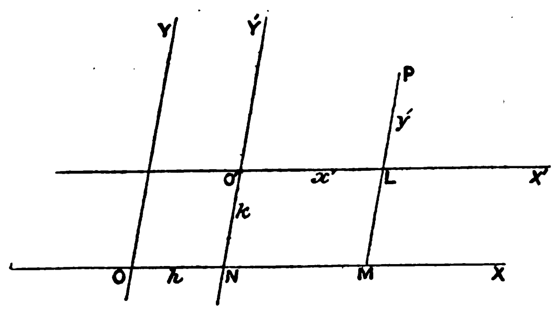
make with O X an angle α; then with the same construction, we have evidently

$$x = r\cos(\theta+\alpha), \text{ and } y = r\sin(\theta+\alpha)$$

also $$r = \sqrt{x^2+y^2}, \quad \text{and } \tan(\theta+\alpha) = \frac{y}{x}$$

the required relations.

8. If the rectilinear co-ordinates of a point are given, the axes being oblique or rectangular, and, through a new origin, axes



parallel to the first are drawn, we can connect the co-ordinates of the point referred to the new axes, with the original co-ordinates.

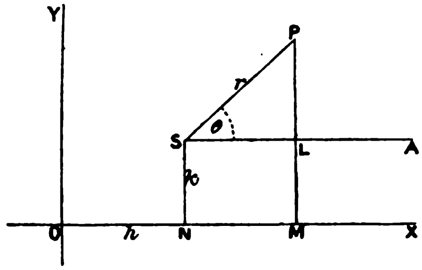
Let the co-ordinates of P referred to the original axes O X, and O Y be x, y. Through the new origin O′ draw the axes O X′ and O Y′ parallel respectively to O X and O Y. Let the co-ordinates of O′ referred to O X and O Y be h, k; and the co-ordinates of P referred to O X′ and O Y′, x', y'. Draw P M parallel to O Y meeting O′ X′ in L and O X in M, and let O′ Y′ meet O X in N; then

$$x = OM = ON + NM = ON + O'L = h + x'$$
$$\text{and } y = MP = ML + LP = O'N + LP = k + y'$$

the required relations.

9. If the rectilinear co-ordinates of a point are given, the axes being rectangular, and through a given point a straight line is

drawn parallel to the axis of x, we can connect the co-ordinates of the point referred to the given point as pole and the line drawn through that point as initial line, with the original co-ordinates.



Let the co-ordinates of P referred to the rectangular axes O X, O Y be $x\ y$. Through a point S whose co-ordinates are h, k, draw S A parallel to O X. Let the co-ordinates of P referred to S A as initial line and S as pole be r, θ. Draw S N and P M perpendicular to O X, and let P M meet S A in L; then

$$x = \mathrm{OM} = \mathrm{ON} + \mathrm{NM} = \mathrm{ON} + \mathrm{SL} = h + r\cos\theta$$

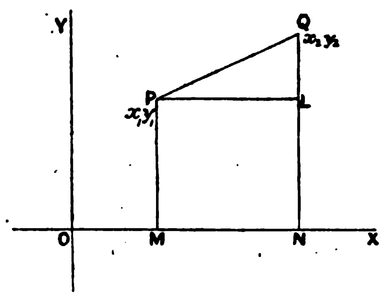
$$\text{and } \mathrm{Y} = \mathrm{MP} = \mathrm{ML} + \mathrm{LP} = \mathrm{SN} + \mathrm{LP} = k + r\sin\theta$$

the required relations.

Arts. 7, 8, 9 are instances of what is called transformation of co-ordinates. We shall return to the consideration of transformation of co-ordinates later on. For our present purposes the foregoing articles will prove more than sufficient. We may remark that they enable us to transform from one set of rectangular axes to *any* other in the same plane. For Article 9 enables us to proceed from rectangular axes to a polar system having *any* pole, but the initial line parallel to O X; and Article 8 enables us to proceed from this system to *any* rectangular axes having the pole of that system for origin.

DISTANCE BETWEEN TWO POINTS.

10. *To express the distance between two points, in terms of their rectilinear co-ordinates.*



Let P and Q be the two points; x_1, y_1 the co-ordinates of P, and x_2, y_2 those of Q. Draw P M, and Q N parallel to O Y to meet O X, and P L parallel to O X to meet Q N. Let P Q = d. Then, by Euc. I., 47;

$$\mathrm{PQ}^2 = \mathrm{PL}^2 + \mathrm{LQ}^2.$$

But P Q = d, P L = $x_2 - x_1$, and Q L = $y_2 - y_1$; therefore

$$d^2 = (x_2 - x_1)^2 + (y_2 - y_1)^2.$$

If P is at the origin, so that $x_1 = 0$, and $y_1 = 0$

$$d^2 = x_2^2 + y_2^2$$

We have supposed the axes rectangular; if they are oblique, and the angle Y O X = ω, we get, from the triangle P L Q

$$\mathrm{PQ}^2 = \mathrm{PL}^2 + \mathrm{LQ}^2 - 2\,\mathrm{PL}\,.\,\mathrm{LQ}\cos\mathrm{PLQ}$$

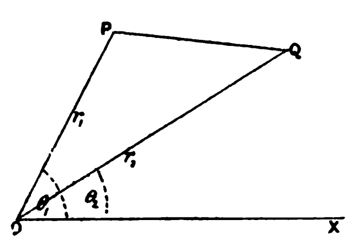
that is, since $\angle$ P L Q = 180° − ω

$$d^2 = (x_2 - x_1)^2 + (y_2 - y_1)^2 + 2\,(x_2 - x_1)\,(y_2 - y_1)\cos\omega$$

and if P is at the origin

$$d^2 = x_2^2 + y_2^2 + 2\,x_2 y_2 \cos\omega.$$

11. *To express the distance between two points in terms of their polar co-ordinates.*



Let P and Q be the two points; r_1, θ_1, the co-ordinates of P, and r_2, θ_2, those of Q. Join P Q, and let P Q = d; then,

$$\mathrm{PQ}^2 = \mathrm{PO}^2 + \mathrm{OQ}^2 - 2\,\mathrm{PO}\,.\,\mathrm{OQ}\cos\mathrm{POQ}$$

that is

$$d^2 = r_1^2 + r_2^2 - 2r_1 r_2 \cos(\theta_1 - \theta_2)$$

We might have deduced this result from Art. 10 as follows:—

The rectilinear co-ordinates of the points P and Q referred to O X as axis of x, and a perpendicular to O X through O as axis of y are respectively $r_1\cos\theta_1$, $r_1\sin\theta_1$, and $r_2\cos\theta_2$, $r_2\sin\theta_2$, Art. 7. Hence by Art. 10

$$d^2 = (r_1\cos\theta_1 - r_2\cos\theta_2)^2 + (r_1\sin\theta_1 - r_2\sin\theta_2)^2$$
$$= r_1^2 + r_2^2 - 2r_1 r_2\cos(\theta_1 - \theta_2)$$

(*To be continued.*)

EASY RIDERS ON EUCLID'S FIRST BOOK,

WITH OCCASIONAL SUGGESTIONS.

PROP. 8.

23. The diagonals of a rhombus intersect each other at right angles.

24. A quadrilateral has two of its opposite sides equal, and its diagonals are also equal. Show that the diagonals divide the quadrilateral into four triangles whereof two are isosceles and the other two equal to each other in all respects.

25. From every point of a given line the lines drawn to each of two given points on opposite sides of the line are equal. Prove that the line joining the given points will be bisected by the given line at right angles.

26. Show how Prop. 8 may be established without the use of Prop. 7, by applying the base of one triangle to the base of the other, the equal sides being conterminous but the vertices lying on opposite sides of the base.

Join the vertices; the rest is obvious.

PROP. 9.

27. If the base angles of an isosceles triangle be bisected, and the point of intersection of the bisectors joined to the vertex of the triangle, show that the vertical angle is bisected by the line thus drawn.

PROP. 10.

28. Show how to bisect a given straight line without making use of any proposition beyond the Sixth.

See fourth rider to Prop. 6.

PROP. 11.

29. Show how to draw a straight line at right angles to a given straight line from a given point in the same without making use of any proposition beyond the Sixth.

30. Find a point in a given line such that its distances from two given points may be equal.

31. Describe a circle of given radius to pass through two given points.

(*To be continued*).

THE sixth edition of Mr. Thomas Smith's handy manual entitled "Successful Advertising" has just reached us. It is neatly got-up, and concisely presents a great deal of real information, not only to inexperienced advertisers, but also to those who, while knowing the "tricks of the trade" and being well able to take care of their own interests, yet have not time to elicit such information for themselves. As Macaulay has truly observed, "Advertising is to business what steam is to commerce—the grand propelling power"; and those who desire to turn that power to the best account are recommended to peruse the little book in question.

THE Director of Public Gardens in Jamaica reports the existence in St. Helena of large quantities of black oxide of manganese, or pyrosulite, samples of which have been analysed by Professor Roscoe, with the result that one sample of St. Helena manganese, soft, found in clay beds, yielded 35·41 per cent. of manganese di-oxide; while a second sample, hard, found in clinker, yielded as much as 63·19 per cent. of manganese di-oxide. This recalls the fact that large quantities of this material exist in Jamaica, samples of which, analysed by Dr. Lewis Hoffmann for the Geological Survey of Jamaica, show 88·89, or practically 90 per cent. of manganese di-oxide.

Our Chess Column.

BY MEPHISTO.

PROBLEM No. 115.

IT is very gratifying for us to find that this Problem has given general satisfaction. The competition has been a very keen one. An unforeseen difficulty arose in connection with it: we received some solutions dated London, the 20th, but also some from the country that must have been posted a few hours later only. Giving our country cousins an allowance of time and the benefit of the doubt, we have decided to award the Prize to MISS ASHWORTH, of Brighton. We shall give the solution next week.

Correct solutions received from:—C. H. Brockelbank, W., S. B. B., E. Louden, S. Lawther, H. B. J., E. Stevens, Edward Carter, Wm. Fazan, J. A. Miles, A. W. Overton, E. Waller, J. E. Hull, E. Hamburger, W. Powell, J. Alfred Hill, H. Humphrys, W. Ratcliff, G. Woodcock, Berrow, Fell, L. F., Thos. Carroll, F. L. d'Anson, H. H. Shanks, J. H. Meredith, Edward Ridgway, Uncle John, W. H. S. Monck, A. Rutherford, A. W. Orr, A. McD., A. Smith, H. A. L. S., E. C. H., John Watson, A. Schmucke, W. Kemsley, A. J. Argles, F. J. D., G. Robson, R. McKay, Watkins Calvert.

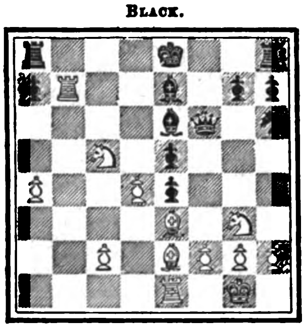
Incorrect solutions:—Edwin Butler, M. A., J. P. Harrap, W. H. Percy, T. D., R. Dutton, A. M. Holland, K. W., K. W. Johnson, Dr. Highett, J. W. Ellitt, A. Mooney, Clarence Becker, H. H. Shanks, Senex, Thomas K. Bentley, Thos. P. Burton, Bonnward, Geo. Smith, W. J. Peek, The Owl (M.D), J. Winfield, A. S. Orr, J. Griffith, Lawrence Small, Dr. Healy, J. H.

We also thank our numerous correspondents for the good wishes expressed towards us.

A SINGULAR ENDING.

THE following position occurred a few days since in a game in which Mr. Proctor had given the Queen to a young amateur. It was White's move, and certainly the position at a first view does

BLACK.



WHITE.

not suggest even the possibility of so rapid a clearing off as actually followed. The game proceeded thus:—

White. Mr. Proctor.	Black. Young Amateur.	White. Mr. Proctor.	Black. Young Amateur.
1. R tks B (ch)	Q takes R (a)	7. R takes Q	R to QB sq
2. B takes Kt	P takes B	8. R to Q4 (ch) (e)	K to QB2
3. Kt takes B	Q takes Kt	9. R to QB4 (ch)	K to Q sq
4. P takes P	Q takes P (b)	10. R takes R	K takes R
5. Kt tks P (c)	Q takes Kt (d)	11. P to Kt3	
6. B to Kt5 (ch)	K to Q sq		

And White won easily.

(a) White's move was unsound. K takes B was the better reply, but the move in the text was safe enough.

(b) White's fourth move was unsound, but played with the assurance that Black would snap at the Pawn.

(c) Up to this point it has been a case of *nothing venture nothing win*.

(d) The proffer of Knight for Pawn should have shown there was a trap. Black's game was still perfectly safe.

(e) Not a piece is now left on the King's file, which at starting had not shown a vacant square!

THE following interesting game was played some time ago at Purssell's Rooms. It may be said to demonstrate the unreliable nature of the Scotch Gambit attack. We say this without any intention of depreciating the opening, but so much is sure that with average play the first player will often find that he has not retained a perfect command over his forces, although he has apparently an attacking position. This is particularly the case where P to KB4 has been played, which will often result in White, after having Castled, having his B on K3 and Kt on Q4, blocked in an inconvenient manner by the Black B on QB4:—

SCOTCH GAMBIT.

White. Mr. E. N. F.	Black. Mephisto.	White. Mr. E. N. F.	Black. Mephisto.
1. P to K4	P to K4	18. Kt to B3	P × P
2. Kt to KB3	Kt to QB3	19. Kt × P	P to R5
3. P to Q4	P × P	20. P to Kt5 (c)	Kt to B4
4. Kt × P	B to B4	21. B to KB4	Kt to Kt6 (ch) (d)
5. B to K3	Q to B3	22. K to Kt sq	Kt × Kt
6. P to QB3	KKt to K2	23. P × Kt	Q × P
7. B to QB4	Q to Kt3	24. P to Kt6 (e)	P to R3
8. Castles	Kt to K4 (a)	25. P × P	K × P
9. B to Q3	P to Q3 (b)	26. R to B sq (ch)	K to Kt sq
10. P to B3	B to KR6	27. B × RP (f)	B to B3 (g)
11. R to B2	Castles QR	28. Q to Kt3	KR to B sq (h)
12. K to R sq	B to Q2	29. B × KtP	B × B
13. B to KB sq	P to KR4	30. P to R6	P to Kt4 (?)
14. P to QKt4	B to Kt3	31. R to K2 (i)	Q × P (ch) (j)
15. P to QR4	P to KB4	32. B to K3	Q to Q4
16. P to R5	B × Kt	33. Q to Kt5	Resigns
17. P × B	Kt to B2		

NOTES.

(a) Q × P would be dangerous.

(b) Not so good as P to Q4, which would result in 10. P to B3, Kt × B. 11. Q × Kt, P × P, and White would remain with an isolated Pawn.

(c) So far the game has resulted in a slight advantage in Black's position, who disregards any possible danger from the advance of White's Pawns.

(d) This wins a Pawn, as White dare not take the Kt, for after P × Kt, P × P, discovering check; for that reason Kt or B × Kt would also not hold good, as after the Black Pawn had taken, the White RP would remain pinned by the Black Rook.

(e) A very attacking move. From this point White rapidly recovers lost ground by ingenious play. We give a diagram of the position.

BLACK.

WHITE.

Position after White's 24th move.

(f) Very well played.

(g) It would seem better play to capture the B, when the following might have resulted:—27. P × B. 28. Q to Kt3 (ch), B to Kt4. 29. Q × Kt, R to Q2. 30. B × P (ch), K to R sq. 31. Q to B4, and the game would probably result in a draw.

(h) This was very weak. R to Q2 would have been better.

(i) The climax of a series of brilliant moves. White we think would now win in any case, but B to K3 appears to be stronger.

(j) An interesting continuation would have been if the Black Queen had, instead of capturing the Pawn, played Q to Q4; then follows 32. Q × B (ch), Q × Q. 33. P × Q. Black could not now play P × B on account of 34. R to R2; but R to Q2 would have equalised matters.

ANSWERS TO CORRESPONDENTS.

*** *Please address Chess Editor.*

An Inquirer.—Cook's "Synopsis" is out of print.

Our Whist Column.

By "Five of Clubs."

ILLUSTRATIVE GAME BY MR. LEWIS.

The Hands.

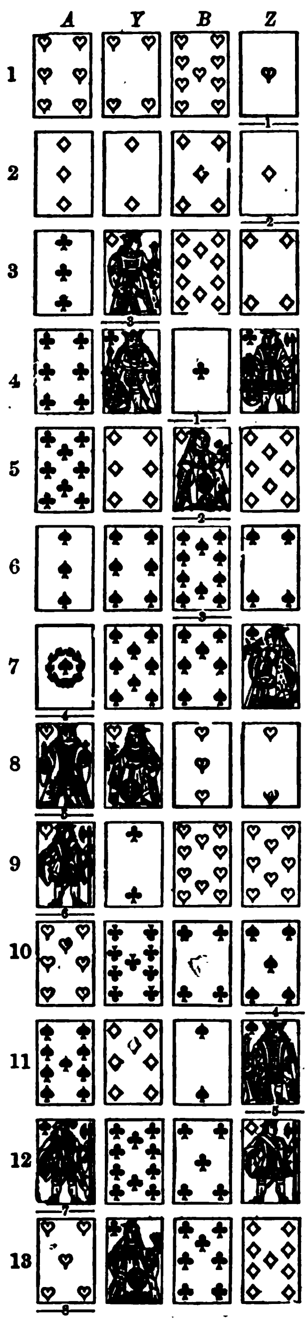
B { *Spades*—10, 7, 2. Hearts—10, 9, 3. Clubs—A, 7, 5, 4. Diamonds—Q, 10, 5. }

Y { *Spades*—8, 6. Hearts—Q, 4. Clubs—K, Q, 10, 9, 2. Dia.—K, 7, 6, 2. }

A B, 3. B — Y — *Y Z*, 3. — Dealer. Z — *Trump.* S. K. — Leader. A

Z { 4, 5, Q, K—*Spades*. 2, 8, A—Hearts. Kn—Clubs. 4, 8, 9, Kn, A—Dia. }

A { *Spades*—A, Kn, 9, 3. Hearts—K, Kn, 7, 6, 5. Clubs—8, 6, 3. Diamonds—3. }



NOTES AND INFERENCES.

By Mr. Lewis.

1. *A* leads the penultimate.

3. *A* properly discards a Club, having ample strength in trumps.

4. The fall of the Ten in the previous trick leaves *B* with either Knave or Queen. *Y* opens his strong suit of Clubs.

5. *B* gives his partner another discard.

6. The lead of trump through the honour is here forced. *Z* having four trumps passes, but he ought to have played the Q, so as to get the lead on the second round.

7. *B* continues; the fall of the cards shows *A* that the small trump is with *B*.

8. *A* now knows that he must continue his suit of Hearts. If he draws the third round of trump he is immediately forced, and leaves the long trump with *Z*, to enable him to bring in his D. The fall of the H Q enables *A* to see his way to the game, which now plays itself, but it is a curious fact that the penultimate of the original lead is, legitimately, the last card, and the winning one.

Note by "Five of Clubs."

The game is the more interesting that the hands are so well matched. The strength of *A-B*, however, is divided, while *Y-Z*'s lies nearly all in *Z*'s hands. Thus I think *Z* would have done well to lead trumps at trick 2, trusting to get some help from his partner. The game would then have opened as follows:—

	A	*Y*	*B*	*Z*
1.	H 6	H 4	H 9	<u>H A</u>
2.	S 3	S 8	<u>S 10</u>	S 4
3.	<u>H K</u>	H Q	H 10	H 2 (a)
or 3.	C 6	<u>C Q</u>	C 4	C Kn (b)
or 3.	S <u>A</u>	S 6	S 2	S Q (c)

In case (a) it is obvious that *A* would be at a disadvantage. He would not force *Y*, the weak trump hand; he could not know that a lead of Ace Spade would draw *Y*'s last trump and leave him, *A*, free to force *Z*, and he would have no means of determining which of the other suits would turn out the best lead. In case (b), *Y* would lead Spade 6, and the cards falling would be those shown for trick 3, case (c), *A* still being in doubt whether *Y* held another Spade or not, after the Heart King had drawn *Y*'s Queen. If after 3 (b) followed by 3 (c) as fourth trick and 3 (a) as fifth, *A* ventured to play a forcing card, trusting to find *Y* without trumps, the round would end favourably for *A-B*, but they would make but the odd trick. If, on the other hand, *A* preferred, as he probably would, to return his partner a Club at trick 6, and *Z* passed the Ace, the seventh trick would fall to *Y*'s Club King. At the eighth, *A* would be forced with Club Ten, and the remaining tricks would fall to *Y-Z*, being won by S Four, S King, D Ace, D King, and C Nine. Lastly in case (c), which, considering the trump card, would be the best (though the boldest) play for *B*, *A* would lead Heart King at trick four, which would fall as 3 (a) above. He would then, knowing *Y* had no more Spades (for trick two shows he cannot hold the Seven), have forced *Z*, and so have given to the game the same aspect as in the actual play.

Answers to Correspondents.

J. D. Brown, Ealing.—You are quite right. In the Whist hand last week *Y* would have done well in discarding Diamond Ace, to show he had (probably) complete command of the suit. Yet, as his own suit was so very strong, he really had no occasion to retain the Spade to return his partner's lead. He was certain of his own Diamonds, but not of his partner's Spades.

S. R. W.—I think Clay meant rather that there is no more fatal —*i.e.*, foolish—habit at Whist than that of discontinuing a strong suit *merely* because you find it will be trumped. Certainly it is a risky experiment to go on when you are uncertain how the trumps lie. Only you have to remember that one of your opponents is likely enough to lead it, if it suits his game, and that you may thus gain little by changing your suit. If you are quite in doubt as to the other suits, you run a greater risk of playing your opponents' game by opening a weak suit, than by going on with your own. For after all, even though you are ruffed by the weak trump hand, you have advanced the clearing of your own suit, which, if your partner has strength in trumps, will tell in the long run.

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Contents of No. 127.

GHOSTS AND GOBLINS.

By Richard A. Proctor.

(*Continued from page* 180.)

As an instance of a story which has been unwisely insisted upon by believers in the supernatural, I take the marvellous narrative of M. Bach and the old spinet. As given in outline by Professor Wallace, it runs thus—"M. Leon Bach purchased at an old curiosity shop in Paris a very ancient but beautiful *spinet* as a present to his father (a great-grandson of Bach, the great composer), a musical amateur. The next night the elder Bach dreamt that he saw a handsome young man, dressed in old court costume, who told him that the spinet had been given to him by his master King Henry. He then said he would play on it an air, with words composed by the King, in memory of a lady he had greatly loved; he did so, and M. Bach woke in tears, touched by the pathos of the song. He went to sleep again, and on waking in the morning was amazed to find on his bed a sheet of paper, on which were written, in very old characters, both words and music of the song he had heard in his dreams. It was said to be by Henry III., and the date inscribed on the spinet was a few years earlier. M. Bach, completely puzzled, showed the music to his friends, and among them were some spiritualists, from whom he heard, for the first time, their interpretation of the phenomena. Now comes the most wonderful part of the history. M. Bach became himself a writing medium; and through his hand was written involuntarily a statement that inside the spinet, in a secret niche near the keyboard, was a parchment, nailed in the case, containing the lines written by King Henry when he gave the instrument to his musician. The four-line stanza, which it was said would be found on the parchment, was also given, and was followed by the signature—Baldazzarini. Father and son then set to work to search far this hidden scroll, and after some two hours' close examination found, in a narrow slit, a piece of old parchment about 11 in. by 3 in., containing, in very old writing, nearly the same words which M. Bach had written, and signed—Henry. This parchment was taken to the Bibliothèque Impériale and submitted to experienced antiquarians, and was pronounced to be an undoubtedly genuine autograph of Henry III."

"This is the story," says Prof. Wallace, and proceeds to dwell on the care with which Mr. Owen, who narrates it (in "The Debatable Land between this World and the Next"), had examined all the details. "Not content with ascertaining these facts at first hand, and obtaining photographs of the spinet and parchment" (!) "of both of which he gives good representations, Mr. Owen sets himself to hunt up historical confirmation of the story, and after much research and many failures, he finds that Baltasarini was an Italian musician, who came to France in 1577, and was in great favour with Henry III.; that the King was passionately attached to Marie de Cleves, who became wife of the Prince de Condé, and that several of the allusions to her in the verses corresponded to what was known of her history. Other minuter details were found to be historically accurate." (In other words, "the bricks are alive this day to testify it; therefore, deny it not.") "Mr. Owen also carefully discusses the nature of the evidence, the character of the persons concerned, and the possibility of deception. M. Bach is an old man of high character; and to suppose that he suddenly and without conceivable motives planned and carried out a most elaborate and complicated imposture, is to suppose what is wholly incredible." (That is, we must not suppose so because we cannot suppose so.) "Mr. Owen shows further that the circumstances are such that M. Bach could not have been an impostor even had he been so inclined, and concludes by remarking, 'I do not think dispassionate readers will accept such violent improbabilities. But if not, what interesting suggestions touching spirit-intercourse and spirit-identity connect themselves with this simple narrative of M. Bach's spinet!'"

Here is a story which to most readers, I venture to say, appears absurd on the face of it, suggesting not "interesting," but utterly ludicrous "ideas of spirit intercourse;" yet we are to believe it, or else indicate exactly how our doubts are divided between Mr. Owen himself (who may have been somewhat misled by his evidence), the Bachs, father and son, the spiritualist friends who instructed M. Bach how to become "a writing medium," and so on.

Again, we are to believe all such stories unless we are prepared with an explanation of every circumstance. It seems to me that it would be as reasonable for a person who had witnessed some ingenious conjuring tricks to insist that they should be regarded as supernatural, unless his hearers were prepared to explain the exact way in which they had been managed. Indeed, the stress laid by the superstitious on narratives such as those related by Mr. Owen is altogether unwarrantable in the presence of all that is known about the nature and the laws of evidence. In works like Mr. Owen's the author is witness, judge, and advocate (especially advocate) in one. Those who do not agree with him have not only no power of cross-examining, but they commonly have neither time nor inclination to obtain specific evidence on their side of the question. It requires indeed some considerable degree of faith in the supernatural to undertake the deliberate examination of the evidence adduced for ghost stories—by which I mean, not the study of the story as related, but the actual questioning of the persons concerned, as well as an examination of the scene and all the circumstances of the event. Therefore I cannot see any force in the following remarks by Professor Wallace:—"How is such evidence as this" he says, speaking of one of Owen's stories, "refuted or explained away? Scores, and even hundreds of equally attested facts are on record, but no attempt is made to explain them. They are simply ignored, and in many

cases admitted to be inexplicable." Yet this is not quite satisfactory, as any reader of Mr. Owen's book will be inclined to admit. *Punch* once made a Yankee debtor say:—

> This debt I have repudiated long ago;
> 'Tis therefore settled. Yet this Britisher
> Keeps for repayment worritting me still.

So our philosophers declare that they have long ago decided these ghost stories to be all delusions; *therefore* they need only be ignored; and they feel much "worritted" that fresh evidence should be adduced, and fresh converts made, some of whom are so unreasonable as to ask for a new trial, on the ground that the former verdict was contrary to the evidence.

All this affords excellent reason why the "converts" should not be ridiculed for their belief; but something more to the purpose must be urged before "the philosophers" can be expected to devote very much of their time to the inquiry suggested. It ought to be shown that the well-being of the human race is to some important degree concerned in the matter, whereas the trivial nature of all ghostly conduct hitherto recorded is admitted even by "converts." It ought to be shown that the principles of scientific research can be applied to this inquiry; whereas, before spirits were in vogue the contrary was absolutely the case, while it is scarcely going too far to say that even the behaviour of spirits is to be tested only by "converts," and in the dark. It ought, lastly, to be shown that the "scores and even hundreds" of well-attested facts, admittedly singular, and even, let us say, admittedly inexplicable, are not more in number than the singular and *seemingly* inexplicable facts likely to occur (by mere casualty) among the millions of millions of events which are continually occurring; but this is very far from having been as yet demonstrated: on the contrary, when we consider the scores and hundreds, and even thousands of facts which, though they have been explained, yet seemed for awhile (and might have remained for ever) inexplicable, the wonder rather is that not a few books like Mr. Owen's, but whole libraries of books, have not been filled with the records of even more singular and inexplicable events.

THE SUN'S ATMOSPHERE.

By W. Mattieu Williams.

On page 166 (Knowledge, March 14), Mr. Proctor expounds a difficulty on which I have often pondered—viz., that presented by the actual density of the sun contradicting so positively the density which is theoretically due to the self-condensation of his immense atmosphere by its own pressure.

The old idea that the dark nucleus of the spots is the comparatively cool surface of a *solid* body underlying the luminous atmosphere need not be discussed. We now know sufficient of the materials resting on the sun to understand that in viewing such a nucleus we are looking into the profundity of a dark abyss of intensely heated matter, which is dark simply because it contains no solid or liquid particles capable of reflecting light. Recent investigations have shown that gaseous matter, free from solid or liquid particles, gives out no light even at what we call a "white heat" of the greatest obtainable intensity.

Assuming that the photosphere is the superficial region in which occurs the recombination of dissociated gases from within, and that the spectroscope tells us the truth concerning its materials, there must be a definite limit to its temperature, as such gases cannot combine at a temperature reaching that which effects their dissociation; but the dissociated elementary gases of the inner depths may attain any conceivable temperature, and therefore become hotter and hotter at greater and greater depths. This increasing heat (*i.e.*, increasing repulsive energy) may at some point balance the pressure due to the gravitation of the superincumbent gaseous mass, and this critical point once reached, the balance of opposing forces may continue even to the centre of the solar mass, as further condensation could not be effected by the pressure without simultaneously developing more heat to overcome itself.

According to this view the maximum density of solar matter may be reached at a comparatively moderate depth below the photosphere, and be no further increased, even down to the very centre, in spite of the enormous magnitude of this orb.

Something like this must be supposed, or we must throw overboard all the revelations of the spectroscope, and assume that the sun is composed of matter utterly different from anything terrestrial; for if it were merely a great bubble of hydrogen gas, its mean density would (without such critical limit to compression) vastly exceed that which its own magnitude and the rates of planetary revolution prove it to possess.

Some of the readers of Knowledge will probably remember that I expressed this idea of the critical condition of the materials of the sun at the conclusion of my papers on "Solids, Liquids, and Gases" (Vol. I., p. 157), as follows:—"Applying now the researches of Dr. Andrews to the conditions of solar existence, as I formerly applied the dissociation researches of Deville, I conclude that the sun has no nucleus, either solid, liquid, or gaseous, but is composed of dissociated matter in the critical state, surrounded, first by a flaming envelope, due to the recombination of the dissociated matter, and, outside this, another envelope of vapours, due to this combination." For the further reasons on which this conclusion is based, I must refer to that paper.

Besides the above-stated difficulty, there is another atmospheric mystery connected with the sun which is still more paradoxical.

Far away, more than a million of miles beyond the surface of the photosphere, the spectroscope reveals gaseous matter, "*helium*" it has been named. What is this? That it should be an actual atmosphere extending so far beyond the sun is inconceivable, unless we construct a special ether from the materials of our own imagination and transfer it to the sun to serve this particular purpose.

Nevertheless, we need not despair. I think we may find an explanation of this mystery without transcending experimental fact.

Assuming that my explanation of the corona (see "Fuel of the Sun") is correct, viz., that it is mainly composed of matter ejected by the prominences, originally gaseous, but more or less condensed into liquid and solid masses in the course of its flight, and also that I am right in supposing that the ferruginous meteorites that fall on our earth may, some of them, be samples of extreme ejection, the helium mystery is solved at once.

Graham proved, and others have reproved, that such meteors contain large quantities of occluded hydrogen, which pours out when a fragment is heated in vacuo. If, then, the corona consists of a thick hail of such meteoric matter newly ejected from the bowels of the sun wherein the occlusion of the compressed hydrogen was effected, every one of these fiery hailstones must be vomiting its occluded hydrogen, &c., as it flies outward into comparatively vacuous space, and these streams of occluded gas must outspread themselves violently when thus released in such approximate vaccuum.

I will even go so far as to predict the chemical composition of the material which supplies the ·1474 line.* I believe that by properly operating upon meteoric iron with the apparatus required, we shall find it to be hydrogen gas holding iron in solution; ferruretted hydrogen gas, analogous to the arseniuretted hydrogen obtained in using the well-known Marsh's test for arsenic; and to other corresponding compounds.

I have tried to produce it artificially, but without decided success, with a degree of failure fairly attributable to want of appliances for proving its existence, rather than to its actual non-existence.

* A green line, not to be confounded with the orange-yellow line of Helium.—R. P.]

HINTS ON ROWING.

By an Old Club Captain.

It is singular how unapt are most beginners in any form of exercise to consider the special aim and purpose of the exercise. They mostly try to acquire, only, the movements involved in the exercise. Thus the special object in tricycling is to work the outer rims of the driving wheels against the ground in such a way that the body of the tricycle and with it that of the tricyclist may be urged rapidly forward. But nine out of ten who begin tricycling, work as though the great object were to work the feet round the axle. In their zealous efforts to do this, it seldom occurs to them till they have been some time at work (many weeks in some cases) that they are using one leg against the other. They wonder that they make little progress, and soon get tired out; but the real wonder rather is that so many persevere in the wearisome task. At last the idea dawns on them that the tricycle is a piece of mechanism designed for a certain purpose, and that it is as well to direct their efforts to accomplish that purpose, and not a purpose for which the machine was not devised. *Then*, putting out their strength in such a way that first one foot drives, the other carefully refraining from resistance, then the other takes its turn, and so forth, each stroke being mentally directed to bring round the driving rims as energetically as possible, the tricyclist finds his strength doubled and his labour halved,—or his effective work quadrupled. That which had been a weariness of the flesh becomes a pleasure.

With rowing the case is similar. Not only the beginner but in nearly every case the teacher also, proceeds as though the great object in rowing were to put the blade of an oar into the water in a certain way to bring it through the water, to feather it with skill and dexterity, and then to carry it back for another movement through the water. If both the beginner and the teacher consider the real purpose of rowing, a much better start will be made in the task of learning to row, and much quicker progress will be effected. Let the beginner regard his oar as a lever for urging the boat along, and let him consciously direct his exertions to that end, and he will from the beginning work on correct principles. He will *feel* that to get work out of his lever he must put the blade in squarely. He will *feel* that jerking and scooping must be useless and worse, and that hawling in any other way but parallel to the course in which the boat has to go must be bad. He may be troubled a little by the curved course which his hands have necessarily to take, but he will *feel* that the pull upon the oar must not vary correspondingly in direction, but should be made (and may pretty easily be made) always parallel to the boat's length. Presently he sees that to secure effective propulsion he may act as though his object were to propel the water backwards, and that the greater the amount of water driven along the surface backwards (no part of it being driven either upwards or downwards) and the more effectively it is so driven, the greater the propulsive effect on the boat,—action and reaction being always equal. This I have invariably found, when teaching the young idea to row, marks the beginning of an effective style. The learner from the time he notes this begins to send sternwards the kind of swirl which the coxswain and the stroke like to see, not a formless roughening of the water but a well-shaped and swiftly circling swirl which means work done by the backward propulsion of a goodly mass of water. In aiming, on his own part, to send down such a swirl, the learner, if he really means work, soon finds his way to all the good points of style. Beginning (as is generally best) with strokes not too long, he lengthens his stroke by forward reaching as his mastery of the oar increases, and so increases the mass of water sent sternwards. He carefully keeps his blade square that there may be nothing lost by the slip of the water over or under it. He keeps his blade well covered that no part of it may waste its power on the idle air; but he refrains from dipping it lower where no increase of power is to be found though the labour would be much increased. Then too he feels that at the end of his stroke the oar must be brought cleanly from the water or the backward swirl (which really means and measures the forward urging of the boat) will be impaired and the boat's way correspondingly checked. So he soon acquires the art of bringing the top of the blade above water just before the end of the stroke, then on the instant giving the blade a sharp half-turn by which the swirl is neatly finished off and sent down as a pleasing little Maelstrom besides the boat's wake. In going forward the oarsman, still rowing with his head as well as his hands, feels that he cannot do better that to keep his blade flat, for so it will least oppose the air and do least harm should a passing wave unluckily touch it. He will recover sharply, especially in a heavyish boat, because all the time he is not pulling the effect of his last propulsive effort is being exhausted. And he will square his blade at the very instant that the forward motion of his arms and hands ceases, and he is about to drop in the blade for a new propulsive effort.

Of course a good Mentor, though he may save himself ever so much trouble by reminding his pupil of the real object in rowing (the propulsion of the boat) can still be very useful to the learner by telling him how best to apply his strength by showing how the arms are most effectively used in conjunction with the swing of the body, not *alone* (as many strong-armed learners try to use them), by calling his attention to the necessity of a straight swing from the hips, and by many hints as to the use of hands and wrists, loins legs and feet. But the great point, I am persuaded, is to correct at the outset the idea that the object in rowing is to work the oar through air and water alternately, instilling instead the true ideas—viz., that the object is to propel the boat, that the oar is a most effective lever, the boat the weight levered, and the water the fulcrum, from which alone,—yielding or unyielding,—propulsive purchase is to be obtained.

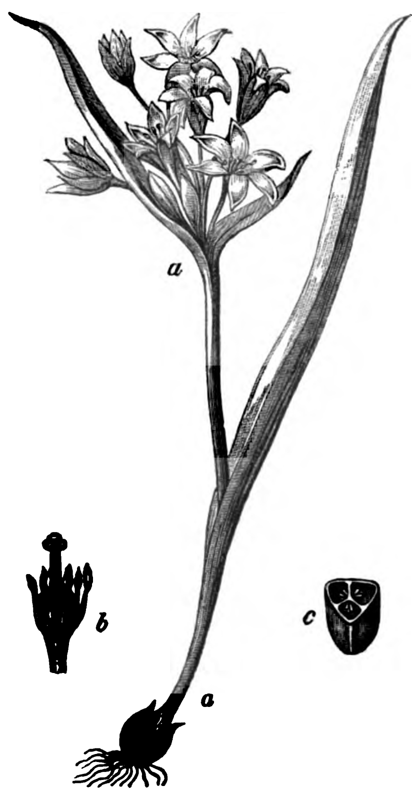
On these principles I have taught a beginner more in two lessons than many learners acquire in a month's practice. My last pupil, for instance, was a lady, and a waterman who steered during the second lesson, spoke with approval of her *style*, where in reality there was nothing but the conscious endeavour to secure propulsive effect.

THE EVOLUTION OF FLOWERS.

By Grant Allen.

V.—TRUE LILIES.

THE flowering rush and the arrowgrass brought us so very near the true lilies in every important particular that we scarcely feel we have made any transition worth speaking of when we arrive at the simplest and most primitive existing members of the restricted lily family—the Liliaceæ of technical botanists. Of these simplest kinds we have two or three excellent representatives in our own country, quite as good as any we could get if we were to search the whole dried specimens of the Kew herbaria. The best of these for our present purpose is that rare little field lily, the yellow gagea (*Gagea lutea*).



Gagea lutea.

In a few sandy meadows of England and Scotlands, some straggling colonies are found of a pretty little golden lily, whose proper habitat stretches over the great central plains of Eurcpe and the warmer portions of the Siberian slope. Like most other true lilies, this little plant is a dweller in the fields, while the Alisma-like kinds with which we have hitherto been dealing are all of them denizens of the ponds and marshes. Such a fact is in itself a significant one: the more advanced type has overrun the wide plains and uplands of the entire world, while the lower types have everywhere been crowded out into the less desirable habitats, such as water-courses, swamps, and hilltops. Like most other meadow-plants, too, our gagea has been forced, in competition with the grasses, to acquire long and narrow blade like leaves, so as to reach the air and light among the tall plants with which it has to struggle. Furthermore, it shares one common habit of a great many lilies in the fact that it possesses a bulbous root-stock. In this root-stock the starches and other food-stuffs laid by in one season are stored away for the use of the flower in the succeeding spring; and, as a general rule, it may be said that most bulbous lilies and other lily-like monocotyledons (including the very closely-allied iris and amaryllis families) are spring-flowering plants. There can be very little doubt that the prevalent bulbousness of the lilies is one of the points which has chiefly aided them in establishing themselves so widely and firmly as they have done in the very best situations over the entire world.

These, however, are not the peculiaries of the true lilies which chiefly strike the classificatory botanist. As a matter of structural development, the important particulars to note about the gagea, as compared with the flowering rush and the water-plantain, are chiefly these. The carpels, reduced to a single whorl of three, as in marsh arrow-grass, are here firmly united into a single solid ovary, which never at any time divides into its component parts, but opens in the centre of each carpel to shed the seeds. The stigmas (or summits of the carpels) are combined into a single style, which, however, in most lilies bears three separate stigmas at its top, as a last witness to their original distinctness. Each carpel contains several seeds—an advance which we already noted in the flowering rush. The stamens, instead of being numerous, as in arrowhead, or nine, as in flowering rush, are six in number, as in water-plantain. The general formula for the lily family (though subject to a few exceptions) is about as follows:—Perianth, of six divisions petal-like; stamens, six; ovary, free, three-celled; styles, single, with a solitary or tripartite stigma; seeds, usually many in each cell, sometimes solitary.

Now, what is the practical meaning of all this in its evolutionary aspect? Simply that the lilies have taken, for greater security of fertilisation, to running their three carpels together, and especially to uniting their three stigmas or sensitive surfaces into one, so that a single act of fertilisation suffices for the whole lot. Being all (in the main—about the exceptions we will speak hereafter) insect-fertilised, they have conspicuous-coloured flowers; and the sepals as well as the petals share in the attractive display. The lower lilies bear capsules with many seeds; in the higher ones, as we shall soon see, the development of berries has allowed the number of seeds to be still further reduced to three, or one in each carpel—called, in the case of united ovaries, a cell.

Our gagea thus possesses all the most important distinctive lily features, as compared with the flowering rush, the water plantain, and the other alismaceæ; but, in certain minor respects, it shows many signs of being a very primitive lily indeed. One need only compare the present illustration with the illustration of the flowering rush in order to see how markedly like the two plants are in the most notable external features. The flowers here are several in number, with leaf-like bracts beneath each flower-stalk, and the sepals and petals, instead of being bell-shaped, as in the tulip and wild hyacinth, or combined into a single piece, as in the lily of the valley and the garden hyacinth, are quite distinct and broadly spreading, as in the flowering rush. In fact, whilst most other lilies display the common lily features, with some special modifications and additions, such as tubular corollas, fleshy-coloured berries, flattened stamens, abortive leaves, and so forth, the gagea displays them almost in their uncompounded purity, without any complications or additions of any sort. It thus shows itself to be a survival from a very primitive and simple form of the common liliaceous stock. There are, however, a few other lilies which, while more advanced in some ways than gagea, yet preserve some more antiquated or original features which this little plant has entirely lost.

Indeed, the Scottish asphodel (*Tofieldia palustris*) perhaps still more closely resembles the earliest lily ancestor in most important respects, especially in the fact that, like the arrowgrass, it has three distinct and separate styles—a very archaic characteristic, certainly; but, for some other reasons, I incline to consider the Tofieldia genus as a degenerate one, and so have not chosen it as my first representative of the true lily group.

I haven't yet mentioned the most interesting particular of all about our little gagea, and that is the peculiar colour of its sepals and petals. In common with all the other members of its genus, it has yellow flowers; and I have already tried to show on several occasions that yellow was the original colour of all blossoms—white, pink, and blue being successively later acquisitions. Moreover, gagea bears a very striking resemblance in hue and general appearance to several buttercups, especially to the lesser celandine (*Ranunculus ficaria*); and buttercups, as we know, are very primitive dicotyledons. Curiously enough, too, the petals and sepals are yellow on the inner (or exposed) side only; the outer side is green, so that the colour looks almost as if it had been daubed on with a brush upon one surface of a small green leaf. I have very little doubt that we see here a relic of an extremely early stage in the acquisition of colour by the petals of insect-fertilised flowers. And it is a significant fact that the other primitive genus of lilies, Tofieldia, has likewise greenish-yellow blossoms. Gagea is mainly impregnated by bees and flies. It seems to increase for the most part, however, by means of the bulbs, each old bulb producing two new ones in the course of every season, one on either side. These two bulbs are the store-houses in which the old plant lays by material for the flowering of its two successors in the following spring. Perhaps this practical comparative neglect of true reproduction by fertilised seeds, and substitution of the essentially non-reproductive method of increase by means of bulbs, may account for the numerous early characteristics displayed by gagea; for in reality each bulb is not a new plant, formed by genetic union between two old ones, but merely a bud from the old plant, springing afresh just as a cutting or sucker might do. Thus many existing gageas may really be parts of the very same plant that flourished innumerable generations since, a contingency which would bring them far nearer the original ancestor than other lilies which have been almost yearly reproduced from seed for countless ages.

NOTE ON FLYING-MACHINES.

In all ages there have been thoughtful men who have looked with envy on the flying powers of birds, and have asked themselves the question whether man—the "unfeathered biped" of Plato—might not rival the feathered tribe even in their natural element. Hatton Turnor, in his "Astra Castra," asserts even that King David had such thoughts in his mind when he said, "Oh, that I had wings like a dove, for then would I flee away and be at rest." Without going quite so far, we think it may safely be asserted that few men who have watched the movements of birds in the air have not felt that the possession of such powers of aërial locomotion would be an enviable thing. Those who have taken part in balloon ascents speak of the strange feeling of exhilaration with which they see the things of earth diminishing beneath them. But if this feeling is experienced by those who, though raised above the earth, are yet not free to direct their motions according to their wish, how largely would the pleasure of aërostation be enhanced could the aëronaut direct his flight as freely as the birds of the air. Now soaring high above the clouds, now skimming the surface of the earth, the flying man would enjoy sensations which could almost be worth purchasing at the risk of breaking his neck.

Old as the problem of flight is, it is only within the last few years that its conditions have been thoroughly understood, and the practicability of its solution evidenced. By flight, it must be understood we refer wholly to the power of winging a way at will through the air—not to the mere power of rising above the earth. It seems to have been proved beyond all possibility of doubt that no contrivance founded on the principle of the balloon can possibly avail to enable man to travel at will through the air. The enormous volume which must be given to the balloon in order that its buoyancy may suffice to raise it safely with its living freight into the air, renders it so unwieldy that it is the sport of even the gentlest air-currents. Even in perfectly calm weather it has been found impossible, even by the most ingenious contrivances which could be devised for the purpose, to urge a balloon with moderate speed in any required direction.

In fact, the careful consideration of the subject has shown that mere lightness is far from being a desideratum in a flying-machine. M. Nadar aptly suggested the true difficulty which lies in the way of aërial navigation by means of balloons, in his description of a circumstance which fell under his notice in the streets of Paris. A workman on the roof of a house let fall a sponge, and called on one of his fellows to throw it up to him. Now, says M. Nadar, the man was too clever to fall into the mistake which so many aëronauts have made, of supposing that the lighter an object the better adapted it was for flight through the air. He knew that the light sponge could never be thrown to the roof of the house. So, what does he do? He first wets the sponge, and when, by this means, he had rendered it heavy, he was enabled to throw it to his companion.

Our first step, then, towards the solution of the problem of flying, consists in the discovery that weight—or at the least a certain proportion between weight and surface—is absolutely essential when something beyond mere ballooning is in question. But we must not fall into the mistake which M. Nadar made, of supposing that weight is in itself an aid to flight. We must remember that the sponge thrown up by the workman was under the influence of a force which was in reality derived from the resistance of the solid earth on which the workman stood. It would be as reasonable to urge that a cannon-ball was the model of what a flying-machine should be, because, propelled from a Whitworth gun, it traverses five or six miles before reaching the ground. Strictly confining our deductions to what is really proved by Nadar's illustrative anecdote, we say that directive power can only be given to an aërial machine which is not so buoyant as to be the sport of every wind that blows. A contrivance like the balloon, which requires that a surface of hundreds of square feet should be exposed to the air, can never, we see, be available for more than mere floatation.

It may be questioned, indeed, whether any modern invention has proved more unfavourable to the science of aërial locomotion than that of the balloon. Many ingenious flying machines had been invented before the time of Montgolfier; and in some of these we see signs of an appreciation of the real difficulties which attend the problem of artificial flight. But so soon as Montgolfier had exhibited the powers of rarefied air, men gave up for awhile the notion of attempting to fly by the aid of machinery. And the discovery that by means of hydrogen gas a

balloon might be made to float in such a way as to be a much safer conveyance than the fire-balloon, induced men of science to turn their attention to the improvement of balloons as the likeliest mode of solving the problem of aërial navigation. Thus it happened that the first half of the present century, though adorned by so many important discoveries in other branches of mechanics, was wholly unfruitful as respects the long-vexed question of the possibility of flying. Of late years, however, the subject has been taken up with renewed vigour and with fair promising results.

As regards old attempts to solve the problem of aërial navigation, we may mention—passing over the fabulous story of Dædalus and Icarus—the automaton bird constructed by Archytas of Tarentum, as in all probability the first successful attempt to construct a flying-machine. There seems no reason to doubt the truth of the narrative handed down to us respecting the labours of Archytas. All that Aulus Gellius mentions is that the Tarentine constructed the figure of a dove, "so contrived as by a certain mechanical art and power to fly; so nicely was it balanced by weights, and put in motion by hidden and enclosed air." On this the ingenious Cardon remarks justly, "there is no reason why such a machine should not be put in motion, especially by a favourable breeze. The lightness of the body would contribute to this result; as would the largeness of the wings and the strength of the wheels; and probably the dove could take its flight in a certain fashion, but with a wavering motion like the flickering of a lamp. Thus, it would sometimes mount upwards spontaneously, flutter its wings, then leave off suddenly and fall—its motive power being unequal to its weight."

But the attempt to attach wings to the human frame, so that man might emulate the birds, is one which has had more advocates than the plan of constructing automatous machines. In the reign of Edward the Confessor, an English monk, having attached wings in an ingenious manner to his limbs, leaped from a church tower into the air, and succeeded in covering a distance of 220 yards before reaching the ground. The flight was a daring one, and similar attempts have not often had so satisfactory a result. An Italian monk, who exhibited his powers of flight before James III. of Scotland, fell to the ground with such force as to break his thigh. And Allard, the rope-dancer, who attempted a similar exploit in the reign of Louis XIV. of France, was equally unfortunate.

Indeed, attempts of this sort have been so uniformly unsuccessful—for even the Saxon monk only managed to attain a limited distance—that the idea has come to be entertained that it is perfectly futile for man to attempt to fly by means of his own unaided powers. Those who have believed in the possibility of flight have fixed their hopes rather on the construction of large aërial ships, propelled by adequate machinery, than on man's power of imitating the flight of birds. The ingenious mathematician Borelli attempted to demonstrate the impracticability of human flight. He exhibited the striking contrast which exists between the pectoral muscles of birds and those of man. On either side of the breast-bone of the bird there are masses of muscle whose energy is wholly efficient in working the wings. But the corresponding muscles in man have scarcely any power whatever. Let any one contract his shoulders and notice how the muscles over the chest take part in producing the movement; their action, he will find, is altogether wanting in energy.

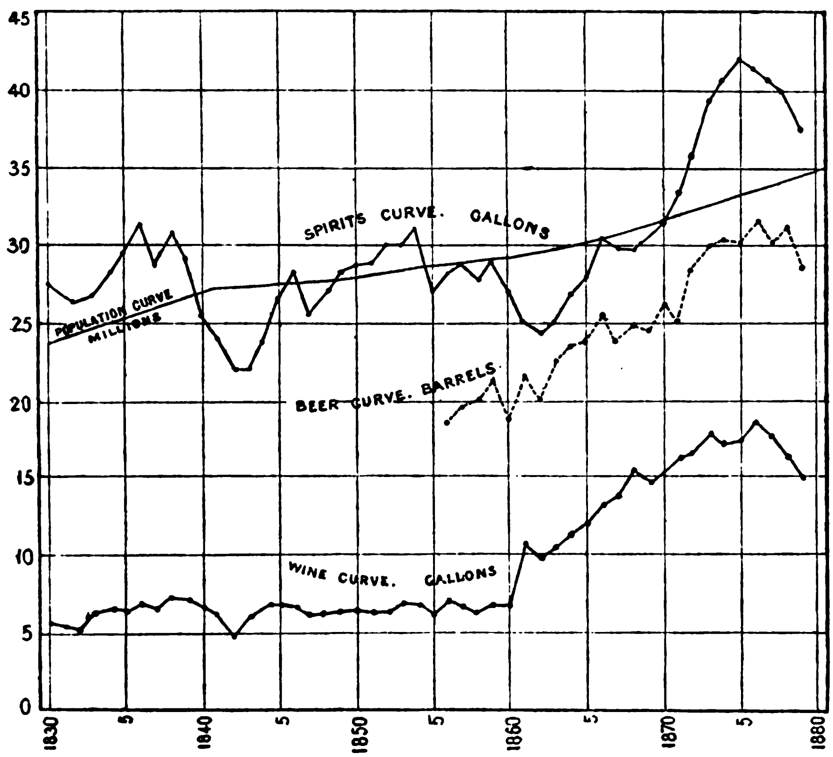
But it needed no mathematician to show us that if man is ever to fly, he must not attempt to imitate directly the action of a bird. As artificial appendages are absolutely necessary, it is obviously in man's power so to choose the arrangement and adaptation of these appendages, that in using them his limbs will work in the manner which best suits them. Since in upward and downward action man's arm is weak—as any one will find who tries to beat the air by such action for only a few minutes—it is clear that the mere flapping of wings attached to the arms cannot possibly avail to enable man to fly, however ingeniously those wings may be devised. On the other hand, the pulling power of the human arm is considerable, and such an action as that used in rowing can be maintained for hours without exhausting the frame. Hence this action is one which may be applied, if only suitable contrivances be devised, to aid in propelling the body through the air. Again, the legs of a bird are useless so far as flight is concerned, but there is no reason why man, in attempting to fly, should not make use of his legs as well as his arms. In walking or running, the legs exert a large amount of power in a manner which does not quickly weary the frame. Hence, in applying the action of the legs to aid in propelling the body through the air, the mechanician must devise such contrivances as shall call this customary action into play. If the full power of the arms and legs can be so applied to ingeniously-arranged mechanism as to work wings more or less resembling those of a bird, there is little reason for doubting man's power of sustaining himself in the air, and even travelling with great rapidity through it. Probably it will be much easier for him to sustain himself while travelling rapidly onwards, than while hovering over the same spot.

The Matrix of the Diamond.—Until the South African mines were discovered, the diamond was always found in sands and gravels, different from the mineral in which it was believed to be formed. At Griqualand West, however, the consolidated eruptive mud of the mines was believed by some to be the true matrix of the diamond; but opinions differed on the question, and arguments were found on both sides. M. Chaper, a French geologist, has, however, during a scientific mission to Hindustan, succeeded in finding the diamond in its mother rock. At Naizam, near Bellary, in the Madras Presidency, M. Chaper has found the diamond in a matrix of rose pegmatite, where it is associated with corundum. The tract of country is almost denuded of trees, bare and rocky, and the rains wasting the rocks, every year expose fresh diamonds in the soil. The rock is traversed by veins of feldspar and epidotiferous quartz. Here the diamond is always found, associated with epidotiferous rose pegmatite. The diamond crystals observed are octahedral, but less distinct in line than the stones of South Africa, which seem to have been formed in a freer matrix. It follows from M. Chaper's discovery that diamonds may exist in all rocks arising from the destruction or erosion of pegmatite—for example, in quartzites with or without mica, clays, pudding-stones, &c. —*Engineering.*

A New Illuminant.—Lieutenant Diek, of the Russian Army, is said to have discovered a new illuminating substance which is capable of imparting luminous properties to objects to which it is applied. It is in the form of a powder, and of three colours—green, yellow, and violet, the latter being the most powerful. Water in a glass vessel is by this means converted into an illuminating fluid. In a lecture recently delivered by the inventor at the Nicolai Engineering Academy, at St. Petersburg, he explained the application of the substance to military and industrial mining operations. The illuminating power lasts for eight hours, and the powder must then be renewed. The German Government is said to have been lately making experiments with Lieutenant Diek's invention.

HOW WE DRINK.*

IN studying the drunkenness of the last fifty years, we are not bound to take the word of any individual as to the comparative dissoluteness of a particular class of people. We are in a position to say with precision when drunkenness has been most indulged in by the drinking class, and when most eschewed. We live in an age of statistics, and data on this subject are rigorously collected for revenue purposes. It appears then that, notwithstanding temperance advocacy, drunkenness has increased during the last fifty years. It is matter for congratulation, however, that this increase is for the most part only the increase due to a larger population; in other words, although the total consumption of intoxicants is much greater now than it was—say, in 1830, the individual consumption remains very nearly the same. We have all the data for an exact study of the subject in the elaborate and formidable array of figures collected by the Rev. Dawson Burns, in a paper on "Fifty Years' Consumption of Intoxicating Liquors in the United Kingdom (1830-79)."† We prefer to present the information in the form of curves, as the reader can then see with ease and at a glance, the fluctuations of consumption in the half-century. To the unscientific reader, however, some explanation is necessary as to the way in which such curves are constructed. In the illustration a series of perpendicular lines (ordinates) represent the years from 1830 to 1881, and a series of horizontal lines (abscissæ) represent millions, from twenty millions at the bottom to forty-five millions at the top. Now, let us say that in 1830, 27¾ million gallons of spirits were consumed in the United Kingdom, then a point on the 1830 line, and proportionately between the twenty-seven and twenty-eight million lines, marks the fact. Similarly, if in 1831 the consumption were 26¾ million gallons, a point on the 1831 line and between the twenty-six and twenty-seven million lines would represent it. By joining these two points we obtain the first portion of the drink curve relating to the consumption of spirits in the United Kingdom in 1830 and 1831. In the figure the general form of this spirit curve is given for comparison with the beer and wine curves below it. The comparison is a very instructive one.



Looking at the wine curve it will be observed that the consumption slightly fluctuated above and below six million gallons per annum until 1860. Hitherto it had been the beverage of the rich; alternations of good and bad trade had very little affected the consumption of it, but the great leap in the curve from 1860 to 1861 marks the era of the introduction of cheap and nasty wines into this country; and thenceforward, under the ægis of free trade, the wine curve showed itself more sensitive, and followed the general trend of the beer and spirits curves.

Beer is the liquor of the poorer class of drinkers. It is drunk in such quantities in the United Kingdom that for the convenience of comparison we have plotted the curve for millions of barrels instead of millions of gallons. So far as the curve goes, data being wanting for its construction prior to 1856, it follows the general direction of the other curves. It will be noted, however, that from whatever cause or causes the fluctuations in these curves arise, the spirits curve is the most sensitive of the three. It reached its highest point in 1875 before the others, and, as the most susceptible of the three, began its downward course the first. We regard it as *the* drink curve *pa*-

* From Mr. W. Ackroyd's exceedingly interesting "History and Science of Drinking." (Simpkin, Marshall, & Co., London.)

† The National Temperance League's Annual for 1881, pp. 38-45.

excellence, showing as it does in unmistakable manner whether the rate of drinking is a high or a low one, and whether it is on the increase or on the decrease. We may, therefore, now turn our attention to it alone.

If the population curve, showing the increase in population from 1831 to 1881, be plotted down along with the drink curve, it will be seen that they both take the same general upward direction; in other words, that the home consumption of spirits in the United Kingdom has increased with the population, and at about the same proportionate rate. It will be further observed that the two curves cross each other several times in the course of the fifty years. On each of these occasions the individual consumption was the same, viz., one gallon of spirits per head per annum.

The most remarkable thing about the drink curve is its irregularity, its succession of peaks and valleys contrasting markedly with the gently sinuous population curve, and it is a matter of some importance to our subject for us to inquire into the causes of this irregularity. It is perhaps not possible to formulate any definite proposition respecting those causes. We find that the curve descends during bad times and ascends during good times, so that commercial depression and prosperity are two important factors in producing the irregularities of the drink curve. Thus, for example, the drink curve has been below the population curve during periods of commercial depression, as *e.g.*, in 1842-1843 and 1859-1870. The former was the period of plug-drawing riots, and the latter of the American War, and of the failure of joint-stock companies. Turning to the last rise in the curve, we know that in 1870 to 1871 commerce was recovering from years of reverses. The next few years were years of great prosperity. In 1871, 1872, and 1873 the curve went upwards with great speed; the ascent was continued in 1874 and 1875, though at a diminished rate. During the three former years money was plentiful among the people, and the drinking section spent it recklessly in liquors, and continued to do so even up to 1875. The tide of depression appears, however, to have already set in, and since then the drink curve has been going downwards.

The continued fall of the drink curve in recent years, or, in other words, the decline in the drink revenue, has commanded much attention of late because of its political importance. It has been regarded as an indication of reformation of English habits. We trust that it may be so. There have certainly been other causes at work to make the decline continuous besides commercial depression. One of no small importance has been the spread of education since the passing of the Education Act of 1870. Another has been the substitution of suitable places of resort, such as coffee taverns and working men's clubs, for beershops and liquor shops. And last, though not least, there has been the persistent advocacy of temperance reformers. The great work which has been accomplished by the temperance societies of the United Kingdom has probably been of a checking nature; they appear to have been highly successful in preventing the contagion of drunkenness from spreading as it otherwise might have done, so that after fifty years of work the consumption of spirits per head per annum is little more than it was when they started. How it might have developed is illustrated by the example of Belgium, where the consumption of liquors per head per annum has increased to a frightful extent, and in 1882 had the alarming quantity of seventy pints! England is therefore not the most drunken of nations, thanks to the checking agencies which have been at work for a great number of years. We cannot hide from ourselves, however, the great probability there is that the drink curve will take another decided turn upwards on the return of thoroughly prosperous times, but it is the wish of all who are well acquainted with the significance of this upward movement that on the return of genuine prosperity there may be no corresponding change in the drink curve.

THE UNIVERSE OF SUNS.

By Richard A. Proctor.

(*Continued from p.* 188.)

HERSCHEL then proceeds from class to class of well-marked clusters, to the most compressed and presumably the most distant clusters, and thence to globular nebulæ which are probably sidereal, but are ambiguous on account of their faintness or distance. He remarks that precisely as "in passing from faint nebulosity to the suspected sidereal condition we cannot avoid meeting with ambiguous objects," so a similar "critical situation will again occur, when from the distinctly sidereal appearance we endeavour to penetrate gradually further into space." The former sort of ambiguity results from the really ambiguous condition of the observed objects, the latter from the effects of an increase of distance.

In closing this remarkable paper, Sir W. Herschel (*then in his seventy-sixth year*) says: The extended views I have taken in this and my former papers, of the various parts that enter into the construction of the heavens, have prepared the way for a final investigation of the universal arrangement of all these celestial bodies in space; but as I am still engaged in a series of observations for ascertaining a scale whereby the extent of the universe, so far as it is possible for us to penetrate in space, may be fathomed, I shall conclude this paper by pointing out some inferences which the continuation of the action of the clustering power enables us to draw from the observations that have been given.* . . . *It is evident that if ever the Milky Way consisted of equally scattered stars, it does so no longer*," and our observations "*authorise us to anticipate the breaking up of the Milky Way, in all its minute parts*, as an inevitable consequence of *the clustering power arising out of those preponderating attractions which have been shewn to be everywhere existing in its compass.*" "Now, since the stars of the Milky Way are permanently exposed to the action of a power whereby they are irresistibly drawn into groups, we may be certain that from mere clustering stars they will be gradually compressed through successive stages of accumulation, ... till they come up to what may be called the ripening period of the globular form, and total insulation; from which it is evident that the Milky Way must be finally broken up and cease to be a stratum of scattered stars.† We may also draw a very important additional conclusion from the gradual dissolution of the Milky Way; for the state into which the incessant action of the clustering power has brought it at present, is a kind of chronometer that may be used to measure the time of its past and future existence; and although we do not know the rate of going of this mysterious chronometer, it is nevertheless certain, that *since the breaking up of the parts of the Milky Way affords a proof that it cannot last for ever, it equally bears witness that its past duration cannot be admitted to be infinite.*"

* The omitted passages are descriptive portions relating to the Milky Way, and have no special bearing on our subject.

† Herschel has already shown that it no longer consists of *equally* scattered stars; so that this sentence should not be misinterpreted to imply that he still retained the idea that the Milky Way is constituted like the part of the heavens forming our constellations.

I conceive that as the theory of 1785 marks a certain stage in Herschel's progress towards the interpretation of the sidereal universe, so the ideas enunciated in 1811 and 1814 mark another stage, which is as definitely distinguished from the subsequent work of Herschel as from the work resulting in the theory of 1785. We have in those two papers a second grand sketch of the universe as Herschel now understood its features. The sketch includes celestial objects of all known forms from the faintest nebulous haze to the most condensed star-clusters. It constitutes, to my view, notwithstanding Struve's dictum to the contrary,* a theory of the universe even more complete and precise than the theory of 1785.

I find it difficult to indicate the opinion which according to my judgment should be formed of the memoirs of 1817, 1818. These pages are full of striking passages, full of the clearest evidences of power. They have been admired and justly admired for the grandeur of the conceptions enunciated by the great astronomer. "*Heureux mortel*," exclaims Struve, "*heureux mortel que fut Herschel, de jouir à l'age de* 80 *ans d'une pénétration de l'esprit, et d'une clarté du jugement qui le firent composer les deux derniers mémoires, remplis d'une spéculation sublime et profonde!*"

While sharing to the full this sentiment of admiration for the noble papers contributed by Herschel in his 79th and 80th years, to the science he had served so earnestly and so long, it appears to me that they exhibit unmistakable traces of failing powers. It is with reluctance I touch thus on a point which I would have preferred to leave untouched did truthful dealing with my subject permit. Not indeed that it can be regarded as in any way injurious to Herschel's memory to point out that his powers were less in his 80th year than they had been a few years earlier; but that if it were possible, one would avoid to dwell on his less perfect work. It is, however, impossible to leave unnoticed theoretical considerations which have been over and over again classed with the finest generalisations of the great astronomer.

In the papers of 1817 and 1818 Herschel undertakes the consideration of the sidereal universe under an entirely new aspect. In 1785 he had been guided chiefly by the numerical relations observable among the stars in different parts of the heavens; and he had inferred from such relations the dimensions and proportions of the stellar system. In 1811 and 1814 he based his conclusions upon the aspect of the celestial objects and of various regions of the heavens,—inferring thence not merely the laws according to which the stars are distributed throughout space, but the processes by which during long past ages they have reached their present condition, as well as those which in future ages they will probably undergo. In 1817 and 1818, he took as the basis of his researches the quantity of light received from individual stars and from star groupings of various orders and endeavoured to infer thence the actual profundity of the celestial depths. The idea was not less magnificent than either of those conceptions on which he had based his former researches, nor was the work of observation carried on with less vigour and energy; but the elasticity of mind which Herschel had displayed in dealing with his observations on former occasions seems here in great degree wanting.

* "*On peut demander*," says Struve, "*pourquoi les astronomes ont-ils maintenu généralement l'ancien système sur la voie lactée, énoncé en* 1785, *quoiqu'il eût été entierèment abandonné par l'auteur lui-même, comme nous l'avons démontré. Je crois qu'il faut en chercher l'explication dans deux circonstances. C'était un système entier, imposant par la hardiesse et la précision géométrique de sa construction, et que l'auteur n'avait jamais révoqué dans sa totalité. Dans ses traités publiés depuis* 1802, *on ne rencontre que des vues partielles, mais qui suffisent, en les comparant entre elles, à comprendre l'idée finale du grand astronome.*" It appears to me after a careful study of Struve's reasoning,—that he in the first place over-estimates the precision of the theory of 1785, in the second that he under-estimates the completeness of the theory of 1811-1814, and lastly that he fails to notice sufficiently the circumstance that the papers of 1817, 1818 are distinct in all respects from Herschel's former work.

TRICYCLES IN 1884.

By John Browning,

Chairman of the London Tricycle Club.

THE TRIAL OF THE "STERLING."

My readers have kindly shown so much interest in the article I wrote a short time since on "Trying Tricycles" that I am induced to devote an article to a fuller account of the manner in which I have just been testing one of the special machines recently completed for me, and exhibited by the ingenious maker, Mr. Burdess, at the Floral Hall.

Having got the light "Sterling" from the Stanley Show all right, I at once spent a few hours in a preliminary trial of the machine. The roads were far from perfect; being heavy with recent rain, which had been falling for nearly two days previously, the narrow tyres in many places sank into them.

The best test I have found for the light running of a machine is that it shall run swiftly down a very moderate incline without working the pedals. Judged by this standard, the light "Sterling" beats any machine I have ever ridden. Again, in riding up hill, I tested it on the stiffest hill near my place, known as "Robinhood Hill." I found the machine ran so easily that I actually got half way up it, without feeling any strain, with the speed-gear on, that is, the machine running at about 50 in. As soon as I felt anything like an unpleasant resistance, I back-pedalled, thus bringing the power-gearing into action, and rolled up the rest of the hill without exerting myself in the least. Now, I have only ridden up this hill once before on my light "Humber," which weighs 15 lb. less than the "Sterling," and is geared 2 in. lower than it, and I thought that the machine would break under the strain I had to put on it, and I decided never to ride up the hill again. I should not have tried it on this machine had not my friend, Mr. Grace, told me that, being a single driver, he thought I should drive it round on the hill-side, without being able to get up it.

I could only suggest one improvement to the machine, viz., that owing to its running so lightly it might be geared 5 in. more, both for power and speed; but what would be better and cost less, would be for the machine to have a crank with 1 in. less throw—5 in. instead of 6 in. This would be a great advantage in a long day's journey, as it is found that a 6-in. throw is too long for continuous work. I shall try to get such a crank made 1-16th less in diameter, with ball rat-trap racing pedals; this will reduce the weight of the machine about 3 lb., without decreasing its strength.

I found the stearing-gear rather stiff in action; but that, I hope, will get easier with a little use.

I have made two further trials of what I will call, to distinguish the machine, the "Phantom Two-speed Sterling," under the following circumstances. Having asked a friend to go with me for a day's ride on the machine, he expressed a doubt whether so light a machine would hold together for a day if we had a hard ride. To test this, I took the machine out and rode it up and down hills for

about six miles as hard as I could ride it over the worst and roughest macadam in the hamlet of Penge, and there is not a quarter of a mile of this granite to be found but what is a disgrace to the parish, even in the best parts of it. The roads were dry and hard, in which condition granite is the most trying.

Having stood this test successfully, I then rode the machine against a 6-inch kerb twice, the second time so hard that I nearly threw myself out of the saddle, though I leant well back in anticipation of the shock. I dented the rims, but the machine remained otherwise uninjured. Another friend suggested to me, after hearing this, "Well, the machine may go very well in dry weather and on good roads (mind, I had been testing it exclusively on bad ones), but," said he, "as soon as we have rain and wet roads, then it will slip, and you will be able to do next to nothing with it."

The day after this, in the morning the roads looked as if it had rained nearly all night, and after allowing them to dry only two or three hours, in which condition they are the heaviest, becoming on clay soils pasty, I took out an excellent double-driver, on which I have done some good, hard days' work, knowing well its capabilities, and rode it exclusively over hills from Penge to the top of the Crystal Palace hill, up the Crystal Palace Park-road. I found this excessively hard work—so hard that I thought I should scarcely manage it. Then I rode back again, and without a moment's delay took out the light "Sterling" and rode it over the same ground. This was, of course, unfair to the "Sterling," as I had already tired myself—in fact I was nearly done up; yet I rode it with the most perfect ease. I do not say the work was not half as hard; it gave me the impression of not being one-quarter as hard, and though the "Sterling" was about 12 lb. lighter than the double-driver, it ran down many parts of the hill where it was not so steep without driving, while the heavier machine required driving all the way. I have now tested the machine not only severely, but unfairly in every way I can devise, and am perfectly satisfied with it except in two respects—that I wish to have a shorter crank and ball pedals.

I should not wish it to be thought that I am praising this "Sterling" unduly, but it is no exaggeration to say that, with the low-speed gearing, it is possible to rest oneself while riding up a tolerably steep hill. At the same time, I must add, that if any one wishes to ride as fast as he can, it is not the machine he should choose; for if he desires to ride at the rate of from twelve to fourteen miles an hour on a common road, the machine will not suit him; but if he will only exercise common care in riding down hills, and be satisfied with covering seven or eight miles an hour on up and down roads *easily*, then I doubt if any other machine in the market will answer his purpose equally well.

The weight of this machine with two-speed gearing is about 65 lb. The power-gearing runs as 28 in., and the speed-gearing at a little over 49 in.; both are rather too low for so light a machine. They might run from 5 in. to 6 in. higher with advantage. The wheels are 37 in. diameter only, and it is due only to the small size of these that it has been possible for the maker to produce a machine so light in weight. I wish to repeat that I attach no value to small wheels, beyond this, that their adoption enables us to make the tricycle lighter and stronger. At present, with 48-in. driving-wheels and 18 in. steering-wheels, our driving-wheels are too large and our steering-wheels are too small.

Again, our rubber tyres are too small. Frequently, manufacturers put a smaller rim and tyre on the steering-wheel than on the driving-wheels; the very reverse should be done. The rim and tyre of the small wheel should be at least one-eighth larger than those on the driving-wheels. I shall favour those machines in future which have the driving-wheels smaller and the steering-wheels larger, and in which the weight of the rider is more distributed over the three wheels, instead of being placed nearly all on the driving-wheels, and only barely enough placed on the steering-wheel to enable the rider to guide the machine.

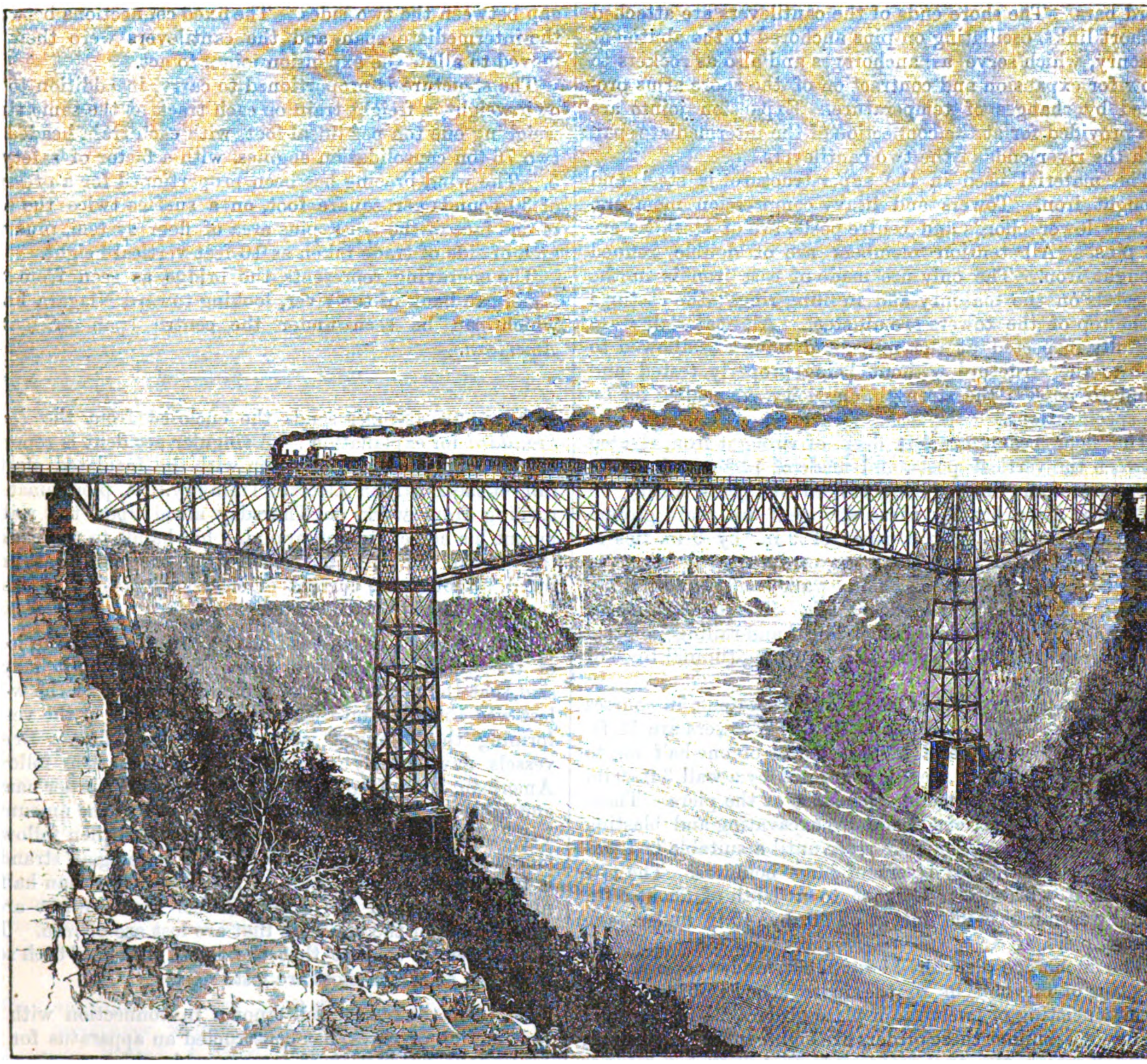
An Elephant that Reads.—Louis E. Cook, agent of Cole's Circus, tells a remarkable story that illustrates the elephant's wonderful capacity for receiving and retaining impressions, and at the same time, shows the great possibilities still offered in the direction of educating the pachyderm. "Our trainer, George Conklin," said Mr. Cook, "while in New York last winter, had several conversations with an eminent zoologist, who, among other theories, advanced this: That the elephant could be taught to read written characters. He explained that the education of the great beast had gone even further than mere feats of memory, either in the matter of language, or judging of shapes and forms. It had been trained to do work of many kinds in the East, such as piling lumber in systematical columns, and even setting the heavy stones in masonry so that expert workmen found it seldom necessary to make any changes in their position. The cunning of the elephant was well known, for the trained monster was often used to lure his wild brethren into snares, and frequently assisted in throwing and holding down the captive until the chains were placed upon him. The zoologist used other arguments of the same kind, and Conklin at last determined to make the attempt, and determine the possibility of teaching the elephant how to read. He selected 'Rajah,' a 15-year-older, for the experiment. He got a blackboard two feet long and eight inches wide, on which to inscribe the letters. He did not begin, however, by teaching the animal the alphabet. He did not seem to want to make the experiment in that way, as it might be too tedious; but, as you know, ring or trained elephants know all the words of command spoken by the trainer. They will stop when he cries 'halt,' move on when he says 'march,' run at the order to 'double-quick,' and indeed obey every word spoken to them. Well, Conklin hit upon this plan. He took 'Rajah' into the ring at least once every day, and writing the word 'march' in Roman characters about five or six inches long on the board, placed it before the animal's eyes, and after allowing him to scan it, then, pointing to the white letters, shouted the order 'march,' which, of course, was complied with. He found by writing the letters while the board was held before the elephant was a surer way of attracting the animal's attention to the characters, so he now writes in this way. Rajah soon grew accustomed to the board and the letters, and now very seldom makes a mistake when a written command is presented to him. He will march the moment the last letter is finished, halt when the trainer shows him the word, and, indeed, recognises every inscription of this kind Conklin places on the board. The elephant will not be introduced into the ring in this new act until next season, by which time the trainer thinks it possible to educate the whole herd in reading, so that the words of command may be given in writing on a large blackboard. Having succeeded so well in teaching "Rajah" to read words, he will now try to advance backward by teaching him the alphabet, and I myself think it possible that the elephant, which is quick and intelligent, may be taught by means of lettered blocks to spell the words that he is familiar with in the ring."—*New Orleans Times Democrat.*

CANTILEVER BRIDGE OVER NIAGARA.

THIS double track railroad bridge, completed within the past few days, was designed to connect the New York Central and Michigan Central Railroads. It is located about 330 ft. above the old railroad suspension bridge, spanning a chasm 870 ft. wide between the bluffs and over 200 ft. deep. The banks of the river are formed of masses of broken rocks and immense boulders reaching up to within about 60 ft. of the level land.

in charge of the work, and Edmund Hayes, engineer of the Central Bridge Works.

The structure consists of two immense steel towers, 132 ft. $6\frac{1}{2}$ in. high, resting on stone piers 39 ft. high. Each of these towers supports a cantilever 395 ft. $2\frac{5}{16}$ in. long. One end of each tower rests upon an abutment at the edge of the bluff, while the other end extends out over the river. The shore ends of the cantilevers are anchored to the abutment masonry or anchorage piers, and both river arms are connected by an intermediate span of 120 ft.



As the foaming rapids at this point rendered it impossible to build piers in the river or erect temporary supports, it was necessary to design a structure which could be erected without such false work; to attain this end a bridge of the cantilever type was adopted which would be self-supporting during erection. The principle of the cantilever is that of a beam supported at or near its centre, with arms extending both ways, one arm being held down by an anchorage or counterweight so that the load on the overhanging arm produces an uplifting force in the opposite end which is resisted by the counterweight. The designs of this structure were worked out jointly by C. C. Schneider, chief engineer

which is suspended from the extreme ends of the river arms. The total length of the bridge proper is 910 ft. $4\frac{5}{8}$ in. between the centres of the anchorage piers, the clear span between towers being 470 ft. The height from surface of water to base of rail is 239 ft.

The towers are braced steel structures, containing four columns each, which are made up of plates and angles riveted together, braced with horizontal struts and tie-rods. The batter of the columns at right angles to the centre line of the bridge is 1 in 8, and parallel to the centre line 1 in 24.

The trusses are two in number, 28 ft. apart between

centres; the various members being connected with steel pins, $7\frac{1}{2}$ in., $6\frac{3}{4}$ in., and $5\frac{3}{8}$ in. in diameter, turned accurately so as to fit the bored pin-holes within 1-64th of an inch. The depth of the cantilever trusses over the towers is 56 ft., and at the shore ends 21 ft., and at the river ends 26 ft. The lower chords and centreposts are made of plates and angles riveted together and latticed, the intermediate posts being of 12 by 15-in. channels, latticed. The upper chords of the cantilevers are 8-in. eye bars, the shore arm having a compression member 18 in. deep composed of plates and angles packed between the chord bars. The shore ends of the cantilevers are attached to short links, oscillating on pins anchored to the abutment masonry, which serve as anchorages and also as rockers to allow for expansion and contraction of the shore arms produced by changes of temperature. Expansion joints are also provided for at the connection of the intermediate span with the river ends of the two cantilevers.

The material used in the superstructure is steel and wrought iron. Towers and heavy compression members, such as lower chords and centre posts, are of steel, as are all pins. All tension members are of double refined wrought iron. The only use made of cast iron is in the pedestals on the masonry and in filing rings; the castings at the top of the towers are all steel. All materials were carefully inspected at the mills, and none was allowed to go into the structure without being properly tested and found to possess the strength, elasticity, &c., called for by the specifications.

The floor beams are 4 ft. deep, of wrought iron, riveted between the vertical posts, and made of plates and angles. There are four lines of longitudinal stringers, resting on top of the floor beams; these stringers are plate girders $2\frac{1}{2}$ ft. deep. The track consists of 9 in. by 9 in. ties of white oak, spaced 18 in. between centres, every other tie projecting to support a plank walk and hand-railing, making the width of the floor 32 ft. The guard timbers are 8 in. by 8 in. of white oak. The hand-railing consists of cast-iron posts, 6 ft. apart, and four longitudinal lines of $1\frac{1}{4}$ in. gas piping.

All masonry is built of Queenstown limestone, in courses of 2 ft. rise. The piers supporting the towers are 12 ft. square under coping, and have a batter of one-half in. to the foot; each pair of piers is connected by a wall 3 ft. 9 in. thick on top, and battering the same as the piers. These piers are on foundations made by excavating and blasting the rock on the banks of the river until a suitable bed was reached, consisting of layers of huge boulders. The pits were filled with beton Coignet to a depth of about 8 ft., thereby forming beton blocks of 20 by 45 ft. under each pair of piers.

The anchorage piers are 11 by $37\frac{1}{2}$ ft. under coping, with a batter of one-half in. to the foot. They rest on a platform consisting of twelve iron plate girders, $2\frac{1}{2}$ ft. deep and 36 ft. long; under these girders are eighteen 15 in. I-beams, through which the anchorage rods pass in such a manner as to distribute the pressure over the entire mass of masonry. Each anchorage pier contains 460 cubic yards of masonry, weighing 2,000,000 pounds. As the maximum uplifting force from the cantilevers under the most unfavourable position of the load is only 678,000 pounds, it will be seen that this upward force is amply counter balanced.

One of the most interesting features of this important work is the erection of the river arms of the cantilevers. After the towers were built the shore arms of the cantilevers were erected on false work in the usual way; after the shore arms had been placed in position and anchored down to the anchorage piers, the river arms were built out from the towers toward the river, one panel or section at a time, by means of great travelling derricks designed and constructed specially for the purpose and provided with steam power. After one panel had been built and its bracing adjusted, the traveller was moved forward and another panel erected. Thus the work progressed until the ends of the cantilevers were reached. The intermediate span of 120 feet was so designed, with bottom compression members, that it, too, could be built out from the river arms of the completed cantilevers until the middle panel was reached, which was accurately fitted to close the remaining gap between the two sides. The fixed connections between the intermediate span and the cantilevers were then removed to allow the expansion joints to act.

The structure is proportioned to carry, in addition to its own weight, a freight train on each track at the same time, weighing one ton per lineal foot, with each train headed by two 76-ton consolidation engines, with a factor of safety of 5. The wind bracing has been proportioned for a pressure of 30 pounds per square foot, on a surface twice the area of one face of the truss, plus area of floor system, plus the area of side of trade taken as 10 feet vertical height.

The engraving represents the bridge as seen from the American bank of the river, looking toward Niagara Falls, which can be seen under the centre span.—*Scientific American.*

A correspondent sends the enclosed from the *Derry Standard* for explanation:—A singular accident is reported to have happened in connection with the insulated electric rail on the Portrush Electric Tramway. A ploughman returning from work on Thursday stood upon the rail to mount his horse, and, on applying his hands to the back of the animal, the brute fell dead, while the man was uninjured, although the current of electricity must have passed through his body to the horse.

The Bureau Veritas has issued a list of the marine losses of the world for the past year. Summarised, it may be said that the net tonnage of the sailing-vessels of the world lost was 458,798, and that more than one-half of the vessels forming this tonnage were lost by stranding. British vessels gave nearly one-half of the loss; then followed American, Norwegian, German, Italian, in the order named, and others. Of steamers the losses were to the amount of 162,217 tons, five-eighths being British, and then following German, French, and American; and here again stranding is the cause of a large part of the loss—more than half in number. Most of the sailing-vessels that are lost are of wood; most of the steamers that are lost are of iron. This would seem to suggest that the oldest vessels of both sorts are those of which most are lost.

M. Cailletet, so well known in connection with the liquefaction of gases, has constructed an apparatus for the continuous production of intense cold, which consists of a closed steel cylinder containing a coil of copper pipe which projects from each end of the cylinder. Two copper tubes are also screwed into the cylinder, and one of these communicates with the mercurial piston pump already used by Cailletet, while the other receives the ethylene which has been compressed by the pump and cooled by methyl chloride. By this arrangement he forms a circuit in which the same quantity of condensed ethylene is repeatedly evaporated in the copper coil, producing intense cold, and then compressed again by the pump being sufficiently cooled with methyl chloride and ready for evaporation again. This process goes on as long as the sucking and compressing pumps are working.

Gossip.

It is probably scarce necessary to explain that the figures illustrating the safe-with-care and the safe-with or without-care forms of limelight illustration in last week's Gossip, are not pictures of any instruments actually in existence. The fact is I drew figures to be made up with type as here shown. But blocks were actually cut, and entirely novel

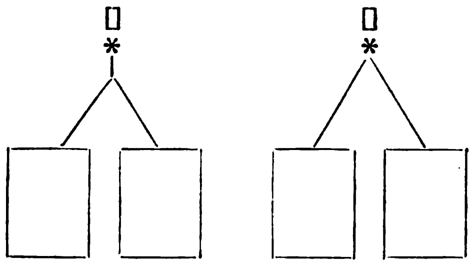


Fig. 1. Fig. 2.

forms of lime-light apparatus were thus elaborated. The principle is the same however.

It is evident, from the comparatively thin audiences at the Memorial Hall—though nothing could be kindlier than their greeting—that the City in the evening is not a good place for scientific lectures.

The Editor reminds readers that he lectures on the 7th and 9th inst. at the Crystal Palace, the subjects being "The Life of Worlds" and "The Moon."

The *Globe* discusses the question whether the opinion of us indicated by the French saying "As original as an Englishman" is justified by British originality. The medicinal properties of a man who has been pronounced "a cure" may next be discussed.

One has only to take up the first average translation of a French work to come across multitudes of similar misapprehensions of the meaning of French words and phrases. I open at random, as I write, "John Bull and his Island," and the first sentence I read begins "That which," where the corresponding French sentence begins "*Ce qui.*" There is a considerable difference between the two expressions. For instance, when a Frenchman says, "*Ce qui me plait,*" he does not mean "*the* thing which pleases me," or "that which pleases me," but "a thing which pleases me."

Can anything be much more amusing than the way in which newspaper writers speak of the rowing in the University Boats? I learn from one account that Cambridge would be a good crew if some of the crew did not miss the beginning and if others could be persuaded to pull their weights; "seven" cannot keep time, and so puts "six" out; another rows too deep; the crew as a lot do a deal of splashing; it is far inferior to last year's crew (of which the same story was told, as of every crew since the races began); but it may improve. As for the Oxford crew, some accounts would suggest (to any one who did not know the real worth of these reports), that a clumsier set of oarsmen were never seen; yet their strength and "life" may save them, if they will but attend to newspaper hints; true, their stroke rows light, barely covering his blade, and there are others who unless watched will shirk their work; but though far inferior to last year's crew (as usual) the Oxford men are a promising lot. (Perhaps in ten years' time some of them will have learned how to row.) If these scribes knew anything of the way in which University crews are selected they would not write—perhaps—so much nonsense. Every man has his record, and has proved himself in many hard-fought contests a stalwart and skilful oarsman. The worst of the sixteen is the pride of his college first boat, and the worst oar in a fourth or fifth college boat at Cambridge or in a torpid at Oxford would scarcely ever display the gross faults of style, and would *never* display the laziness and want of pluck so complacently attributed to the picked oarsmen of the leading boats by ignorant reporters. One of these it was who a few years ago, seeing the Cambridge boat pass when a strong side wind brought the stroke side down, announced that all the men on the stroke side brought in their hands too low, the bow side showing as grievous a fault of the opposite kind! And of like worth are nine-tenths of their criticisms.

But the most preposterous mistake of all was made a few days back by a writer who said that with all the contradictory advice given them, the crews could hardly be expected to improve! As if a man in either boat was likely to pay the least attention to newspaper suggestions!

I have received a great number of communications in reference to the pregnant remarks of my esteemed correspondent "N. W." on the subject of scientific societies. The evil to which he referred, the election to such societies of men without scientific or literary standing, even it may be without scientific or literary attainments, is unquestionably an evil and a crying one. But I know not how it may be diminished. Examinations, as suggested by one correspondent, are in reality quite out of the question. The whole matter turns and must turn on the qualities of the members themselves of such societies, and thus the evil instead of being one which brings its own remedy is one which increases itself.

Still it may be well to point out to the genuine members—the really working members—of these societies, that they at least should refrain from the work of lowering the societies to which they belong by nominating those who are not the right sort of men for membership. I find some have positively the notion that in increasing the number of fellows or members they are increasing the weight and influence of their society. They may increase the balance at the society's bankers, but in all other respects a society is lowered when men are brought into it who are not capable of advancing the work for which it was started. The clause which all self-respecting societies and clubs introduce into their bye-laws, that none shall be nominated except on personal knowledge, becomes a mere dead letter, if personal knowledge be not understood to signify something more than mere personal acquaintanceship. No man should be nominated for any society of whom it may not be said that he is known to be interested in the purpose, and capable of advancing the work, for which the society was founded. Otherwise a society is almost as much degraded by its electoral system, as a body which elects men without even ascertaining that they desire election,—as in the case of my own election to the St. George's Chess Club which I cited some time ago.

But the matter may be viewed from the other side also. Men who think of joining a society should consider whether

they are really likely to prove efficient members, and whether they really have a right to put themselves forward for election. This should be so much a point of honour that even when election has been granted unsought a man should reflect before he accepts it. He should not, merely because elected, allow himself to be enrolled on the society's lists, unless he is satisfied that it is right and fitting he should be. If not so satisfied he may pay, if he will, whatever fees his election involves, and so avoid the unpleasantness of flinging back an unsought favour: but he should take the earliest opportunity of withdrawing.

In passing, I may note that so far as my own experience is concerned, even the paying of fees in such a case is undesirable. For I have recently learned from the Secretary of the St. George's Chess Club that *had I omitted that ceremony* an expression of regret for my undesired election would have been tendered me (somehow I regarded this as my due in any case), but that as I had paid their fees the Club is no longer responsible for that act of courtesy. But this may be simply Mr. Minchin's idiosyncrasy: after all it is a mere question of taste and breeding.

If a bad system of election to Societies is to be deprecated, much more may exception be taken to a system by which unsuitable officers are selected. It has been pointed out by our valued contributor, "F.R.A.S.," in a letter to a contemporary, that among the members of the Council of the Royal Astronomical Society are positively several who have contributed nothing to the proceedings of the Society *for four years!* They must surely have been elected by mistake, and without having been consulted; for to have remained so long without contributing to the Society's proceedings is practically equivalent to saying that either one does not or one cannot take any interest at all in the Society's work and purpose. I know at any rate that this has been the reason for my own failure to contribute for several years (since the internal squabbles began) to the Society's proceedings or to attend meetings. No one who so withdraws from a Society's work should be regarded as eligible to office of any sort whatever within the Society.

Reviews.

SOME BOOKS ON OUR TABLE.

George Birkbeck, the Pioneer of Popular Education. By J. G. Godard. (London: Bemrose & Sons. 1884.) —To the present generation Dr. Birkbeck represents but little more than the "*nominis umbra*" of which Lucan speaks. And yet, in the words of Mr. Godard's title, he may well be regarded as the pioneer of that education which is becoming daily more widely diffused among the masses. His latest biographer tells us how the quondam "Mechanics' (now Birkbeck) Institution" in Chancery-lane had its inception in lectures given to working men by George Birkbeck, at the time he was Professor of Physics in the Andersonian Institute—at present absurdly called the Andersonian University—in Glasgow; and gives the history of the struggles of what has now grown into a great educational establishment from the date when its earliest lectures were delivered in a chapel in Monkwell-street, through the erection of its own original proper house in Southampton-buildings, down to the date of the laying of the foundation-stone of the Birkbeck Literary and Scientific Institution by the Duke of Albany in 1883. It is interesting and instructive to follow the Institution through its struggles, and to learn how successfully it surmounted the difficulties it had to face both from pecuniary troubles within and violent opposition from without. As a record of the educational movement which originated during the first quarter of the present century, Mr. Godard's book will repay perusal.

German Conversation Grammar. By I. Sydon. (London: W. Kent & Co. 1883.)—As far as a foreign language can be self-taught, we are disposed to regard Herr Sydon's manual for use to that end with very considerable favour. His method is simple, and his examples chosen in a way likely to develop the conversational powers of the student. The beginner will find himself talking German *pari passu* with his acquisition of the rudiments of grammar. In fact, any one who will conscientiously master the contents of the work now before us should have no difficulty in making himself thoroughly intelligible on a maiden tour in Germany.

Oriental Carpets. By Herbert Coxon. (London: T. Fisher Unwin.) 1884. Now that so-called "Tapestry" carpets are relegated to lodging-houses and the saloons of sea-going steamers; and when in recently furnished houses the time-honoured "Brussels" of our fathers is giving way to Turkish, Persian, and Indian carpets of more or less authenticity, Mr. Coxon's book will prove interesting to those who wish to know something of the origin of what (while treated as almost a necessity) is, at present, a somewhat costly luxury. The author of this little book, who appears to be in the carpet-trade at Newcastle-on-Tyne, travelled during the year 1883, through Odessa, across the Black Sea, and from Batoum by the New Baku Railway, right down to the shores of the Caspian, diverging when he reached them to visit Resht and Tabreez, and crossing to Kramovodsk, to open up a direct trade in Oriental carpets with the North of England. He tells us of his wanderings in a very unpretentious and readable style; carefully avoiding the two temptations which seem to beset the majority of authors of such books as his—either to be fine or funny. His work will while away an idle half-hour in a train pleasantly enough.

Universal Attraction: Its Relation to the Chemical Elements. By W. H. Sharp. (Edinburgh: E. & S. Livingstone.)—An introduction should be arranged immediately (or sooner) between the author of this work and Mr. Newton Crosland. One of two results would probably accrue: The one being the formation of a New Patent Gravitation Association (Limited); the other, a conflict akin to the feline one in a south-eastern Irish county, which has passed into a proverb.

Watch and Clockmaker's Handbook, &c. By F. J. Britten. (London: W. Kent & Co. 1884.)—Ostensibly addressed to and intended for the Clock and Watchmaking Trades, Mr. Britten's excellent book may be read with interest and advantage by every possessor of a watch or clock in the kingdom. It is arranged in the form of a dictionary, for convenience of reference, and comprises complete and well-illustrated descriptions of every part of a watch and clock, and of the tools used in their construction and repair, short biographies of eminent chronometer-makers, a dissertation on time and the transit instrument, and a map of cognate information. It forms a succinct encyclopædia of horology, and, as such, we can strongly recommend it.

Mineralogy (Systematic and Descriptive). By J. H. Collins, F.G.S. (William Collins & Co., London and Glasgow.)—This, the second volume of Collins's Mineralogy, contains a complete and practically exhaustive description of all known minerals arranged upon the chemical system.

No less than 420 woodcuts illustrate the account of the various substances treated of, and as a compendious hand-book of the subject on which it treats Mr. Collins's volume is not likely to be soon superseded.

Notes on School Management. By GEORGE COLLINS. (London: Moffatt & Paige.)—Intended on a *vade-mecum* for the school teacher, and well fulfilling the end its author had in view in compiling it, this is a book which will also be found useful by school managers; and, in fact, by all interested in education. The table in pp. 150 and 151 may be studied by those who wish to understand what the promoters of compulsory primary education are aiming at; and to estimate how far the study of the subjects included in the higher standards is calculated to fit the rising generation of the working classes for manual labour. The remarks on corporal punishment on p. 61 appear sensible and judicious.

Botanical Micro-Chemistry. By V. A. POULSEN. Translated by William Trelease. (London: Trübner & Co. 1884.)—Since the invention of the achromatic microscope-objective by Mr. Joseph Jackson Lister in 1829, down to the present hour, the march of microscopy through the field of natural science has been a sequence of triumphs. Were it possible to eliminate the discoveries made with the microscope during the last fifty years from the record of intellectual progress, the sciences of zoology, botany, physiology, and animal and vegetable histology, would be reduced to a comparatively inchoate and rudimentary condition. Time was, and that within the memory of men now living, when the height of the microscopist's ambition was to be able to exhibit with effect some fifty to a hundred "objects," derived from the animal, vegetable, and mineral kingdoms. Now, so specialised has microscopical work become, that the very instrument itself is modified to suit particular branches of research. Chemistry, too, has been pressed into the service of the observer; and various re-agents are familiarly employed in the investigation of minute organic structures. The work of MM. Poulsen and Trelease gives full instructions for the preparation and use of the most approved re-agents employed in the study of Vegetable Histology. It is well written, contains the latest information on the subject of which it treats, and should be possessed and read by all who are interested in the minute structure of plants as developed by the microscope.

Medical Fashions in the Nineteenth Century. By EDWARD T. TIBBITS, M.D. Lond. (London: H. K. Lewis, 1884.)—This is an amusing book, and, we fear, in the main, as true as it is amusing. It is not every day that a physician of standing has the temerity to show the outside public how very empirical an art medicine really is. Most elderly men must recollect the extraordinary change which has taken place in the practice of bleeding within their remembrance, and must congratulate themselves on the practical disappearance of the lancet from the doctor's impedimenta. So, again, with the employment of calomel. These, and the swing of the pendulum from the enormous doses of Dr. Elliotson to the (algebraically speaking) "minus quantities" of the homœopaths, hydropathy, mesmerism, the use—and abuse—of alcohol and anæsthetics, chloral, digitalis, mineral waters, and health-resorts, all furnish our author with materials for his *exposé*. So far, we are in the fullest accordance with Dr. Tibbits. Whether, though, his dicta on the discoveries of Lister and Pasteur are equally sound may, we think, reasonably be a matter for discussion. The inoculation experiments of Pasteur go very far to show that specific forms of disease bacilli actually do exist. Would that they could be got rid of by sneering at them.

Aids to Physiology. By B. THOMPSON LOWNE, F.R.C.S. (London: Baillière, Tindall, & Cox.) This really excellent manual is specially designed for students preparing for examination. It presupposes the attendance at a course of lectures, or the study of one of the larger text-books, and summarises in a form at once succinct and intelligible the results thus acquired. Certainly the student who will master Mr. Lowne's capital little book, need have no fear of being "plucked" by the most exacting examiner. It must not, however, be supposed that it is only useful and interesting to those preparing for a medical career. It is so plainly written and, where necessary, illustrated, that it may be read with profit by anyone and everyone who cares to know anything of the working of that most marvellous mechanism, his own body.

ERRATA.—Page 199, column 2, "Mr. J. W. Ward" should be "Mr. I. (Isaac) W. Ward"; and "Mr. Mark," "Mr. Marth."

"Let Knowledge grow from more to more."—ALFRED TENNYSON.

Only a small proportion of Letters received can possibly be inserted. Correspondents must not be offended, therefore, should their letters not appear.

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STRANGE RESUSCITATION.

[1169]—With reference to a letter signed "John Hame," respecting the plaice which recovered after being frozen for twenty-four hours, and which he appears suprised at and asks for an explanation from the readers of KNOWLEDGE, I may perhaps say that fish, in common with reptiles, "both being cold-blooded animals," generally become more or less torpid if subjected to cold, and recover when warmth is applied to them, and not only after twenty-four hours, but after about twenty-four weeks—"a winter." Perhaps with respect to the plaice in question, the warmth of his hands, as well as the shock of falling on the deck, may have brought about resuscitation. I don't know whether he will think this a sufficient explanation, but I think it would have been more surprising if it had been a warm-blooded animal.

Boston, Feb. 26. J. H. GARFIT.

RED SUNSETS AT THE CAPE.

[1170]—It appears to me that it is high time we should have some more authoritative and logical explanation of the present (not "late") unusual sunsets than any which have yet appeared. These extraordinary crimson sunsets have been constant here ever since the first week in October last, and show as yet no sign of cessation. The crimson colour on clear evenings is not confined to the western horizon, but shades slightly to the east, and also towards the zenith. Last night it plainly included Venus, Aldebaran, Jupiter, Mars, Saturn, Leo, the Centaur, and Southern Cross, and the red tinge extended as a pale violet even to the zenith, and such has been the normal appearance on every clear evening here since the date above mentioned.

That this phenomenon, extending as it appears to do over the whole earth, can be referred to the explosion in the Straits of Sunda, seems simply impossible; and yet nothing save hypothetical explanations have as yet appeared. Those basing it upon meteoro-

logical states of the weather are quite untenable, for no identical state of the weather could exist all over the globe at once; and I question whether at any moment, since the creation, the state of England and of the diamond fields of Griqualand were hygrometrically or electrically the same. We want spectroscopic and polariscopic observations to account for this world-wide phenomenon.

It is a pity there exists no astronomical observations in this part of the world, an elevation of some 5,000 ft. above the sea, and a climate where there is never any dew, save for a few days after rain, and where ninety nights out of a hundred are cloudless, are circumstances giving advantages for research in physical astronomy not easily attainable elsewhere. Our nearest neighbour, Sirius, crossing nearly our zenith, might tell us something if questioned here.—Yours faithfully, FRANCIS H. S. ORPEN, F.R.A.S.

St. Clair, Douglas, Cape Colony,
Feb. 26, 1884.

VALUE OF THE PIECES AT CHESS.

[1171]—Page 209, March 28, you say, "The Chess Column is meant for home Chess players." May I suggest that a table of the *relative value of the Chess pieces* would be instructive and interesting to many? [If it could be trusted.—R. P.]

I will endeavour to explain myself. Suppose the sixteen pieces to be collectively = 1,000, what would be considered the relative value of each?—*e.g.* :—

K	=	0	KP	=	30
Q	=	250	QP	=	30
KR	=	125	KBP	=	22
QR	=	120	QBP	=	22
KB	=	90	KKtP	=	18
QB	=	85	QKtP	=	18
KKt	=	80	KRP	=	15
QKt	=	80	QRP	=	15
					1000

C. W.

BLUE SUN.

[1172]—Would you kindly remark on the following difficulty:— Fine particles scatter small blue waves, but allow the larger red waves to go on their course—this explains the blue colour of the sky, also the red colour of the setting sun. According to this explanation, the sunny side of a fine dust-cloud should be blue, and the sun seen *through* the cloud should be *red*, not blue. L. S.

[Dust of a certain degree of fineness would let the smaller waves through and stop the larger.—R. P.]

DARK APPEARANCE OF JUPITER'S SATELLITE IV.

[1173]—I have noticed the frequent remarks of "F.R.A.S.," in his "Face of the Sky," about Jupiter's outer satellites having sometimes been seen as dark bodies when in transit, but I never was fortunate enough to witness anything of the kind until last night (March 12), when there was a transit of IV., commencing at 7.41, and ending at 12.8. I did not witness either the actual ingress or egress, as the sky, unfortunately, was overcast here at both periods, but as it was very clear in the interval I observed, with my 3-in. telescope the greater part of the transit, and during the whole of the time that I observed it the satellite was distinctly visible as a very dark spot, like a shadow, travelling slowly along that very bright part of Jupiter's disc just above; that is, south of the great dark belt. It seemed a strange thing seeing a body which ordinarily is so bright, appearing so dark, and I should like to know how it is accounted for, if indeed it ever has been accounted for. The idea struck me that it might possibly be simply a case of contrast—that is, that the satellite, or at all events the side of it which was then turned towards us, might from some cause or other be so faintly illuminated as to appear dark in contrast with the very bright part of the planet on which it was superposed, and yet be sufficiently illuminated to appear bright in contrast with the dark background of the sky, when not in transit. How far this theory is from being correct you will have more means of knowing than I have.—Yours, &c., EXCELSIOR.

Huddersfield, March 13, 1884.

[That seems unquestionably the true explanation.—R. P.]

VISION.

[1174]—May I inform "Cosmopolitan," (Letter 1147, March 14,) that, although all-seeing is intracranial, there are two kinds of it, objective and subjective—orthoscopic and pseudoscopic. Conscious, intelligent seeing does not take place in the eye, which is only the camera to collect information, but is really a sensation in the frontal lobes of the brain, caused by the vibrations of the optic nerve fibres, communicated by them through several ganglionic centres, of various functions, to the frontal lobes.

Ordinarily light, reflected from surrounding objects, is the normal agent which sets the visual apparatus and machinery working, and then vision is objective, orthoscopic, giving true internal reports and information of external objects and circumstances, sufficient for our safety, edification, and amusement.

But under abnormal circumstances, other *stimuli* than light, even of internal origination, such as irritation, magnetical currents, or congestion of the optic ganglia, &c., may give rise to optic-nerve vibrations, and cause visual impressions, without the existence of any corresponding external object. Vision then is no longer objective, but subjective or pseudoscopic. Such are the horrible visions of the patient in *delirium tremens*, fever, &c. I am glad that optics are to take a more prominent part in KNOWLEDGE; they are worthy of its pages. WILLIAM WILSON, M.A., LL.D.

STRANGE PHENOMENON.

[1175]—The light seen by Mr. Rogers was clearly that of a tolerable-sized meteor, which, for some reason, Mr. Rogers did not see. C. R. BREE, M.D.

TRICYCLISTS AND THE PARKS.

[1176]—The riders of tricycles, whether for pleasure, health, or profit, are forbidden the use of every park in the Metropolis save Finsbury Park. Yet nothing but prejudice can be urged against their right to use at least those parts of the parks now open to hackney carriages. The tricycle, even in the hands of a novice, is more under the rider's control than any vehicle drawn by a horse. Pedestrians have, therefore, nothing to fear from the tricyclist, neither can it be reasonably contended that an equestrian within the park railings would incur greater danger of his horse shying at a passing tricycle than he now does outside them.

F. S. COBB, Hon. Sec. T. U.

PLANETARY MOTIONS AND THE CONSERVATION OF ENERGY.

[1177]—In your number of March 7, Vol. V., No. 123, you allude to the difficulties of understanding motion in an elliptical orbit and explain the general principle of it very clearly, but can you as easily clear up the difficulty of reconciling the facts of the revolution of the earth with the doctrine of the conservation of energy? This doctrine teaches that there can be no gain of energy without an equivalent loss in the source from which it came, nor a loss without a corresponding gain. But the planets in their elliptical courses are gaining or losing momentum every hour, and the increase in the energy of momentum of the earth between June and December must be enormous; this increase, however, though due to the gravitation of the earth and sun, involves no loss of this energy of gravitation in them, which remains, as is supposed, a constant quantity.

Though I do not doubt the absolute exactness of the law of the conservation of energy, for the old Greek axiom, "Naught comes from nothing and naught to nothing turns," is sufficient proof of it, the empirical evidence in respect to motion of all kinds derived from gravitation seems to me defective; and, were I not afraid of intruding too much upon your space, I would give other instances to illustrate my meaning. J. C. MURRAY AYNSLEY.

[This is no instance. In the distance between two bodies there resides potential energy. In planetary movements the distance diminishes as the momentum increases, and *vice versâ*.—R. P.]

STRANGE DREAMS, &c.

[1178]—May I ask the recent contributors to KNOWLEDGE of the "strange dreams" and "ghostly visitants" incidents if they would kindly, by sending me their addresses, enable me to communicate with them on the subject?

Other readers of your articles on "Ghosts and Goblins" may have incidents to narrate similar to those given in accounts 1131, 1143, 1144; and I should like to draw the attention of these to the Society for Psychical Research (President, Prof. Henry Sidgwick, Cambridge), one of the objects of which is to systematise such narratives, with the view of obtaining some satisfactory theory concerning them.

We should be specially glad if any contributors on these subjects could favour us by forwarding to myself their names and addresses, not for publication (except by their express permission), but that the evidence for such narratives may be made as nearly complete as possible. Stories relating to premonitory presentiments and dreams, apparitions at time of death, haunted houses, &c., will be thankfully received by the Committee. RICHARD HODGSON,

St. John's College, Cambridge (Cambridge Branch S.P.R.).

LETTERS RECEIVED.

Senex.—It would seem that you unconsciously use the right eye only for distant objects.—C. R. Bree.—Hugh McMaster. The moons of Mars were discovered in 1877.—W. F. Jackson. Indexes have been published systematically.—W. G. Woollcombe. Fear cannot reach Mr. Luxmore.—Alfred T. Jenkins. Atmospheric pressure at any point depends on total weight of atmosphere above that point. You have only to go high enough to get the pressure low enough, even in the solar atmosphere. Will certainly try to maintain the independent character of the articles, &c. Many fail to note as you do the large part that plays in increasing cost of production. It was a *great* mistake in setting the price of Knowledge originally at twopence. My own idea was always for the present price.—J. L. You must surely have observed under very unfavourable conditions. All that "F.R.A.S." describes he has actually seen with three inches of aperture.—T. P. Battersby. It is really not our fault if some choose to consider that certain scientific subjects must only be dealt with from a religious standpoint.—C. T. C. Woad. Many thanks.—G. G. H. Subject is interesting and may have its turn.—H. Lewin. The idea has been mooted before, but there are serious difficulties.—W. T. Bell. (1) The buoyancy of a body is no greater in twenty feet than in ten feet of water: but precisely the same. (2) The sun exerts no appreciable influence on a burning fire.—A Beginner. For your first problem, note that you have not taken the difference of longitude into account,—as you should have done since local time not Greenwich time is in question. Bring London under the brass meridian, and measure off 90 degrees along the equator on either side of the point of the equator thus brought under the meridian. Take a string round through these points and the points on the arctic and antarctic circles uppermost and lowest. It will show you the equal arcs on the latitude parallel of London from sunrise to noon and from noon to sunset. Now do the like for Dublin, or if you prefer it for a point north of London and in the latitude of Dublin. (This will save trouble as the same position of string and globe will serve.) You will find the arcs for Dublin longer than those for London. (2) In the other problem you inadvertently wrote a cylinder where axis passes through the centre of the sphere. Flammarion's illustration is decidedly incorrect. Your best plan is to consider a small cylinder only, in which case the curve of intersection is appreciably an ellipse, of constant minor axis wherever the cylinder meets the sphere, but of major axis (and area) varying as the cosine of the angle, between the axis of the cylinder and the vertical to the sphere at the centre of the ellipse.—Alphard. Thanks.—Peccavi. Who can account for the vagaries of reporters? Have nothing to do with the arrangements made for cutting the paper or leaving it uncut. Quite differ from you in opinion as to the suitability of the papers you mention; to say they are out of place in a scientific journal is to beg the whole question, which is whether the rules discussed have or have not their scientific significance.—J. H. Garfit. Thanks.—W. Poole. Never give answers for one person under pretence of answering another.— One Who has Passed His Little Go. Probably the fault is that pipe enters tank too near the top. Theoretically pressure at B″ ought to be the same, bar effects of friction, whether pipe enters tank near top or near bottom; but practically friction is an important factor.—E. Anderson. Thanks very much for the cuttings and the Euclid triangelic. The answer about the Road and the other R is astronomically beautiful.—A. P. Williams. The moons of Mars are *very* little ones: the largest cannot be much more than one two-millionth of our own in size. You might try Guillemin's Heavens.—Frank M. Many thanks; but was away from town that day.

Royal Victoria Coffee Hall, Waterloo-road, S.E.—The fourth lecture of the Gilchrist Penny Science Lectures was delivered on Tuesday evening, 25th inst., by Mr. Edward Clodd, on "The Working Man 100,000 years ago." The lecturer began by referring to the scanty knowledge that existed till the last half century about the races living in Britain before and up to the time of the Roman invasion. For convenience, the time from the unknown date of man's appearance to the Christian era is divided into three Ages, those of the Old Stone Age, the Newer Stone Age, and the Age of Bronze or Iron. Traces of the Newer Stone Age are found on surface remains, and witness to a race unlike the Kelts but with whom they mixed. Remains of the earlier or Old Stone Age are found in the mud dredged from the shallow North Sea, in the Thames gravel, in old river gravel called drift, and in lime-stone caverns. Mr. Clodd justified his title by briefly summarising the proof of the enormous age of the deposits in caves and the older Drift, in which worked flint tools and weapons have been found by thousands, witnessing to the presence of man on the earth not merely 100,000 years ago, but probably 500,000 years ago.

Our Paradox Column.

THE FLAT EARTH AND HER MOULDER.

By H. Ossipoff Wolfson.

II.

It was in September of last year that I had occasion to make the acquaintance of the wonderful philosopher who styles himself "Parallax." He is passionately fond of his *nom de plume*, and we shall in so far fall in with his wishes by calling him so, although the reader must bear in mind that this is a pseudonym of Rowbotham, which Rowbotham is no other than Goulden, *alias* Tryon, the same individual who at present appears before the public as Dr. Birley. A man who dodges about by different names must have some motive or other for doing so, and we outsiders feel justified to regard such proceedings with a certain amount of suspicion. Men who act rightly carry their names of their fathers to the grave, and are generally proud of them, which they should be, if there has nothing occurred to stain them.

When I made the acquaintance of the many-headed eagle, I knew nothing beyond his *nom de plume*, and his present name. Our first interview has led to many others, and in the result, he succeeded in so far gaining my confidence as to induce me to entirely embrace his cause, and to endorse his teachings. I am here forced to pay him a reluctant compliment, which I have no doubt will very much flatter my old friend. His manners were engaging, and his demeanour self-possessed; and, although intermingled with a certain amount of vulgarity, you would feel as if this was a mere eccentricity of an old man, and far from creating a prejudice against him in your mind, it rather tended to confirm your belief in the veracity of his statements, accompanied, as it generally was, by a face animated with enthusiasm, and with sincerity depicted on it.

Now, if such a man tells you that water is horizontal, and that he saw it with his own eyes, describing the particulars of his alleged experiments on the Bedford canal, how on earth, I ask, can you disbelieve him? Again, if water is horizontal, how can the earth be round? And if the earth is not round, how then could she move in an orbit and rotate on an axis? These are questions enough to rouse you against all the teachings and calculations of Copernicus, Kepler, Newton, La Place, and to provoke your just anger against Mr. Proctor, who, in spite of the self-evident fact, not only ignores it, but continually reflects on the sincerity and character of a great philosopher.

We must, however, leave the astronomical part of our narrative until we come to its proper turn. Let us first see what are the aims and objects of the much despised and *unjustly* persecuted philosopher. I must, however, add, that I was in blissful ignorance of his existence up to last September, and, consequently, knew nothing of the affair of Mr. Hampden and Mr. Wallace. How *could* I know it? The fame of the great philosopher must have been frozen into silence, in her flight, at the very gates of Moscow, and, perhaps, at the very doors of the schoolroom where my education was being enforced upon me at the time. Cherubs did not sing his praises—they must have had something else to engage their attention with at the time. Nor did our Russian journalists, so far as I am aware, open the columns of their respective journals for the discussion of the important subject. And since my sojourn in England, although an ardent student of English literature, the name of "Parallax" did not, up to the above-named date, meet my eye, except as an astronomical term.

It is scarcely necessary for me to ask the indulgence of the readers of Knowledge for entering into the details of the aims and principles of the man, who, for nearly half a century, has been stumping the country, and making converts to his teachings. He himself professes to seek after *truth*. We shall therefore act on his own advice, and take her scales to weigh him *in propriâ personâ*.

To sum up before we proceed with our actual *exposé*, I will declare in a few words that, which I shall substantiate further on, viz., that "Parallax" is an accomplished *quack*, calling himself now, Dr. Samuel Birley, professing to possess the secret for preserving and prolonging life, but which he also can shorten; who undertakes to cure every disease imaginable for a *guinea* for every two months; the flat earth being only one of the means for decoying the suffering part of humanity, for whose benefit he pretends to live, but whose units, in the meanwhile, assist him in enlarging his fortune, over which he keeps most careful watch. The only wonder is that such proceedings are suffered with impunity—but, then, one must also bear in mind that there is a way to escape punishment—and all those who wish for the same should follow it—let them simply change their names.

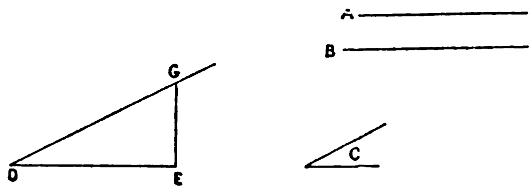
Our Mathematical Column.

NOTES ON EUCLID'S FIRST BOOK.

By Richard A. Proctor.

Prop. 22 is the only one in which Euclid show show to construct a triangle from given elements. But by means of Prop. 23 the following other cases can be solved.

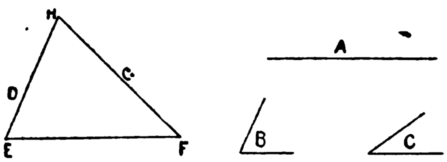
Prop. III.—Prob. *To make a triangle having two sides equal to two given straight lines and enclosing a given angle.*



Let A, B be the given lines, C the given angle.

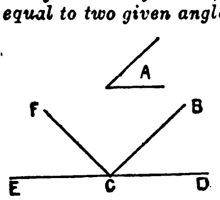
Take a straight line D E equal to A, and at the point D make the angle E D G equal to C. Take D G equal to B, and join G E. Then it needs no demonstration to show that D G E is the required triangle.

Prop. IV.—Prob. *To make a triangle having one side equal to a given straight line, and the angles adjacent to this side equal to two given angles whose sum is less than two right angles.*



Let A be the given line, B, C the given angles. Take E F equal to A, at E make the angle F E D equal to the angle B, and at F make the angle E F G equal to the angle C. Then the sum of the angles F E D and E F G, being equal to the sum of the angles B and C, is less than two right angles. Hence E D and F G meet if produced far enough. Let them meet in H, then E H F is obviously the required triangle.

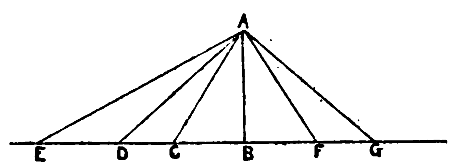
Prop. V.—Prob. *To construct a triangle, having one side equal to a given straight line, and two angles not both adjacent to this side equal to two given angles, whose sum is less than two right angles.*



If A and B C D are the given angles, we obtain the third angle of the triangle, and thus reduce the problem to Prop. IV., by producing D C to E, and making the angle E C F equal to the angle A. For the three angles of the triangle are together equal to the three angles E C F, F C B, and B C D together (Euc. I. 32). Therefore the third angle of the triangle is equal to F C B.

Another case remains, before proceeding to which, however, it will be well to establish the following theorem :—

Prop. VI.—*The perpendicular is the shortest line which can be drawn from a given point to a given line; and of others that which is nearer to the perpendicular is less than the more remote, and converse; and not more than two equal lines can be drawn from the given point to the given straight line one on each side of the perpendicular.*



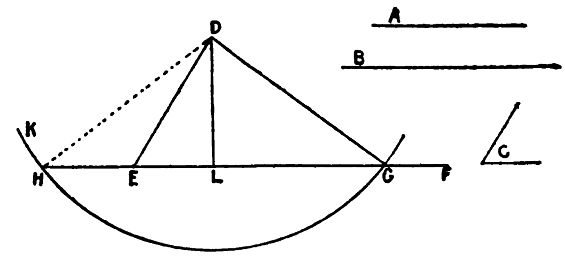
Let A B be a straight line drawn from A perpendicular to E F; and let A C, A D, A E be other straight lines from A to E F, in their order of distance from A B. Then, in the triangle A B C the angle A B C is a right angle; hence the angle A C B is less than a right angle (Euc. I. 17), and A B is therefore less than A C (Euc. I. 19). Also in the triangle A C D, A C D is an obtuse angle, being the supplement of the acute angle A C B; hence A D C is an acute angle, and A C is less than A B (Euc. I. 19). Similarly A D is less than A E, and so on.

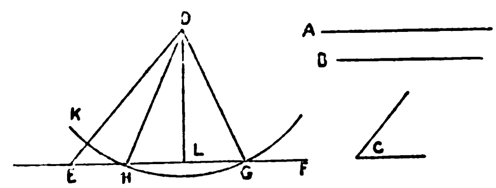
Also if A F be drawn so as to make the angle B A F square to he angle B A C (Euc. I. 23), and meeting E G in F (Euc. I. 17, and Axiom 12), the triangles B A C and B A F are equal in every respect (Euc. I. 26), and therefore A F is equal to A C. Also no other line as A G can be equal to A C, that is to A F, because, then, two lines unequally remote from A B would be equal, which has been shown to be impossible.

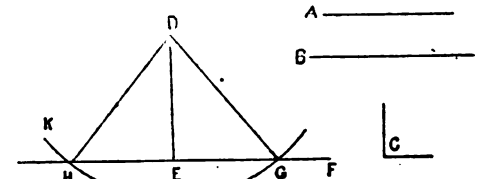
Prop. VII.—Prob. *To construct a triangle having two sides equal to two given straight lines, and an angle opposite one of these sides equal to a given angle.*

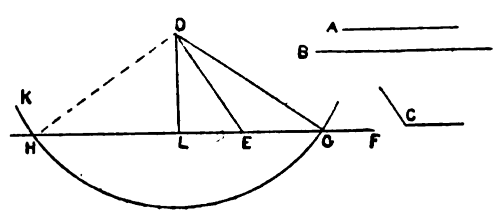
Let A and B be the given lines, C the given angle; and let the side equal to B in the required triangle be that which is to be opposite to the angle equal to C.

Draw a line E F terminated towards E, and from E draw E D making the angle D E G equal to C. Take D E equal to A. From D draw D L perpendicular to E F. Then since D L is the shortest line connecting D with a point in E F (Prop. VI.), if B be less than D L it is impossible to construct a triangle with the given elements. But if B be not less than D L, with centre D and radius equal to B describe the circle G H K cutting E F, produced if necessary towards E, in H and G.









First suppose C an acute angle. Then if B is greater than A, or D G, D H greater than D E, the points H and G lie on opposite sides of E (Prop. VI). Hence by joining D G, we obtain the triangle D E G (and only this triangle), which has the required elements. If B is less than A, the points H and G lie on the same side of E (Prop. VI.), and by joining D H and D G we obtain two triangles D E H and D E G, each of which has the required elements. No demonstration is required in either case. If B be equal to D L, D G and D H coincide and there is but one solution.

If C be a right angle, the two triangles D E H and D E G (which are equal in all respects, however) have the required elements.

Lastly if C be obtuse, it is clear there is but one solution, since D G must be greater than D E that the circle K H G may meet E F in a point between E and F. The triangle D E G clearly has the required elements.

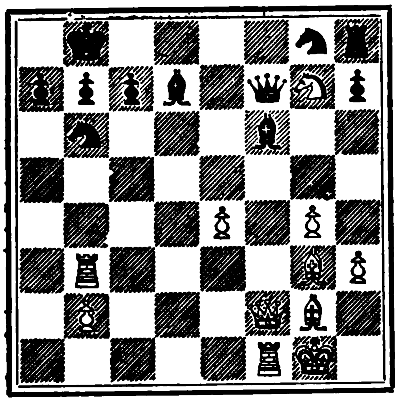
In Euc. I. 5 and 6, we learn two properties of isosceles triangles. And it is to be noticed that the 9th, 10th, 11th, and 12th involve, more or less, the properties of such triangles. It is clear, for instance, that the proof of Prop. 10, does not require that the triangle A C B should be equilateral; but only that A C, C B should be equal. So also, if D F, F E are equal in Prop. 11, the proof is sufficient. In Prop. 12 F C G is an isosceles triangle.

(*To be continued.*)

Our Chess Column.

BY MEPHISTO.

THE following pretty ending occurred in a game at odds, recently played :—



White.	Black.
R × Kt	RP × R
Q × P	Q × Kt
P to K5 (!)*	P × Q

(The only move, for if either B to B3 or P to B3—to prevent the mate—then P × B, or B × B would be fatal.)

P × B (ch)	Q to B2

(If K moves, R mates.)

P × Q and wins.

"THERE is nothing new under the sun!" The prettiest and most brilliant combination in a game may very often be due merely to a recollection of what had previously actually occurred, or analytically shown to be possible in positions similar or closely resembling those from whence the combination arose. A practical player knows that in each opening, in reply to a particular move, a certain attack is possible. The better a player knows the openings, and the more experience he has, the more dangerous he will become as an adversary, by being able to take prompt advantage of a weak move. In the following pretty game we have a recurrence of a fine finish which has occurred to us before several times in games printed in KNOWLEDGE. In the present instance, however, the game opened in a different manner from those previously recorded.

GAME PLAYED BY MEPHISTO AND A STRONG AMATEUR.

Hampe Algaier.

White. Mephisto.	Black. Amateur.
1. P to K4	P to K4
2. Kt to QB3	Kt to QB3 (a)
3. P to B4	P × P
4. Kt to B3 (b)	P to KKt4
5. P to KR4	P to Kt5
6. Kt to Kt5	P to KR3
7. Kt × P	K × Kt
4. P to Q4 (c)	P to Q4
5. Kt × P	B to K3
6. KB to B4	Kt to K4 (d)
7. B × P (e)	Kt × B
8. Castles (f)	K to Kt3 (g)
9. Q to Q3	Kt to Q3 (h)
10. P to R5 (ch) (i)	K × P
11. B × Kt	B × B
12. P to K 5 (j)	B × P (k)
13. P × B	Q × Kt
14. R to B5 (ch) (l)	B × R (m)
15. Q × B (ch) (!)	K to R5
16. P to Kt6 (ch)	K to R6
17. Q to Bsq (ch)	K × P
18. Q to B2 (ch)	K to R6
19. Q to R2 mate	

* [But why not Q takes P(ch), followed by Q to QR5, mate? Possibly, however, the position, as seen by me in proof, is incomplete?—R. P.]

NOTES.

(a) This is considered best, and is called the Vienna, or Hampe, game. B to B4, or B to K2, are considered less satisfactory replies for Black. Kt to KB3, which is followed by 3. $\frac{\text{P to B3}}{\text{P to Q3}}$ 4. $\frac{\text{P to Q4}}{\text{P to Q3}}$, &c., lead to an even game.

(b) With 4. P to Q4 we have the Steinitz Gambit, to which the best reply is Q to R5 (ch). 5. $\frac{\text{K to K2}}{\text{P to Q 4}}$ 6. $\frac{\text{P} \times \text{P}}{\text{Q to K2}}$ (ch), &c.

(c) This move now transforms the opening into a Hampe-Algaier, which is believed to be richer in attacking variations than the ordinary Algaier attack, without the development of the Q Kt.

(d) Kt to R4 in the beginning of a game is very often but a precarious resource, as it loses time and puts the piece out of play. B × Kt would not have been good, as after B × B (ch) the White Q can capture the unprotected KtP. We believe 6. Kt to B3 might have been played here, threatening Kt × Kt. Black has nothing to fear from 7. $\frac{\text{Kt} \times \text{Kt}}{\text{B} \times \text{B}}$ 8. $\frac{\text{Kt} \times \text{P}}{\text{Q} \times \text{QP}}$ 9. $\frac{\text{Q} \times \text{R}}{\text{Kt} \times \text{Q}}$ 10. $\frac{\text{Kt to K5 (ch)}}{\text{KtK3}}$ 11. $\frac{\text{Kt} \times \text{B}}{\text{Kt} \times \text{P (ch)}}$, &c.

(e) Taking advantage of the position for the purpose of attack at the expense of another piece.

(f) Threatening to win the Q.

(g) Zukertort advocates K to Kt3 in similar positions in preference to K to Kt2, to avoid the attack of the White QB. If K to Kt2, then 9. P to QKt3, with the object of gaining the square on K5.

(h) If White plays 10. $\frac{\text{B} \times \text{Kt}}{\text{B} \times \text{Kt}}$ 11. $\frac{\text{P to K5 (ch)}}{\text{KtKt2}}$ 12. $\frac{\text{P} \times \text{Kt}}{\text{B} \times \text{Kt}}$ Black has a defensible game.

(i) Black has no alternative but to capture the P; for if K to R2 then follows 11. B × Kt threatening 12. P to K5 (ch), and mate by Q to Kt6. K to B2 is, of course, inadmissible on account of the discovered check by the B and R. We give a diagram of the position.

Amateur.
BLACK.

WHITE.
Mephisto.

(j) Threatening Kt to B4 (ch), followed probably by Q to K3, &c.

(k) Black had no good move at his command to avoid Kt to B4.

(l) This was unforeseen by Black. The mate following this move is very often made possible, even under slightly altered circumstances, provided the Black King has to go to R4. We have had several opportunities for *administering* it before.

(m) A vain hope.

THE fourth of the series of Gilchrist Penny Science Lectures was delivered on Friday evening, 21st March, on "Air and why we breathe it," by Wm. Lant Carpenter, Esq., to a numerous and deeply interested audience. After demonstrating experimentally the weight and upward pressure of the air, he referred to its chemical constitution and successfully performed several brilliant experiments, illustrating the changes produced in air by combustion and by respiration, pointing out the source of the carbonic acid in each case. The properties of this poisonous gas and the lime-water and other tests for it were carefully demonstrated. The oxyhydrogen lantern was then employed to exhibit tables of the composition of air under various conditions, methods of ventilation, &c. The statement that the air of the Vic. contained only ten parts of carbonic acid in 10,000 of air, as against an average of 16 parts for London theatres, and three parts for fresh country air, excited sympathetic applause, as did also the lecturer's concluding references to the evils of the present fashion of tight-lacing, illustrated by photographs on the screen of Paris fashion books contrasted with the Venus of Milo and with Miss Mary Anderson as Galatea.

Our Whist Column.

By "Five of Clubs."

The following game was played recently in the Editor's home circle, *Y*, *Z* being respectively the Editor and Mrs. Editor; *A*, *B* other members of the family circle.

The Hands.

B { *Diamonds*—Q, 10, 9, 4. Clubs—Q, Kn, 9, 3. Hearts—Q, 7, 4. Spades—Kn, 3. }

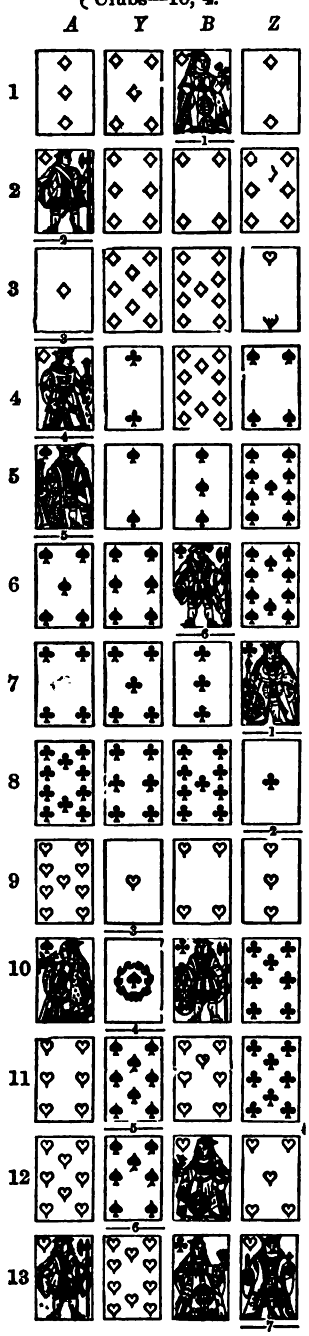
Y { *Diamonds*—8, 6 5. Clubs—6, 5, 2. Hearts—A, 10. Spades—A, 8, 7, 6, 2. }

A B, 3. **B**

Y Y Z, 4. Dealer. **Z** Trump. D. Q. Leader. **A**

Z { 2, 7—*Diamonds.* 7, 8, K, A—Clubs. 2, 3, 5, K—Hearts. 9, 4, 10—Spades. }

A { *Diamonds*—A, K, Kn, 3. Clubs—10, 4. Hearts—Kn, 8, 9, 6. Spades—K, Q, 5. }



NOTES AND INFERENCES.

1. *A* regards the game as won' because with the four honours nothing can save *Y*, *Z* but making the odd trick, which with such cards as *A* and *B* hold between them—to *A*'s knowledge—seems unlikely.

2 and 3. *B* has shown by the return of the small Diamond that he held four. But *A* is a rather young player, who has not as yet learned duly to notice such points. Accordingly,

4. *A* draws partner's last Trump instead of the adversaries'.

5. *Y* does not cover the King, as he sees it is important to win the third trick in Spades.

6. Still *Y* passes the trick. Apart from the necessity of winning the third trick in Spades, there is a chance that *Z* may win the second. However, it is won by *B*. Observe that *Z*'s discard at trick 3 shows *A* that Hearts are *Z*'s longest suit (Trumps being declared against *Y-Z*).

8. *Z* is quite right, as the game and score stand to secure a trick in Clubs before leading a Heart. Every trick has to be made to save game, so that retaining the King card of opponent's suit would here be of small use.

9, 10, 11, 12, 13.—The rest of the game plays itself. *Y-Z* win the odd trick.

The game is given, "errors included," precisely as recorded immediately after the hand was played. It shows that a game should not be regarded as lost until it is won, nor *vice versâ*.

Answer to Correspondent.

Q. T. V.—Meaning that his partner should at the end of the round know what *Z* knows (also at the end of the round).

A WHIST-PLAYER'S WAIL.

By the Author of "Bumblepuppy."

Whist-players have long been suffering acutely from three uncertainties—uncertainty of the laws, uncertainty of decisions, and uncertainty of authority. I propose to consider these three points, one by one, and then to suggest a remedy or two.

The laws are ninety-one in number, and, in "Cavendish on Whist," are supplemented by forty-three explanatory notes and a couple of suppositions, which again have been further explained—if explain is the right word in this connection—by innumerable irresponsible decisions. Now, though it may be Utopian to expect such a badly-worded jumble of laws and definitions ever to be superseded by an intelligible code, is it impossible to have these decisions based on a principle of some kind, or, at any rate, for them to be consistent with themselves?

At one time the decider has confined himself to the strictest letter of the law, at another time he has strained it to breaking; sometimes he has read the laws one with another; sometimes he has taken one and left the other out in the cold; sometimes he appears arbitrarily to give his decision out of his own head, quite irrespective of any law whatever; and finally, and worst of all, after consistently maintaining one position for years and years, until—rightly or wrongly—some doubtful point is settled, he suddenly turns round, with his tail where his head always used to be, flatly contradicts himself and throws it once more into confusion.

The usual excuse for a *volte face* of this kind is, "that this is a free country, where every man has a right to change his opinions"; and I never hear that dreadful exordium without instinctively making for the door, knowing from bitter experience that mischief is brewing. "That judges themselves differ, and the judgment of one court is often over-ruled by another," this also is, I am afrad, true, though it has no bearing on the matter in hand; for here we have a judge who, on his appointment to the bench—granting, what is strongly disputed, that a Whist arbitrator is a judge and has a bench—having found a well-establishment precedent and taken it for his guide in numerous judgments, one fine day reverses it without notice and without leave to appeal.

(*To be continued*).

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CONTENTS OF No. 128.

COINCIDENCES AND SUPERSTITIONS.

BY R. A. PROCTOR.

(*Continued from page* 196.)

BUT when we have undoubted cases of 'coincidence, without the possibility of any real association (setting the supernatural aside), we have a problem of some interest to deal with. To explain them as due to some special miraculous intervention may be satisfactory to many minds, in certain cases; but in others it is impossible to conceive that the matter has seemed worthy of a miracle. Even viewing the question in its bearing on religious ideas, there are cases where it seems far more mischievous (as bringing ridicule on the very conception of the miraculous) to believe in supernatural intervention, than to reject such an explanation on the score of antecedent improbability. Horace's rule: "*Nec deus intersit, nisi dignus vindice nodus*," remains sound when we write "*Deus*" for "*deus*."

Now there have been cases so remarkable, yet so obviously unworthy of supernatural intervention, that we are perplexed to find any reasonable explanation of the matter. The following, adduced by De Morgan, will, I have no doubt, recall corresponding cases in the experience of readers of these lines:—"In the summer of 1865," he says, "I made myself first acquainted with the tales of Nathaniel Hawthorne, and the first I read was about the siege of Boston in the War of Independence. I could not make it out: everybody seemed to have got into somebody else's place. I was beginning the second tale, when a parcel arrived: it was a lot of odd pamphlets and other rubbish, as he called it, sent by a friend who had lately sold his books, had not thought it worth while to send these things for sale, but thought I might like to look at them, and possibly keep some. The first thing I looked at was a sheet, which, being opened, displayed 'A plan of Boston and its environs, showing the true situation of his Majesty's army, and also that of the rebels, drawn by an engineer, at Boston, October, 1775.' Such detailed plans of current sieges being then uncommon, it is explained that 'The principal part of this plan was surveyed by Richard Williams, Lieutenant, at Boston; and sent over by the son of a nobleman to his father in town, by whose permission it was published.' I immediately saw that my confusion arose from my supposing that the King's troops were besieging the rebels, when it was just the other way" (a mistake, by the way, which does not suggest that the narrative was particularly lucid).

Another instance cited by De Morgan is yet more remarkable, though it is not nearly so strange as a circumstance which I shall relate afterwards:—"In August, 1861," he says, "M. Senarmont, of the French Institute, wrote to me to the effect that Fresnel had sent to England in, or shortly after, 1824, a paper for translation and insertion in the *European Review*, which shortly after expired. The question was what had become of the paper. I examined the *Review* at the Museum, found no trace of the paper, and wrote back to that effect, at the Museum, adding that everything now depended on ascertaining the name of the editor, and tracing his papers: of this I thought there was no chance. I posted the letter on my way home, at a post-office in the Hampstead-road, at the junction with Edward-street, on the opposite side of which is a bookstall. Lounging for a moment over the exposed books, *sicut meus est mos*, I saw within a few moments of the posting of the letter, a little catchpenny book of anecdotes of Macaulay, which I bought, and ran over for a minute. My eye was soon caught by this sentence: "One of the young fellows immediately wrote to the Editor (Mr. Walker) of the *European Review*." I thus got the clue by which I ascertained that there was no chance of recovering Fresnel's papers. Of the mention of current Reviews not one in a thousand names the editor. It will be noticed that there was a double coincidence in this case. It was sufficiently remarkable that the first mention of a review, after the difficulty had been recognised, should relate to the *European*, and give the name of the editor; but it was even more remarkable that the occurrence should be timed so strangely as was actually the case.

But the circumstance I am now to relate, seems to me to surpass in strangeness all the coincidences I have ever heard of. It relates to a matter of considerable interest apart from the coincidence.

When Dr. Thomas Young was endeavouring to interpret the inscription of the famous Rosetta Stone, Mr. Grey (afterwards Sir George Francis Grey) was led on his return from Egypt to place in Young's hands some of the most valuable fruits of his researches among the relics of Egyptian art, including several fine specimens of writing on papyrus, which he had purchased from an Arab at Thebes, in 1820. Before these had reached Young, a man named Casati had arrived in Paris, bringing with him from Egypt a parcel of Egyptian manuscripts, among which Champollion observed one which bore in its preamble some resemblance to the text of the Rosetta Stone. This discovery attracted much attention; and Dr. Young having procured a copy of the papyrus, attempted to decipher and translate it. He had made some progress with the work when Mr. Grey gave him the new papyri. "These," says Dr. Young, "contained several fine specimens of writing and drawing on papyrus; they were chiefly in hieroglyphics and of a mythological nature; but two which he had before described to me, as particularly deserving attention, and which were brought, through his judicious precautions, in excellent preservation, both contained some Greek characters, written apparently in a pretty legible hand. That which was most intelligible had appeared at first sight to contain some words relating to the service of the Christian Church." Passing thence to speak of Casati's papyrus, Dr. Young remarks that it was the first in which any intelligible characters of the enchorial

form had been discovered among the many manuscripts and inscriptions which had been examined, and it "furnished M. Champollion with a name which materially advanced the steps leading him to his very important extension of the hieroglyphical alphabet. He had mentioned to me, in conversation, the names of Apollonius, Antiochus, and Antigonus, as occurring among the witnesses; and I easily recognised the groups which he had deciphered; although, instead of *Antiochus*, I read Antimachus; and I did not recollect at the time that he had omitted the m."

Now comes the strange part of the story.

"In the evening of the day that Mr. Grey had brought me his manuscripts," proceeds Dr. Young (whose English, by the way, is in places slightly questionable), "I proceeded impatiently to examine that which was in Greek only; and I could scarcely believe that I was awake and in my sober senses, when I observed among the names of the witnesses *Antimachus Antigenis* (*sic*); and a few lines farther back, *Portis Apollonii;* although the last word could not have been very easily deciphered without the assistance of the conjecture, which immediately occurred to me, that this manuscript might perhaps be a translation of the enchorial manuscript of Casati. I found that its beginning was, "A copy of an Egyptian writing;" and I proceeded to ascertain that there were the same number of names intervening between the Greek and the Egyptian signatures that I had identified, and that the same number followed the last of them. The whole number of witnesses was sixteen in each. . . . I could not therefore but conclude," proceeds Dr. Young, after dwelling on other points equally demonstrative of the identity of the Greek and enchorial inscriptions, "that a most extraordinary chance had brought into my possession a document which was not very likely, in the first place, ever to have existed, still less to have been preserved uninjured, for my information, through a period of near two thousand years; but that this very extraordinary translation should have been brought safely to Europe, to England, and to me, at the very moment when it was most of all desirable to me to possess it, as the illustration of an original which I was then studying, but without any other reasonable hope of comprehending it; this combination would, in other times, have been considered as affording ample evidence of my having become an Egyptian sorcerer." The surprising effect of the coincidence is increased when the contents of this Egyptian manuscript are described. "It relates to the sale, not of a house or a field, but of a portion of the collections and offerings made from time to time on account or for the benefit of a certain number of mummies of persons described at length in very bad Greek, with their children and all their households."

(*To be continued.*)

THE CHEMISTRY OF COOKERY.

XXXII.

By W. Mattieu Williams.

SINCE the publication of my last paper, I have been told by a lady to whom the readers of Knowledge are much indebted that in the fatherland of potatoes, as well as in their adopted country, they are always boiled or steamed in their jackets; that American cooks, like those of Ireland, would consider it an outrage to cut off the protecting skin of the potato before cooking it; that they are more commonly mashed there than here, and that the mashing is done by rapidly removing the skins, throwing the stripped potato into a supplementary saucepan or other vessel, in which they may be kept hot until the preparation is completed.

Returning to the subject at the point where I left it I must endeavour to describe the effect of cooking on gluten. It is usually described as "partly soluble in hot water." My own examination of this substance suggests that "partially soluble" is a better description than "partly soluble" (Miller) or "very slightly soluble" (Lehmann). This difference is not merely a verbal quibble, but very real and practical in reference to the *rationale* of its cookery. A partly soluble substance is one which is composed of soluble and also of insoluble constituents, which, as already stated, is strictly the case with gluten in reference to the solvent action of hot alcohol. A very slightly soluble substance is one that dissolves completely but demands a very large quantity of the solvent. I find that the action of hot water on gluten, as applied in cookery, is to effect what may be described as a partial solution, that is, effecting a loosening of the bonds of solidity, but not going so far as to render it completely fluid.

It appears to be a sort of hydration similar to that which is effected by hot water on starch, but less decided.

To illustrate this, wash some flour in cold water so as to separate the gluten in the manner described in No. 29; then boil some flour as in making ordinary bill-sticker's paste, and wash this in cold water. The gluten will come out with difficulty, and when separated will be softer and less tenacious than the cold washed specimen. This difference remains until some of the water it contains is driven out, for which reason I regard it as hydrated, though I am not prepared to say that the hydration is of a truly chemical character, not a definite compound of gluten and water, but rather a mechanical combination—a loosening of solidity by a molecular intermingling of water.

The importance of this in the cookery of grain-food is very great, as any body who aspires to the honour of becoming a martyr to science may prove by simply making a meal on raw wheat, masticating the grains until reduced to small pills of gluten, and then swallowing these. Mild indigestion or acute spasms will follow, according to the quantity taken and the digestive energies of the experimenter. Raw flour will act similarly but less decidedly.

Bread-making is the most important, as well as a typical example, of the cookery of grain food. The grinding of the grain is the first process of such cookery; it vastly increases the area exposed to the subsequent actions.

The next stage is that of surrounding each grain of the flour with a thin film of water. This is done in making the dough by careful admixture of a modicum of water and kneading in order to squeeze the water well between all the particles. The effect of insufficient enveloping in water is sometimes seen in a loaf containing a white powdery kernel of unmixed flour.

If nothing more than this were done, and such simple dough were baked, the starch granules would be duly broken up and hydrated, the gluten also hydrated, but, at the same time, the particles of flour would be so cemented together as to form a mass so hard and tough when baked that no ordinary human teeth could crush it. Among all our modern triumphs of applied science none can be named that is more refined and elegant than the old device by which this difficulty is overcome in the every-day business of making bread. Who invented it, and when, I do not know, but perhaps Mr. Clodd can tell us. Its discovery was certainly very far anterior to any knowledge of the chemical principles involved in its application.

The problem has a very difficult aspect. Here are millions of particles, each of which has to be moistened on its sur-

face, but each when thus moistened becomes remarkably adhesive, and therefore sticks fast to all its surrounding neighbours. We require, without suppressing this adhesiveness, to interpose a barrier that shall sunder these millions of particles from each other so delicately as neither to separate them completely, nor allow them to completely adhere.

It is evident that if the operation that supplies each particle with its film of moisture can simultaneously supply it with a partial atmosphere of gaseous matter, the difficult and delicate problem will be effectively solved. It is thus solved in making bread.

As already explained, the seed which is broken up into flour contains diastase as well as starch, and this diastase, when aided by moisture and moderate warmth, converts the starch into dextrine and sugar. This action commences when the dough is made, and this alone would only increase the adhesiveness of the mass, if it went no further; but the sugar thus produced may, by the aid of a suitable ferment, be converted into alcohol. As the composition of alcohol corresponds to that of sugar, minus carbonic acid, the evolution of carbonic acid gas is an essential part of this conversion.

With these facts before us, their practical application in bread-making is easily understood. To the water with which the flour is to be moistened some yeast is added, and the yeast cells, which are very much smaller than the grains of flour, are diffused throughout the water. The flour is moistened with this liquid, which only demands a temperature of about 70° Fahr. to act with considerable energy on every granule of flour that it touches. Instead, then, of the passive, lumpy, tenacious dough produced by moistening the flour with mere water, a lively "sponge," as the baker calls it, is produced, which "rises" or grows in bulk by the evolution and interposition of millions of invisibly small bubbles of gas. This sponge is mixed with more flour and water, and kneaded and kneaded again to effect a complete and equal diffusion of the gas bubbles, and finally the porous mass of dough is placed in an oven previously raised to a temperature of about 450°.

The baker's old-fashioned method of testing the temperature of his oven is instructive. He throws flour on the floor. If it blackens without taking fire, the heat is considered sufficient. It might be supposed that this is too high a temperature, as the object is to cook the flour, not to burn it. But we must remember that the flour which has been prepared for baking is mixed with water, and the evaporation of this water will materially lower the temperature of the dough itself. Besides this, we must bear in mind that another object is to be attained. A hard shell or crust has to be formed, which will so encase and support the lump of dough as to prevent it from subsiding when the further evolution of carbonic acid gas shall cease, which will be the case some time before the cooking of the mass is completed. It will happen when the temperature reaches the point at which the yeast-cells can no longer germinate, which temperature is considerably below the boiling-point of water.

In spite of this high outside temperature, that of the inner part of the loaf is kept down a little above 212° by the evaporation of the water contained in the bread; the escape of this vapour and the expansion of the carbonic acid bubbles by heat increasing the porosity of the loaf.

The outside being heated considerably above the temperature of the inner part, this variation produces the differences between the crust and the crumb. The action of the high temperature in directly converting some of the starch into dextrine will be understood from what I have already stated, and also the partial conversion of this dextrine into caramel, which was described in Nos. 13 and 14 of this series. Thus we have in the crust an excess of dextrine as compared with the crumb, and the addition of a variable quantity of caramel. In lightly-baked bread, with a crust of uniform pale yellowish colour, the conversion of the dextrine into caramel has barely commenced, and the gummy character of the dextrine coating is well displayed. Some such bread, especially the long staves of life common in France, appear as though they had been varnished, and their crust is partially soluble in water.

This explains the apparent paradox that hard crust, or dry toast, is more easily digested than the soft crumb of bread; the cookery of the crumb not having been carried beyond the mere hydration of the gluten and the starch, and such degree of dextrin formation as was due to the action of the diastase of the grain during the preliminary period of "rising."

Everybody has, of course, heard of "aërated bread," and most have tasted it. Several methods have been devised, some patented, for effecting an evolution of gas in the dough without having recourse to the fermentation above described. One of these is that of adding a little hydrochloric acid to the water used in moistening the flour, and mixing bi-carbonate of soda in powder with the flour (to every 4 lb. of flour ½ oz. bi-carbonate, and 4½ fluid drachms of hydrochloric acid of 1·16 specific gravity). These combine and form sodium chloride, common salt, with evolution of carbonic acid. The salt thus formed takes the place of that usually added in ordinary bread-making, and the carbonic acid gas evolved acts like that given off in fermentation; but the rapidity of the action of the acid and carbonate presents a difficulty. The bread must be quickly made, as the action is soon completed. It does not go on steadily increasing and stopping just at the right moment, as in the case of fermentation.

I remember the first introduction of this about half a century ago, and the anticipations which accompanied it. London was agitated by the bread reform movement, and bakers were alarmed. A large establishment was opened in Oxford-street, and much amusement created by an opposition placard display in some of the neighbouring bakers' shops, "Bread sold here with the gin in it." This, of course, was fallacious, as the alcohol produced by the panary fermentation is driven off by the heat of the oven. Other methods similar in principle have been adopted, such as adding ammonia carbonate with the soda carbonate. The ammonia salt is volatile itself, besides evolving carbonic acid by its union with the acid.

In spite of the great amount of ingenuity expended upon the manufacture of such unfermented bread and the efforts to bring it into use, but little progress has been made. The general verdict appears to be that the unfermented bread is not so "sweet," that it lacks some element of flavour, is "chippy" or tasteless as compared with good old-fashioned wheaten bread, free from alum or other adulteration. My theory of this difference is that it is due to the absence of those changes which take place while the sponge or dough is rising, when, if I am right, the diastase of the grain is operating, as in germination, to produce a certain quantity of dextrin and sugar, and possibly acting also on the gluten. Deficiency of dextrin is, I think, the chief cause of the chippy character of aërated bread. It must be remembered that this stage is protracted over several hours, during which the temperature most favourable to germination is steadily maintained. Other and very interesting phenomena connected with bread-making will be treated in my next.

PLEASANT HOURS WITH THE MICROSCOPE.

By Henry J. Slack, F.G.S., F.R.M.S.

A VEGETABLE common now on the family dinner-table, rhubarb, readily affords to the microscopist spiral vessels of considerable beauty. A small piece of soft rhubarb may be taken from a well-cooked pie, and picked to pieces, under a low power, with needles inserted in sticks. The rhubarb should be so soft that most of its tissues are pappy. The spiral vessels are seen as fine threads, offering more resistance. They should be teased out of the surrounding tissue in a drop of water, and the refuse matter washed away. It is convenient to do such things under a dissecting microscope, but if the experimentor has not got one, a hand-magnifier, supported on a stand easily made with a piece of brass wire, will do nearly as well. If the rhubarb is in a raw state, it should be boiled in water to which a little caustic potash is added. A piece the size of a small pea will do for a couple of ounces of water. Only a small piece of the vegetable is required, say an inch long, cut into strips. A large test-tube does well to cook them in, or a small round enamelled iron soap-dish, sold by ironmongers, and much less likely to break than a porcelain one. As soon as a few of the thread-like vessels are drawn out they should be placed on a slide and examined with an inch or two-thirds objective. This will show whether they are good specimens, and if more cleaning from extraneous matter is required. When all right, and spread out to avoid entanglement, they may be allowed to dry on the slide. A drop of Canada balsam, thinned with benzine, should then be placed on a covering-glass and pressed down on the objects. The spring clips sold by opticians for this purpose will keep the cover down tight, and if the slide is put in a warm place it will be dry enough to examine in a few hours. It is well, however, to keep the clips on until the balsam is hardened.

Some of the spiral vessels may, before balsaming, be stained with carmine or hæmatoxylin, the colouring matter of logwood. They are prettier this way with ordinary illumination as transparent objects, but some should be mounted without staining, as they are best to examine with polarised light. The spirals make a splendid display of colours if their action on the light is assisted by selenite or by films of mica.

To understand the nature of these objects, cut a very thin horizontal slice off a rhubarb stem, and examine it with a low power. It will be seen that there are numerous darker patches distributed amongst the tissues of the transparent cells. Make a thin vertical section of the stem, and they will appear as long slender columns. Any one of them pulled to pieces by the needles will show that it is largely composed of the spiral vessels, some of which may be pulled out into threads. Vessels occurring in various plants in groups of this sort are known as fibro-vascular bundles. Some are kept open with the spiral thread, others are strengthened by deposits in patterns which give them a pitted, a dotted, or a stair-like (scalariform) appearance. Whatever may be their pattern, it is formed by growths that thicken the cell walls. The result may be a stout vessel or pipe, or, as in the case of some of the spirals, the cell walls may disappear, and leave the spirals as isolated coils of cellulose.

Spiral vessels are often so strong that when a plant-stem is torn they hang out as loose threads. A leaf-stalk of the great plantain, or stinging-nettle stem (*Plantago major*) shows them well; but the student should examine how these bundles are disposed in different orders of flowering plants, and in the underground stem of the brake (*Pteris aquilina*), a horizontal section of which shows the vessels in the eagle pattern which suggested its specific name.

At this season, the flowery banks and woods abound in the elegant Wood Sorrel (*Oxalis acetosella*), which the microscopist should not neglect. This plant owes its pleasant sharpness to small quantities of oxalic acid in its juices, and the leaves contain numerous patches of oxalate of lime. To see these to advantage, let a leaf or two soak in strong methylated spirit for four-and-twenty hours. This removes nearly all the chlorophyll and makes the leaf more transparent. Place a leaf so prepared on a glass slide, gently press it with blotting-paper to remove most of the alcohol, and then mount with thin Canada Balsam, placed as before described on the cover, and inverting that on the leaf. The oxalate is not found in well defined crystals, but in irregular patches, some extremely small, and others a good deal bigger. The way in which they are distributed amongst the cellular tissue is well seen with a magnification of about 50 linear, but to convert the leaf into an object of beauty requires polarised light, and selenite, or mica, as advised for the spiral vessels. The crystals should be seen as of a golden or silver tint upon a green or a dark chocolate ground, which is easily managed by revolving the polarising and analysing prisms until the desired effect is obtained.

THE MORALITY OF HAPPINESS.

A Study of the Scientific Aspect of Conduct and Duty.

By Thomas Foster.

(*Continued from page 205.*)

CARE OF OTHERS AS A DUTY.

I ENTER now on a portion of my subject where I shall seem less at issue with those who repeat with their lips, and fancy they hold in their hearts (though they never think of following in their lives) certain rules of conduct in which due care of self is treated as objectionable and evil is spoken of as not to be resisted but encouraged. I shall still be at issue with those who assert, apparently without thinking—certainly without alleging any reasons—that conduct and duty are not matters for scientific discussion at all, that they have no scientific aspect, and that such considerations as the progress and improvement of life, the increase of the fulness and happiness of life, and so forth, have no bearing whatever, and should have none, on our opinion as to what is right or wrong. But we may very well afford to disregard objections having so little relation to actual facts. Every one really guides his conduct in large part by such considerations as many thus allege to have no proper bearing on conduct; nor can any one draw a line beyond which such considerations must not operate: when any one has tried to do so, and perhaps imagines he has succeeded, then I shall simply meet his objection with the remark that he need consider what I have said and what I may hereafter say as only applying to such parts of conduct as he has admitted to be within the range of scientific discussion.

Let us take, now, the doctrine that while due care of self comes to each man, and indeed to every creature having life, as essentially *first*, yet due care of others—though second to due care of self—is as absolutely essential. The two are interdependent—and that to such degree that neither can exist without the other. The great difference

in the treatment which science has to extend to the two forms of duty—the egoistic and the altruistic—resides in this, that whereas in insisting on egoistic duties science is really insisting on what every normally-constituted man is already apt to attend to, in insisting on altruistic duties science is insisting on duties wofully neglected, despite the fervour with which they are verbally enjoined. Many reject egoistic duties in words, who look so carefully after their own interests in action, that those who inculcate due care of self as a duty are ashamed to have to admit such utter selfishness as among the results (the unwholesome fruits, as it were), of the process of development which conduct, like all things else, has undergone, is undergoing, and will ever continue to undergo. The truth is that the careful study of what may be rightly sought and claimed for self is no unworthy preparation for due thought and care of others.*

Let us briefly trace the development of altruism.

In many of the lower forms of animal life, the acts which tend to race maintenance are altruistic. The parent is sacrificed wholly or partially in the production of progeny. Nor even in the higher forms of life does this form of sacrifice disappear, though the very beginning of new existences may involve egoistic rather than altruistic relations. Unconsciously at first, but consciously afterwards, and later still by definite actions to that end directed, the mother of each new member of even the human race divine, sacrifices herself for her offspring. We may be said to imbibe altruism with our mothers' milk. Every act by which in babyhood our life was fostered was a practical exemplification of the doctrine that care of others is essential to the maintenance and progress of the race. To altruism each one of us owes life itself, and the human race owes its existence as certainly to altruism, though such altruism was secondary to egoism in its influence.

And note here, in passing, how development of conduct is related to this early altruistic care of the individual life. As certainly as a want of due care of self leads to the diminution of altruism, by causing those who are not duly egoistic to disappear from the scene of life and leave no successors or few, so does want of due care of others, in the nourishment and rearing of offspring, lead inevitably to the diminution and eventual disappearance of types not sufficiently altruistic. The careless unloving mother is unconsciously doing her part in eliminating selfishness from the world (the process however slow is a sure one), for the child she neglects shares her nature, and must thrive less than a child of happier nature nursed and cared for by a more loving mother. In whatever degree individual instances may seem to tell against this process of evolution, in the average of many cases and through many generations the law must certainly tell.

Nor is this law limited to the influence of the parent who has most to do with the earlier years of life. Throughout childhood and in greater or less degree to the hour and even beyond the hour when each man and each woman begins to take part in the duties of life, and in most cases in the actual struggle for life, development depends on cares which will be well bestowed by unselfish parents, and so tend to increase the amount and fulness of unselfish life, while the selfish will neglect them, and so unconsciously help to eliminate (in the long run) the more selfish natures. It must be so if there is any truth in the doctrine of heredity, and the doctrine is not only true but is universally recognised: it is scarcely more clearly and certainly recognised now than it was by those who in old times made the pregnant proverb, full of old-world wisdom and experience, "The fathers have eaten sour grapes, and the children's teeth are set on edge." Fathers and mothers who are selfish by nature rear with less care offspring who as certainly inherit their nature as the young of beasts of prey inherit the carnivorous tastes of those to whom they owe their lives. Hence, fortunately for the race,—seeing how many egoistic tendencies are apt to be fostered in the struggle for life,—a constant tendency to the elimination of the more selfish natures.

To this may be added the consideration that the ill-reared and unduly egoistic are less likely than those of more generous and altruistic nature to be found pleasing by those of the opposite sex, less likely therefore to marry, so that (speaking always of the average not of individual cases) there is yet another factor opposing the increase in number of the unduly egoistic.

Thus do we recognise on the one hand that within families a due degree of altruism is essential to the development of life and life's fulness, while on the other hand undue egoism tends directly in more ways than one to diminish happiness.

The best proof that such influence is exerted is found in the circumstance that in every advancing community the young are cared for with constantly-increasing care. Among savage races offspring receive few altruistic attentions. They are not reared in the full sense of the word. Almost from the beginning of their lives they have to take part in the struggle for life. In civilised communities they are cared for during many years, and they are better, more thoroughly, and more wisely, cared for, the more such communities advance. All this indicates and enables us to measure the development of altruism, so far as the family is concerned.

And that care of others in this case (*i.e.* within the family) is not only essential to the development of life and its fulness, but also to the happiness of self, will be clear if we consider the matter with the least attention. For the altruistic nature shown in the care of children is inherited by children and developed in them by such care. Hence, as Mr. Spencer well notes, there results such conduct on the part of children as "makes parenthood a blessing." Of the parent of children inheriting such natures and so reared, it may be said that, even in our days (to which the saying of the Hebrew Psalmist was not, I suppose, intended originally to apply), the man is blessed that hath his quiver full of them.* On the contrary, where the parents and therefore probably the children are of selfish nature, and the example set the children is unduly egoistic, parenthood is no blessing, and may well become a source of misery. What happens in this case? asks the philosopher whose treatment of the scientific aspect of duty we are following. "First the domestic irritations must be relatively great; for the actions of selfish children to one another and to their parents cause daily aggressions and squabbles. Second, when adult such children are more likely than others to dissatisfy employers, alienate friends, and compromise the family by misbehaviour, or even by crime. Third, beyond the sorrows thus brought on them, the parents of such children have eventually to bear the sorrows of neglected old age. The cruelty shown in extreme degrees by savages who leave the decrepit to starve, is shown in a measure by all unsympathetic sons and daughters to their unsympathetic fathers and mothers; and these, in

* Even the doctrine so many preach but so few practise "Care for others as for self," would be somewhat unsatisfactory if our care of self was insufficient; it ought then to run "Neglect the rights of others as you are careless of your own."

* So only that it be not so full as to give the little arrows but a narrow space to turn in; for so cannot the young idea be daily taught to shoot.

their latter days, suffer from transmitted callousness in proportion as they have been callous in the treatment of those around. Browning's versified story 'Halbert and Hob' typifies this truth."

We turn next from altruism in the family to altruism as an essential part of social conduct.

(To be continued).

HOW TO GET STRONG.

(Continued from p. 201.)

If the muscles of the abdomen and loins are neglected, even by men, in this country (women cause *all* the waist muscles to become atrophied by carefully packing them inside corsets) the muscles at the sides of the waist are still more thoroughly overlooked. Of course, even in holding the body erect they are in some sort employed; but nothing is done to give them systematic work. Our sculptors, save as they follow Greek models, seem almost to have given up the idea that there are any side muscles to the waist; and certainly a man who should be seen in the bathroom with such muscles as the Greek sculptors show at either side of the waist, would be at once recognised as probably a professional athlete.

Yet these side muscles of the waist are of great importance. It is owing to their weakness that we so seldom see an erect and graceful carriage, especially in walking. If our women wobble in walking, as most of them do, it is chiefly because of the weakness of these muscles. Of course in their case stays are the true cause of the weakness, and they are for the most part to be pitied rather than blamed, since ninety-nine out of a hundred of those who have been brought up to trust in stays, are unable to get rid of them without an interval of discomfort which few are brave enough to undergo. But men (not being among those military monstrosities which "An Observer" once commended to our approval) can very readily correct this side weakness which so seriously impairs their gait. Alike in open air exercise and in indoor work, the proper means of correcting this weakness can readily be obtained.

Thus even in walking, if attention is directed to the uprightness of the body, especially when one side is loaded, as when a heavy hand-bag is carried, the side muscles get useful exercise. Hopping has been recommended as very useful for these muscles; but hopping is not an exercise which can be pursued with any great amount of zeal in public, especially by those who most need the steady development of the neglected waist muscles. A middle-aged gentleman hopping, as Mr. Blaikie recommends, half-an-hour at a stretch (on his way to his office for example) might conceivably attract some degree of attention. Even in a garden he might be suspected of kinship to that next door neighbour who exercised in this way his side waist muscles, on a wall, for Mrs. Nickleby's edification. Even the best way of carrying weights for the special purpose now in view, namely by shouldering them (which is also the easiest and most graceful) is not available for business men, giving too much the look of the hired porter. The superiority of the attitude artistically may be recognised by asking your graceful (though I fear corsetted) daughter to walk across a room with a jar poised on her shoulder, holding it above by the hand and arm curved into such a form as Miss Anderson has shown her, and afterwards asking the same young lady to carry the same jar across the room dangling by her side. But possibly a rather stout City man poising however gracefully a valise on his shoulder, would look less lovely, and might invite unpleasing comment from the ubiquitous street-boy.

But within the house even paterfamilias may attitudinise gracefully without attracting undesired admiration. The number of exercises available for increasing the development (but not necessarily the size) of the side muscles of the waist, is considerable, and most of these exercises are pleasing. Here is one. Place a hand palm upwards, under the cane seat of a light bedroom chair. Then raise your arm steadily till it extends horizontally—beside you, not in front. Now holding the body steadily but not too rigidly upright, raise and lower the hand with the chair on it, while with the other hand and arm you make graceful wavings *ad libitum*. Next, keeping the palm of the hand horizontal, bend the arm and hand so that the fingers point backwards. Then push the hand steadily upwards till your arm extends straight towards the ceiling. Lower the chair and revert to the former position,—arm horizontally extended beside you,—easily and lightly, keeping the other arm and hand moving freely and easily about, not resting the hand on your hips as you feel tempted to do owing to the strain on the weak side muscles of the waist on that side. Next go through the same movements with the other arm and hand. After a little practice, you may take a light chair on each hand, and go through the same movements with both at once. Skill in doing these exercises neatly and gracefully is worth acquiring in itself, and aiming at it makes the exercises interesting.

Lighter, but very pleasant and amusing exercise for the side muscles of the waist, may be obtained by throwing a light, soft ball (at first, or you may use a hard one if you have no objection to raps on the head) from one hand to the other over the head. The swaying motion of the body as you reach out to take the ball on one side and on the other gives capital work to the side muscles of the waist. Practise till you can throw the ball over the head and catch it without looking at it. This is not so easy to do, as it looks when done: probably before you attain certainty in the art you will find your waist muscles have been effectively strengthened and also limbered: for this exercise does both at once; one set of side muscles is tautened while the other is relaxed, and *vice versâ*.

Now for an exercise of a severer type.

Take the clubs one in each hand. Standing with the legs slightly apart, sway the clubs upwards towards the right, so that starting from being both near the ground in front of you they are swung till your arms are both directed some 45 degrees above the horizon the clubs touching each other and both pointing the same way as the well stretched arms which bear them. The body of course must yield to the sway of the clubs, and leans well over towards the right. Then sweep down the clubs in front of you (by which time the body has come upright) and over upwards towards the left, in the same position that they had before had on the right. Then back to the right, then to the left, the body swaying well over each time towards right and left as the clubs are alternately swung upwards on one side and the other. Note that at the end of each swing the face should be directed full towards the side to which the clubs have been swayed. Continue the exercise as long as you find it pleasant. The swaying motion is agreeable enough till you are getting tired, and the exercise is as pleasant as profitable,—unless you are awkward enough to catch your toes with either club, in which case you may desire a pause for rubbing and reflection,—the reverie leading to the resolution to avoid that error in future.

Putting up tolerably heavy dumb-bells, that is thrusting them straight upwards from the shoulder, is another exer-

cise admirably adapted to strengthen the side muscles of the waist. It is best to push them up alternately, and as each approaches its highest point to sway the body over towards the other side so that the upraised hand comes vertically over the other which should then be close to the chest. The upraised hand may then be lowered till it almost reaches the contrary shoulder. It is then brought to the chest on its own side, and the other hand is raised. And so on alternately. The steady swaying of the body from side to side in this exercise is at once pleasant and beneficial.

(*To be continued.*)

THE PERMANENCE OF THE DOMESTIC INSTINCT IN THE CAT.*

THAT the common cat would return to its primitive feral state, if a company of them of both sexes were turned loose in a region where they could have no access to mankind, is probable, and perhaps certain. But that the domestic instinct has now become an integral characteristic of the species, is a matter that will hardly be questioned by any one. Still, opportunities to test the real permanence of this instinct are often presented. I had the good fortune, however, to meet with an interesting opportunity of this kind during the prosecution of my field work for the U.S. Geological Survey in the summer of 1883.

In the prosecution of that work I made the journey, together with my party of three other persons, in an open row-boat, from Fort Benton, Montana, to Bismarck, Dakota, a distance of more than 1,000 miles; nearly 600 miles of that journey being through a region which has so few inhabitants that no post-offices were established there. The only white people in the region were a few buffalo hunters, and the woodman who supplied the few passing steamers with fuel, and even these persons were rarely seen upon our journey.

Upon making our camp one night, about 100 miles above Fort Peck, a good-sized black-and-white male cat came to the boat, and, although shy at first, soon manifested gratification at meeting us. I at first supposed that he belonged to some settler, but on examining the neighbourhood no trace of the recent presence there of white men could be detected. When we started upon our journey in the morning we left the cat on shore, but he followed along the bank, mewing piteously to be taken on board. The boat was headed for the shore, and as soon as it touched the bank the cat jumped on board, evincing delight at being in our company. For the next 100 miles he was our companion, and we became very much attached to him. He was extremely neat, and never soiled the boat in any respect while we had him. At times he would ask plainly, by such signs as he could command, to be set on shore; and then he would hurry back again for fear of being left. He was gratified with our attentions to him, and purred approvingly when we caressed him.

We would have gladly taken him to Washington with us, but as that was impracticable, we decided to leave him at the first place which should seem to offer him a good home. Upon reaching Fort Peck, which is now only an Indian trading-post, we found only one white man there, who was in charge of the store. To him we told the story of our cat, and begged him to give the wanderer a home. He consented, and upon going to the boat he at once declared our cat to be one which he, in company with a party of buffalo-hunters, had the year before taken to the place where we had found him. He said the cat was absent, probably on a hunt, when they broke their camp, and so Jerry, for that was the name we had given him, was left there, and had spent the year alone in the wilderness, hunting his own living.

Of course, I cannot say that Jerry had not associated with mankind in all that time, but the circumstances favour such a conclusion. If he had, after his abandonment, taken up with any settler, it seems hardly probable that he would have been so eager to join us. He seemed quite conscious that we would take him away from his haunt; and this shows that he had formed no attachment to either persons or locality there. The love of locality is doubtless more observable in the cat than the love of persons; while the reverse is true of the dog. Cats are never so demonstrative in their expression of attachment as dogs, and I suspect that the associations of human domesticity has much to no with the attachment to locality which cats manifest. It is true that Jerry was an unusually intelligent cat, and his case may be a somewhat exceptional one. But I cannot help thinking that there is yet much for us to learn of the psychology of this alleged well-known animal.—C. A. WHITE.

* From a paper by C. A. White, read before the Biological Society of Washington.

ELECTRO-PLATING.

III.

By W. SLINGO.

THE closing paragraph of the previous article declared the current to be of the same strength in all parts of the circuit. But we must not, as we pointed out, expect that if we pass a current from a given battery first through one electrolytic cell, and then through 50 similar cells, that in the second experiment we shall get 50 times the amount of decomposition wrought in the single cell. This amount is fixed definitely, and varies as the strength of the current. Thus, if in substituting 50 cells for 1 cell we were to increase the battery proportionately, we should get a 50-fold deposit. Increasing the number of the cells does not necessarily proportionately increase the current strength, which varies as has been previously shown as the ratio between the electro-motive force and the total resistance. Supposing, however, that we modify our battery so as to maintain a current of one ampère (or unit strength) in the circuit and pass the current first through one cell and then through 50 cells we shall in the second case deposit the same weight of metal in each cell as was deposited in the single cell. Two questions here present themselves. First, what amount of metal is a current of given strength capable of depositing in a given time? Secondly, will equal currents deposit equal weights of all metals? The answers are based on experimental as well as theoretical grounds. It has been found that a current of one ampère passing through acidulated water for one second of time deposits or sets free ·0000105 grain of hydrogen gas. As water contains one volume of oxygen to two of hydrogen, it follows that if they were both of equal weight, bulk for bulk, half the above quantity, or ·00000525 grain of oxygen, would be released. But oxygen has not the same weight as hydrogen, nor, in fact, has any other substance. A pint, quart, litre, &c., of oxygen weighs 16 times as heavy as a similar volume of hydrogen. Consequently, for every ·0000105 grains of hydrogen set free, $\frac{16}{2}$ or 8 times as much oxygen is evolved. An ampère thus liberates ·000084 grain of oxygen; 8 is known as the

chemical equivalent and ·000084 as the electro-chemical equivalent of oxygen.

Subjoined are the electro-chemical equivalents of such of the more important elements with which we may have to deal during the course of these articles :—

Copper	·0003307
Gold	·0006875
Iron	·0001470
Lead	·0010867
Nickel	·0003097
Silver	·0011340
Tin	·0003097
Zinc	·0003412

In other words, if a current of one ampère be passed through the various electrolytic cells for one second, the above fractions of a grain would be deposited.

If water be decomposed, and the resultant gases collected separately, and the two platinum electrodes be disconnected from the battery and joined to a galvanometer, the passage of a current will be evidenced. The gases have, therefore, a difference of potential which ceases as they reassume the combined or aqueous state. It will be apparent that if the separated gases have an electromotive force or difference of potential, the current which parts them must have a still higher electromotive force, otherwise no separation would ensue. For this reason weak currents may be passed through electrolytic cells without any apparent effect. The "back" electromotive force between hydrogen and oxygen is 1·45 volts, so that at least two Daniell cells are necessary to decompose water, the electromotive force of such a cell being only 1·079 volts. As the resistance of electrolytic circuits is never, under ordinary circumstances high, a few cells are therefore all that are required to overcome the electrical resistance of the circuit and the chemical affinities of the substances to be separated. Four volts (the EMF. yielded by two good bichromate cells) will, as a rule, be ample.

Having now secured an insight into the general principles governing electro-deposition, let us turn our attention to the practical application of those principles.

The more generally deposited metals are copper, silver, and nickel, of which the latter has recently made extremely rapid strides, more particularly since the dynamo-machine has been utilised to produce the necessary current. Copper is, however, the metal which will for the present engage our attention. It is manifest that in order to procure an equal homogeneous coating, the metal must be deposited regularly and uniformly. An unsteady and overpowerful current produces an extremely irregular surface and structure with the result that the copper is unmanageable and breaks to pieces or peels off the substance on which it may be deposited. Originally copper plating and typing operations were conducted on the single-cell process—that is to say, a Daniell cell* minus the copper plate was employed, the substance to be coated being substituted for the copper.

With small objects to be coated, this process may be easily adopted. Procure an earthenware or other non-conducting waterproof vessel of, say, a gallon capacity. Into this place a porous earthenware pot a little taller than the jar, and about 3 inches in diameter. In the porous pot place the zinc, which may be either in the rod or other convenient shape. If the experimenter has some odd pieces of zinc, he may easily cast a good electrode. There is often an objection to cast zinc, but the presence at the bottom of the cell or two or three ounces of mercury will soon put such matters right, more particularly if the zinc has been previously amalgated.* The casting may be in the form of a rod flattened out at the lower end. The zinc should, for nearly its whole length, be cast round a piece of stout copper wire, which serves the double purpose of supporting the zinc and providing an inexpensive means of connection. Instead of putting a copper plate in the outer cell, suspend the substance to be coated in a saturated solution of sulphate of copper, brine or some other saline solution being placed in the zinc division. On connecting the copper wire attached to the zinc with the suspended article a current flows from the zinc to the outer cell, and in passing through the sulphate of copper, splits it up, and metallic copper is deposited on whatever forms the negative electrode. One or two ounces of strong sulphuric acid should be added to the copper solution. The process is a slow one, and the size of the object is limited. It is still, however, often resorted to. A separate cell for deposition is better, and permits the use of batteries of other forms and of various strengths. In choosing our battery, a great deal depends upon the work to be done. If single small objects have to be coated, one Daniell or other cell will suffice; but where the object is large or where the shape is irregular, larger and stronger currents are necessary than can be obtained from such a source. To this subject, as well as to the preparation of the object itself, our attention must next be directed.

* A description of the Daniell cell and its action may be found in KNOWLEDGE, No. 83.

* This process has been previously described. See KNOWLEDGE, No. 88. The process consists in well washing the zinc in dilute hydrochloric or sulphuric acid, and then, the zinc being still wet, rubbing in a little mercury, which effectually homogenises the surface, and so prevents local action.

ICHNEUMON FLIES.

By E. A. Butler, B.A., B.Sc.

THE word "fly" is of extremely wide popular application; used for all sorts of dissimilar insects, it unfortunately frequently suggests affinities which do not exist. It would be an advantage if its application could be restricted to the flies proper, two-winged insects constituting the order Diptera, several representatives of which are household pests during the summer. The present insects have nothing whatever to do with the true flies, but belong to the Hymenoptera, the order that includes bees, wasps, ants, &c. They form a distinct and most remarkable section of this order, noteworthy for their slender make and strange habits of parasitism. They are the pirates of the insect world, maintaining their own existence by levying blackmail upon their fellow-creatures.

Airy, graceful, and sylph-like, they are yet so thin in body, long in limbs, and restless in habits that one is apt to regard them with some degree of suspicion as creatures possibly dangerous, whose too close acquaintance it may not be desirable to cultivate—an impression which is in some cases deepened by the formidable aspect of a long ovipositor. In slenderness of make they are without rivals in the insect world. They have usually long, narrow bodies; very long, slender, and tapering antennæ; long, slender legs; and four transparent wings, similar in texture and general form to those of bees, but different in the arrangement of the nervures, as will be seen by comparing Fig. 1 with the bee's wing figured in our first paper on Wild Bees. The abdomen is much pinched in at the waist, and attached to the thorax by only a very slender junction, through which all communication has to be kept up between the anterior and posterior parts of the body. It sometimes carries at its apex three

thread-like processes, which are occasionally even longer than the body itself (Fig. 2). These constitute the ovipositor, and consist of a central composite hard and stiff boring organ and its more flexible sheaths. The whole looks a formidable weapon, and the borer is indeed sometimes so hard and sharp-pointed as to be able to inflict a smart prick, but it is not a sting, *i.e.*, no poison gland is attached to it, and its main function is to make suitable perforations for the lodgment of the eggs and to guide them

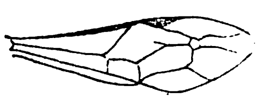


Fig. 1.—Forewing of Ichneumon grossorius.

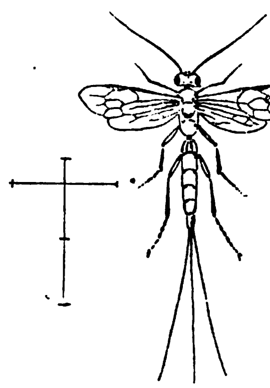


Fig. 2.—Glypta lugnorina.

into their place of deposit. It is scarcely necessary to add, therefore, that it is an appendage to be found only in the females. The head carries the usual two masses of compound eyes, which are of large size, and suggest superior keenness of vision and a general wide-awakeness in excellent keeping with the rapidity of movements so characteristic of the insects. Between the compound eyes are the ocelli, or simple eyes, three small highly-polished knobs, triangularly disposed, and directed skywards.

The majority of these insects are, in their earlier stages, parasitic upon the Lepidoptera, or butterflies and moths, which they usually seek in the caterpillar state. Having found a suitable caterpillar, the ichneumon fly mounts its back, and, bending its own body, brings its ovipositor down on the surface of the unsuspecting grub. By a little pressure the sharp point of the borer is driven through the poor creature's skin, producing, however, apparently, no greater discomfort at the time than a slight tickling sensation. The perforation made, an egg is inserted into the body of the victim sufficiently far to prevent the possibility of its removal with the skin when the caterpillar effects its periodical moults. Frequently a large number of eggs are laid in a single caterpillar, but in many cases only one is deposited, the number depending in large measure upon the size of the ichneumons they are destined to produce, a matter requiring careful consideration as the supply of food is limited to what is or will be contained within the body-walls of the caterpillar. From the moment of the insertion of the egg, the caterpillar is a doomed being, though for a long time there may be no premonitions of its fate. The ichneumon grubs, which soon hatch from the eggs, as soft, white, fleshy creatures with cylindrical bodies and without feet, devour the stores of fat laid up by their host, which it was intended to use in the elaboration of the additional organs of its own future form. These depredations, however, do not affect any vital part, and the creature goes on living, eating, and growing, while the insidious parasites are also flourishing upon the store of nutriment with which it unintentionally provides them. Matters go on thus until the ichneumon grubs are nearly ready to assume the pupa state, a period in their growth which sometimes almost synchronises with the corresponding state in their host. When this is the case, the latter has by this time begun to manifest symptoms of discomfort and disease, ceasing to eat, and becoming more and more feeble until it finally dies of exhaustion, when the parasites eat their way through its skin, and, on reaching the outside, form cocoons to protect themselves while in the pupa state, which they group round the carcase of the caterpillar whose vitality has now passed into their own bodies. So the development of the caterpillar is arrested before it can enter the chrysalis stage, and it therefore never fulfils its destiny. More generally, however, the ichneumon maggots do not mature quite so quickly, and the larva manages to pupate before they make their exit from its body. Still the result is in the end the same; the creature dies prematurely, never reaching the stage in which it can provide for the perpetuation of its race, and so passes away "without issue." Now, as the caterpillars of the Lepidoptera are almost exclusively devourers of vegetable substances, the ichneumon flies, by preventing their too rapid increase, become the saviours of vegetation, and may therefore be welcomed by the agriculturist. It is difficult to discover to how great an extent vegetation is preserved by these means; comparatively little attention has, until quite recently, been given to the ichneumons that inhabit this country, partly, no doubt, owing to the extreme obscurity of many species and their very close resemblance to one another. It is impossible to give an accurate estimate of the number of species we possess, as large numbers of new ones are being discovered year after year; still the number already recorded amounts to more than 2,000. Some of these are very abundant in individuals, and if we remember that every single ichneumon amongst the larger kinds, at least of those that prey upon the Lepidoptera, and every score or so of the smaller ones, means the destruction of a caterpillar, we can see what enormous accessions to their ranks the butterflies and moths would receive, if only the ichneumon flies were blotted out of existence. A large proportion of every brood is no doubt exterminated in this way, and the perils of larvadom amongst the Lepidoptera must indeed be great.

(To be continued.)

Reviews.

JOULE'S SCIENTIFIC PAPERS.*

THE Physical Society of London has conferred at least as much honour upon itself by the republication of these papers of Dr. Joule's as it has upon the great Englishman to whom science is so largely indebted for the establishment of the doctrine of the Conservation of Energy upon an irrefragable basis. Within the lifetime of many who will read these lines, heat was regarded as an actual physical entity; and the "Caloric" of the works published thirty or forty years ago must be well within the recollection of a considerable proportion of middle-aged men. That heat is merely the energy of motion was more than suspected by Rumford and Davy at the close of the eighteenth century; but it is practically in Dr. Joule's application of Faraday's magneto-electrical discoveries to illustrate the mutual convertibility of heat and mechanical motion, that the now universal acceptance of the Kinetic theory of heat had its origin. The mass of scientific papers reproduced in the 657 pages of the volume before us, commence, in the year 1838, with a description of a form of electro-magnet devised by the author; in fact, for the first four or five years of his

* The Scientific Papers of James Prescott Joule, D.C.L. (Oxon), F.R.S., &c. Published by the Physical Society of London. (London: Taylor & Francis.) 1884.

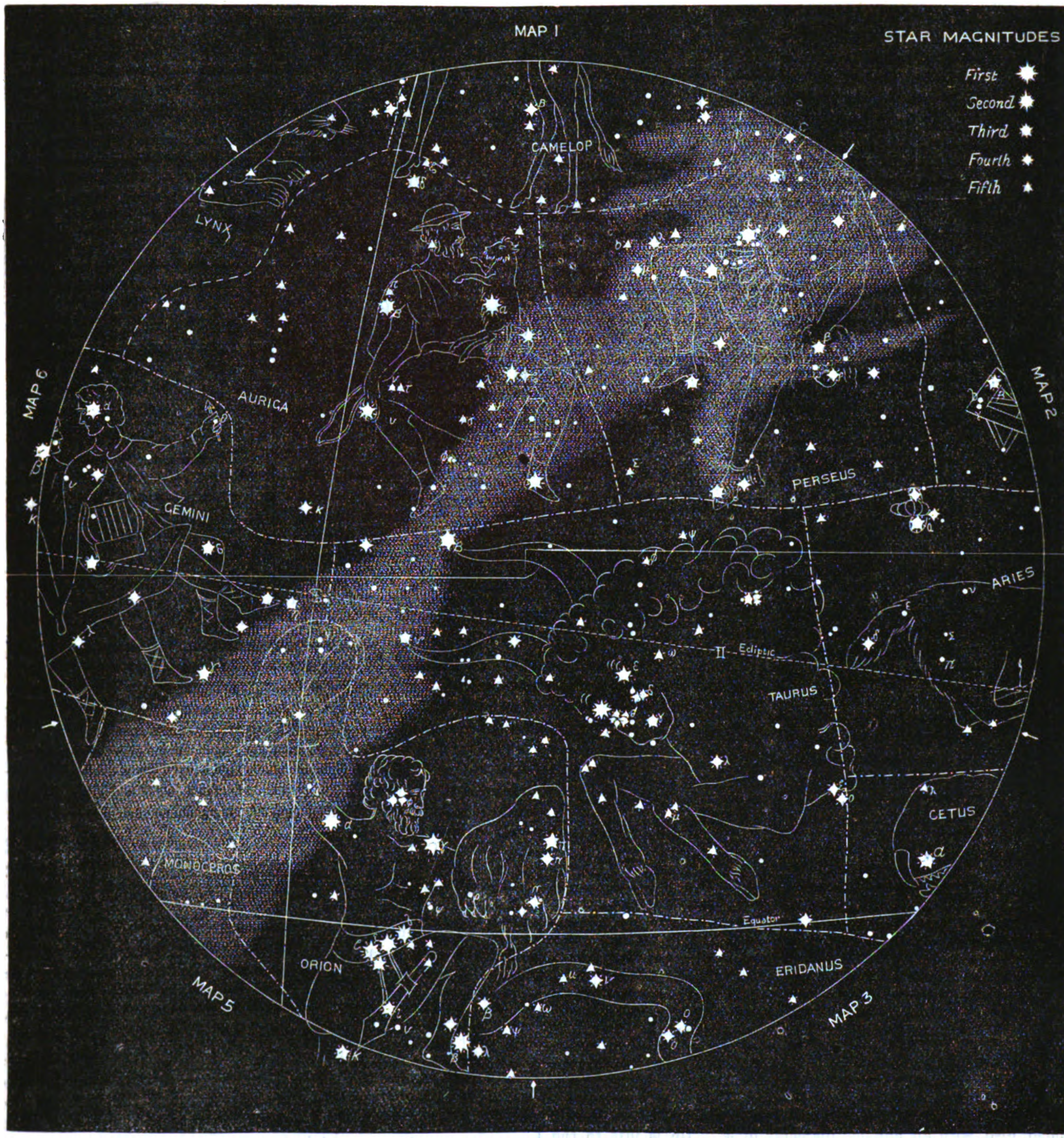
scientific life, Mr. Joule would seem to have mainly devoted his powers to research in electro-magnetism and Voltaic electricity. Soon, however, commenced that splendid series of investigations which will render his name imperishable until the decay of Physical Science itself; and the student of these pages will read at once with interest and admiration the description of the, seemingly, absurdly simple apparatus by the aid of which such mighty and enduring results have been attained. As may be gathered from the report of a lecture on pp. 265 *et seq.*, given by Dr. Joule, "On Matter, Living Force, and Heat," he had succeeded in establishing the Kinetic theory of heat as early as the year 1847. The popular exposition of that theory which he then gave might be transferred verbatim to any treatise in physics published during the present year. *Facile princeps* in the experimental investigation of thermo-dynamics, his collected essays on the whole subject must, perforce, be studied by every one who is desirous of a thorough insight into it. Naturally the results he obtained led him, among other things, to the discovery of the true theory of shooting stars, viz., that they are minute planetary, or *quasi*-planetary, specks of matter which ignite and are dissipated in vapour from the heat engendered by their friction against our atmosphere, as gravity draws them towards the earth; a theory since so abundantly confirmed. But while thermo-dynamical research may be held to have been the chief end of Dr. Joule's scientific life, yet his versatile intellect appears to have ranged far and wide over the field of physical science, and we find him observing, investigating, and experimenting upon such diverse subjects as the Velocity of Sound, the Utilisation of London Sewage, the Intensity of Light during a Solar Eclipse, Amalgams, Air-pumps, both compressing and (mercurial) exhausting, Mirage, the Fall of a Meteor, A New Balance, an Automatic Arrangement for Steering Ships, Sensitive Barometers and Thermometers, Solar Photography, an Abnormal Appearance Presented by the Sun at Sunset, Some Physical Properties of Beeswax, Hydrophobia, Kites, and Checking the Oscillations in a Telescope. Few men living are better entitled to exclaim with Horace, "Exegi monumentum ære perennius," than the author of this volume; whose noble, intellectual features are given in counterfeit presentment in the beautiful engraving which forms its frontispiece.

MR. BUTLER'S SELECTIONS.*

THIS remarkable and, in many respects, important work is primarily constructed on the principle of a sandwich, commencing as it does with amusing extracts from its author's former curious production, "Erewhon," and terminating with equally light and pleasant reading from his "Alps and Sanctuaries;" while between the two appear a series of Essays on Life, Psychology, and Evolution, which must be regarded as a contribution to biology that it will be impossible in the future to ignore or even to neglect. Within these two covers we may find material either to while away agreeably, and far from uninstructively, an hour in a railway-carriage, or to occupy all the thought and attention that the student can devote to it in the solitude of his own library. As "Erewhon" is a book which must by this time have been pretty widely read, it may suffice to say here that its author exhibits much of Swift's vigorous power of satire, without the coarseness which disfigures not a little of the work of the Dean of St. Patrick's. The chapter on "The Musical Banks" will probably require to be read over more than once before its allegorical signification will be fairly realised. When, however, such apprehension has been attained, the exceeding wit with which the idea is carried out cannot fail to commend itself to the reader. So again in the extracts from "Alps and Sanctuaries," which, amid pleasant and sparkling descriptions of North Italian scenery and people contain sententious and epigrammatic little passages to be committed forthwith to one's common-place book. It is, however, to our author's doctrine of Evolution that we would chiefly urge attention; destined, as we believe it to be, to work no inconsiderable change in accepted ideas. Mr. Butler contends that instinct is really inherited memory, and that there is a mental (as, merely as a matter of course, there is a physical) continuity between man—or any other animal—and his most remote ancestor. Moreover, he attributes the origin of species not, with the immortal Charles Darwin, to natural selection operating upon absolutely chance variations in existing forms, but to deliberate will and purpose on the part of the creature itself. Thus our author would say that the progenitor of the giraffe tried hard to bite the highest leaves on the trees; and so in process of generations lengthened its neck. If now, from any cause, a scarcity of leaves arose, such animals as could browse upon the higher ones would, of course, live on after the death of their less cervically favoured congeners, who had finished up all those at a lower level—and here, of course, the doctrine of natural selection would come in. We need scarcely add that this is substantially the doctrine of Lamarck and the elder Darwin in the last century. Professor Hering, too, may be regarded as the modern pioneer of the theory that instinct and ancestral memory are identical. This certainly invests the vague term "heredity" with a fixed and definite meaning which it does not otherwise possess. We will not, however, even attempt to summarise Mr. Butler's conclusions; trusting, as we do, that every one interested in the subject will study them in the original. A discussion with Mr. Romanes occurs as an episode in our author's exposition, in which, we venture to think, Mr. Romanes comes off very decidedly second best. *Inter alia*, we find Mr. Romanes quoting against Mr. Butler a statement as to instinct and inherited memory made by Canon Kingsley, in *Nature*, for Jan. 18, 1867, which well-meant advertisement, however, for a paper so conspicuously adorned by Mr. Romanes's own contributions, rather breaks down in the face of the fact that the first number of the periodical in question did not appear until some three years afterwards! We trust that the "Psalm of Montreal" will disappear from the inevitable second edition of this clever, thoughtful, and readable book.

* "Selections from Previous Works. With Remarks on Mr. G. J. Romanes's 'Mental Evolution in Animals' and a Psalm of Montreal." By SAMUEL BUTLER. (London: Trübner & Co. 1884.)

SAGACITY OF THE HORSE.—On my farm, one Sunday, the house was left in charge of one man, who sat on the porch reading. A mare, with her young foal, was grazing in the orchard near by. At length he saw the mare coming from a distant part of the orchard at full speed, making a loud outcry—a sort of unnatural whinny, but, as he says, more like a scream of distress than the natural voice of the horse. She came as near to the man as the fence would allow, and then turned back for a few rods, and then returned, all the while keeping up the unnatural outcry. So soon as he started to follow her she ran back in the direction of a morass or miry place which had been left unguarded, and only stopped on its very brink. The man hastened to the spot with all speed, and found the colt mired in the soft mud and water. It was already dead.—*J. D. Caton, in American Naturalist.*



MAP OF CONSTELLATIONS.

By Richard A. Proctor.

As an experiment, I give this week a map of those constellations which fall in the fourth map of my School Atlas, with the figures of the various animals or persons associated by the ancients with these star-groupings. Now that I have seen the engraving I recognise several improvements which can be conveniently introduced. The accompanying map must therefore be regarded as merely tentative. A series of twelve maps such as this, but improved and made more effective in various ways, will I think be valued by many students of the heavens, who, however little they care for the preposterous figures which sprawl over too many of our celestial globes and atlases, yet like to be reminded of the position which the ancients assigned to these strange celestial beings, and even find the poetry of the heavens incomplete without them.

Editorial Gossip.

In consideration of the kindness with which our numerous constituency have accepted the change of price (and size) which we have recently been almost forced to introduce, we propose, among other improvements (which will be arranged as quickly as they can be afforded), to have Knowledge printed on thicker and better paper. The paper ordered

for the purpose will be ready we are told by the first week in May.

We invite suggestions as to the series of Constellation Maps, of which a specimen is given this week. The object being chiefly to show stars and constellation *figures*, we believe the series will be much improved by the omission of all lettering, (even the names perhaps of the constellations), meridians, declination parallels, constellation outlines, and so forth. Then we think that outside the circular map the ground should be not altogether so dark as within the map itself.

THE FACE OF THE SKY.

FROM APRIL 11 TO APRIL 25.

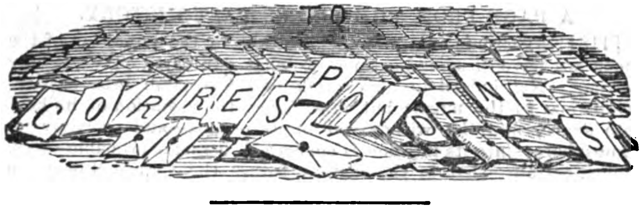
BY F.R.A.S.

THE sun continues to exhibit interesting spots (though faculæ are fewer), and should, as heretofore, be looked at on every clear day. The aspect of the visible night sky is shown on Map IV. of "The Stars in their Seasons." Mercury is an evening star, and attains his greatest elongation east of the sun (20° 21′) in the afternoon of the 25th. About this time he may be caught with the naked eye over the W.N.W. part of the horizon after sunset. Venus is a splendid object in the evening and night sky, where she is travelling through Taurus ("The Stars in their Seasons," Map I.). On the night on which these notes terminate she will not set until about a quarter of an hour before midnight, a very unusual spectacle. Mars is visible all night, but is becoming a very small object now. He is still in Cancer (Zodiacal Map, p. 70). Jupiter should be looked at as soon as it is dark enough, as he is approaching the west. His angular diameter is diminishing, but he is still a noble object. The shading on his following limb (p. 102) is now quite obvious to the careful observer with the telescope. The planet is in the confines of Gemini and Cancer (Zodiacal Map, p. 40). A good many interesting phenomena of his satellites are observable during the next fourteen days, before 1 a.m. Beginning with the 14th, Satellite III. will reappear from eclipse at 7h. 53m. 34s. p.m., and Satellite II. be occulted at 10h. 17m. On the 15th the egress of the shadow of Satellite IV. will occur at 10h. 39m. p.m., and Satellite I. will disappear in occultation 50m. after midnight. On the 16th the shadow of Satellite II. will leave Jupiter's disc at 10h. 8m., at the same time that Satellite I. enters on to the opposite limb. This will be a pretty and curious sight. The shadow of Satellite I. will come on at 11h. 25m. p.m., and the Satellite casting it leave Jupiter's face at 12h. 28m. On the 17th Satellite I. will reappear from eclipse at 10h. 54m. 5s. p.m. On the 18th the egress of the shadow of this same satellite happens at 8h. 14m. in the evening. On the 21st Satellite III. will disappear in eclipse at 8h. 23m. 10s., travel through the planet's shadow, and reappear from eclipse at 11h. 53m. 55s. p.m. On the 23rd Satellite IV. will reappear from occultation at 8h. 15m. p.m. The ingress of the shadow of Satellite II. begin at 9h. 50m.; Satellite II. itself leaves Jupiter's opposite limb at 10h. 13m. p.m.; and Satellite I. enters on to the planet's face three minutes after midnight. On the 24th Satellite I. will be occulted at 9h. 15m. p.m.; while finally, on the 25th, this same Satellite I. will pass off the disc of Jupiter at 8h. 52m., followed by its shadow at 10h. 9m. p.m. Saturn has, for the observer's purpose, left us for the season, as has Neptune; but Uranus may be seen all night long. In the telescope he presents the appearance of a small bluish disc with an angular diameter of 4″. He is just to the north and west of β Virginis (Zodiacal Map, p. 165). The moon is 15·3 days old at noon to-day, and quite evidently her age will be 29·3 days at the same hour on the 25th. One occultation only—and that of a very small star—will happen during the next fourteen days. It occurs on the night of the 13th, when the star B.A.C. 5408, of the 6½ magnitude will disappear at the moon's bright limb at 11h. 54m. at an angle from her vertex of 57°. Its reappearance at her dark limb will not take place until 13 minutes past 1 o'clock the next morning, and will happen at an angle of 239° from her vertex. At noon to-day the moon is in the eastern confines of Virgo, out of which constellation she passes into Libra about 5 p.m. She is travelling across Libra until 6 o'clock in the afternoon of the 13th, when she enters the northern narrow strip of Scorpio. At 6 a.m. on the 14th she quits this for the southern part of Ophiuchus, her journey through which occupies her until 3 a.m. on the 16th, at which hour she enters Sagittarius. It is 5 p.m. on the 18th ere she leaves Sagittarius for the north-western portion of Capricornus, which it takes her until 9 o'clock the next morning to cross. At the hour last named she enters Aquarius, and is travelling through that constellation until 4 a.m. on the 22nd, when she crosses into Pisces. About 2 a.m. on the 25th she enters Aries, where she is still to be found when our notes terminate.

A CORRESPONDENT of the *Daily News*, speaking on the erection of a telegraph line in China, says:—Little knots of people may frequently be seen gathered round a post listening with gaping astonishment, but with the fullest confidence, to some wiseacre who is pretending to explain the *modus operandi*. He tells them that there is a devil in each post, that the posts are placed close enough together for the devil in one post to make himself heard by the devil in the next, that the wire is placed on the top of the posts merely to keep them in an upright position; anything of a slant would make the devil inside feel very uncomfortable. These devils, of course, speak a language which is both unintelligible and inaudible to the uninitiated; "foreign devils," however, understand it well, and so to send a message is a very simple thing. A "foreign devil" speaks it to the devil in the first post, he passes it on to the next, and so it reaches its destination, where another "foreign devil" receives and interprets it. Other explanations are sometimes given, but all of them are equally wide of the mark.

ACCORDING to *Kranick's Zeitschrift für Metallwaaren Industrie*, the introduction of steam into enclosed spaces for the purpose of extinguishing fire has been successfully tried in Berlin. The owner of a steel-pen factory in that city, in consequence of repeated outbreaks of fire in the drying-room, had steam pipes placed in three of the rooms, this appliance being shut off by short soldered pipes of an easily-flowing alloy of lead and tin, arranged to work automatically. One day a hissing noise made the fireman aware that one of these appliances had been called into action. It was found on investigation that the contents of the drying-room had become ignited, but that the steam thus set free had extinguished the fire before it could spread.

THE DUTY ON WORKS OF ART.—The Tariff Act of 1883 advanced the rate of duty on paintings in oil or water-colours, and on statuary, to 30 per cent. *ad valorem*, instead of 10 per cent., as it had theretofore been. The imports under this head for the fiscal year 1882 were 2,550,000 dols., and the late Tariff Commission recommended a duty of 40 per cent. Representative Perry Belmont, of New York, has now introduced a Bill wholly exempting from duty works of art, ancient or modern, the term to be construed as including all paintings, drawings, and photographs, and statues of marble or other stone. The argument favouring a high duty on this class of imports can have no other foundation than that such goods are brought here only by the rich, as luxuries, and for that reason should pay as high a revenue to the Government as possible. But there is another side to the question. Works of art are educators of the people, and, in public galleries or private collections, they exert a far-reaching influence in elevating the taste and exalting the ideals which touch the mainsprings of human life. No question of protection or of free trade can enter into any consideration of placing a duty on such products, for the American artists are strenuous supporters of the Belmont Bill, and the most of them, also, feel it a necessity of their education that their opportunities for studying European work, modern as well as ancient, shall be as free and unrestricted as possible. This, therefore, seems to be a case where we should adopt Goethe's saying, "Encourage the beautiful; the useful will take care of itself," to the extent, at least, of allowing artists' work to be imported duty free.—*Scientific American.*



"Let Knowledge grow from more to more."—Alfred Tennyson.

Only a small proportion of Letters received can possibly be inserted. Correspondents must not be offended, therefore, should their letters not appear.

All Editorial communications should be addressed to the Editor of Knowledge; *all Business communications to the* Publishers, *at the Office, 74, Great Queen-street, W.C.* If this is not attended to delays arise for which the Editor is not responsible.

All Remittances, Cheques, and Post Office Orders should be made payable to Messrs. Wyman & Sons.

The Editor is not responsible for the opinions of correspondents.

No communications are answered by post, even though stamped and directed envelope be enclosed.

SUPERNATURALISM.

[1179]—Will you allow me to say, with reference to the allusion of one of your correspondents to Mrs. Crowe's "Night Side of Nature," that a book of that kind should not be treated too disdainfully. I do not think it possible for any one to read it through, *without prejudice*, and not feel that the authoress has taken a very great deal of pains to present her materials fairly; and without being a good deal surprised at the enormous amount of her material. Let it be granted—for the sake of the argument—that four-fifths of the anecdotes, since they depend on the veracity of the narrator alone, are comparatively valueless as evidence. There still remains a considerable number in which she has merely transferred, or condensed, from other sources of a more or less public character. I confess that, for my part, I am quite unable to regard all these as susceptible of any sort of rational explanation without the aid of mechanism totally different from that which the world at large recognises as human. I fear that I should esteem but lightly the mental calibre of one who could say he had read Mrs. Crowe's collection conscientiously, though without being one whit nearer a belief in the existence of some such mechanism. As for myself, the strange thing is that though I have been prepared, any time this last twenty years, to accept almost greedily any personal experience which could help me to the truth, I have never been a witness of, or accessary to, anything whatever of an apparently supernatural character. I remain, therefore, a sceptic in despite.

J. Herschel.

STRANGE DREAMS AND PRESENTIMENTS.

[1180]—It not unfrequently happens that persons who relate their sleeping experiences or waking impressions of things which have occurred or are about to occur, such as apparitions of deceased or dying friends, or of other circumstances which they could not possibly have known in the ordinary course of things, are regarded by many as visionaries or dreamers, merely, I believe, because such incidents are not common, and do not happen to all of us. The various cases of this nature which have come under my personal experience and observation have caused me to imagine that the state of the health and of the nerves of the individual who receives the impressions has much to do with it.

Ill-health, whether chronic or temporary, causes the bodily forces to be in subjection to the spiritual part of us, and thus draws aside for a brief period the veil which divides us from the world of spirits. The mesmeric forces (of which we scarcely know more than that they undoxbtedly exist) may also have their influence in causing the stronger mind to act upon the weaker one, who involuntarily, even when at a distance, fulfils something desired by a person of stronger mind or will, and which could not possibly have been communicated to the receiver of the command except in the above manner.

I will now proceed to give some cases in point.

Many years ago, when suffering from severe illness which necessitated confinement to bed or sofa for several months, I perfectly remember lying for some hours in a most singular condition. It was not that of somnolence or exhaustion: I was aware that my body was there on the bed, but my spirit seemed to me to be for a time detached from it, to be in the world of space. During this interval I no longer felt any bodily pain; I was conscious also of the exact moment when the two portions of my being became again re-united.

Is it the mind or the spiritual portion of us which alone can suffer pain, either bodily or mental? In this case it was mercifully given as a period of recreation or refreshment to the bodily powers.

Some few years later, when living in lodgings in London, I awoke about 3 a.m. *en sursaut*, as the French say; that is to say, I suddenly found myself very wide awake, and sat up in bed. Whilst in this position, I heard a voice say, very distinctly, "One of your name is dead." When the first post came in, about 8 a.m., it brought the news of the death of a little nephew in India from diphtheria. The last accounts we had received of him, said he was a splendid, healthy child, of two years and a half; therefore I could have had no idea of his illness even—it was before the days of the telegraph to India.

Again, the child of a friend had had more than one attack of convulsions. I saw its parents one Sunday morn; they seemed quite cheerful about him, and said he seemed quite recovered, for the time at least. We did not meet again during the day. At the evening service I was startled by hearing the little one prayed for by name. I said to myself, "I cannot pray for that child's recovery; I know it is dead." On leaving the church my friend's nurse ran up to me begging I would go up to the mother at once, for the child was dead. As I afterwards learnt, it must have passed away almost at the exact moment that prayer was made in its behalf.

Once more. The day that H.R.H. the Prince of Wales was given over, during his severe illness some years ago, I was at a concert in a provincial town in England. A telegram which had arrived was circulated through the large hall from mouth to mouth to the effect that, "the Prince is dying; he cannot live many minutes." I felt certain that he would recover; some inward voice seemed to tell me so; nor is this the only instance in which I have had a similar experience.

All the incidents above mentioned occurred when I was not in a good state of health from the effects of our damp, autumn climate or other causes.

A very remarkable instance of a strong mind acting upon a weaker one was once related to me by a friend. He was taking his summer holiday in Scotland, and started off one morning for a long day on the moors. After he had gone a considerable distance it suddenly occurred to him that he had forgotten to give the mistress of the farmhouse where he was lodging a very important letter which he had written and intended to post that day. My friend said that this circumstance caused him much annoyance. On consulting his watch he found it would be impossible for him to catch the walking postman even were he to return immediately. He therefore resigned himself to the delay, and proceeded on his excursion, only returning to the farmhouse in time for a late dinner or supper. When he got back to the house, his landlady, on meeting him, said, "Oh, sir, I am so sorry you were at the trouble to return from your walk to remind me to give your letters to the postman. I should have done it without your coming back to beg me to do so."

Cosmopolitan.

APPARITIONS.

[1181]—As you are upon the subject of "Ghosts and Goblins," I venture to submit a simple theory, which, if admitted, would offer a rational account of some well-authenticated stories of apparition.

The brain is constructed in hemispheres, to which the double organs—eyes and ears and nostrils—correspond. It is not uncommon for a person to be blind in one eye, deaf in one ear, for some time before he becomes aware of the fact. I think it will not be questioned that some purely intellectual functions may be healthily discharged, while one hemisphere is structurally injured, as in hemiplegia.

Now, I suggest that this separate action of the hemispheres may be carried so far that one may be in the state of sleep and dreaming, while the other performs functions for the man consciously awake. If this be so, then the man, feeling himself awake, and seeing with certain eye the objects around him, will naturally suppose that he sees also the forms presented by the dreaming hemisphere. The vision will disappear as soon as the shock is sufficient thoroughly to awaken the dreamer.

This explanation would fall in with the facts that ghosts are most frequently seen at dead of night. In applying it to an explanation of well-authenticated stories, I have been struck by the fact that the visionaries have been reduced by continuance of long and hard work to a condition of great bodily exhaustion; the system has demanded the refreshment of sleep, yet some feverishness or strength of will has kept it at least partially awake.

This condition will be seen in the story referred to by Sir Walter

Scott, and explained by Mr. Lockhart, of a ghost, or rather apparition, seen by Lord Castlereagh. (Life of Sir W. S., Vol. VII., ch. 8, sub-anno 1822.)

It was also to be seen in a remarkable story told two or three years ago, with sober confidence, by Dr. Jessopp, in the pages of the *Athenæum*. He had gone through heavy work in the day, and was forcing himself to work late into the night.

It seems to me that I have also myself had experience of the conditions; probably most people have. I have lain awake, painfully awake, unable to get "a wink of sleep;" yet suddenly I have been startled with the consciousness of something just seen, said, or done which could only have been dream-work.

Is it not physically possible that one may be both awake and asleep at the same instant?

It may be that the suggestion I now make may have been made and dismissed before for sufficient reason. I can only say that my information in physiology and physics has not hitherto supplied me with such reason, and, therefore, I am bold enough to lay it before you, and, if you please, your readers.

Leonard H. Rudd.

[Mr. Rudd's idea seems to me well worth examining. I may note, as a curious point in connection with the duality of the brain, that my own experience suggests strongly that the sides of the brain act independently in such matters as recalling past impressions.—R. P.]

REMARKABLE FULFILMENT OF A DREAM.

[1182]—The following account of the fulfilment of a dream (which occurred to my mother several years back, but the memory of which is as fresh with myself as when it happened) may be interesting to your readers.

A quarrel had arisen between a certain gentleman (whom I will designate Mr. M.) and our family. Mr. M. had, up to that time, been one of our most intimate friends. Some time after this quarrel we were seated at breakfast, and my mother told us that she had dreamed that night she had seen Mr. M. in the High-street, and had conversed with him, but she had hardly gone two hundred yards from him when he dropped down dead. Whilst my mother was telling us this, one of my brothers, who had been out early that morning, came in with the news that Mr. M. had had a fit in the High-street at the very place where my mother had dreamed she had seen him fall. Mr. M. was taken home insensible and never spoke again. He died two or three days after he was taken ill.

E. L. R.

THE BLUE SUN.

[1183]—"L. S." is not right in supposing that the fine particles which scatter blue light would appear red if seen on the other side. The phenomena observed is due to the fact that particles the diameters of which are comparable with the wave lengths of the visible spectrum act differently with respect to the waves at opposite ends of the spectrum. A float would be carried up and down by ripples which were long compared with the diameter of the float, while a ship's buoy floating in the same water would hardly be moved by the ripples, because one ripple would tend to undo the work done by another. The float would give rise to secondary waves in the atmosphere corresponding in length with the distance between the crests of the ripples, while the buoy would give rise to no such waves in the air, but would reflect the ripples from its surface, and they would pass onwards in a different direction with a new wave front. The analogy is not quite perfect, but it will probably enable "L. S." to see how short wave lengths may be dispersed by small particles floating in the air, while the energy communicated by longer waves is chiefly absorbed in heating the particles. There is a flaw in "L. S.'s" reasoning with regard to the complementary character of reflected and transmitted light. If a body appears blue by reflected light, it does not follow that it will appear red by transmitted light. For example, ordinary blue glass appears blue when held opposite the sun, as well when held between the sun and the eye. Probably an experiment with regard to the light dispersed by small particles will make the matter clearer to "L. S." Let him dissolve a small quantity of shellac in spirits of wine, and pour a few drops of the liquid into distilled water; a fine precipitate will be formed, which, if held in the sunlight, appears blue, whether held opposite to the sun or in any other position. But the particles forming the precipitate are not blue, as "L. S." will discover, if he adds a little more of the lac dissolved in spirits of wine, and allows the liquid to stand till the precipitate settles—with ordinary shellac the precipitate is a bright yellow—which becomes visible directly the precipitate is seen *en masse* at the bottom of the liquid.

A. Cowper Ranyard.

A PAGE FROM THE MOON'S PAST HISTORY.

[1184]—There is reason to believe that in the condition of the moon we have clear indications of what our earth may one day become. Not indeed in appearance will the earth bear any resemblance to the moon; but in so far as the radiation of its heat into space, and the withdrawal of its oceans—and probably its atmosphere—into its interior, is concerned, it seems probable—nay, almost certain—that in the distant future the earth will bear the closest similarity to the present physical condition of its decrepit satellite.

That water formerly existed in large quantities upon the moon seems manifest when the evidence is fairly and impartially weighed. To make this sufficiently obvious, let us as briefly as possible endeavour to gather a little of her past history from the rugged pages of her surface markings. Apparently there is a significance not hitherto noted, so far as I know, in the arrangement and appearance of these evidences of early activity, when considered with reference to the question of lunar oceans.

The earliest record of volcanic energy lies unquestionably in those peculiar ray systems emanating principally from the "metropolitan crater" Tycho. This is placed beyond possibility of dispute by the way in which large craters, standing up on the line of direction of any one of these rays, are apparently tunnelled (I use this word advisedly, although quite aware of its insufficiency) by them. The region around Tycho would appear, from the numbers of mighty craters in its vicinity, to have occupied in earlier ages the weakest portion of the lunar crust, from whence resulted its inability to resist explosion, and its subsequent upheaval above the mean level of the moon's surface. With the progress of time, however, other craters were formed—some solitary, others in systems. Here and there, by these last (apparently when the energy of the older formation was exhausted), a second crater would break out so close to the first that that part of the wall of the first contiguous to the later formation would be broken and overthrown; but almost invariably the second formation is of smaller dimensions than the one superseded, showing at once the decay of the internal forces and the solidification of the moon's crust. These disturbances must have covered an enormous interval of time. It is reasonable to suppose that some of the smaller and later craters may have formed during the later period of the moon's existence as an inhabited world; or granting that such was not the case, but that they belonged to an earlier period, yet millions of years must have intervened between the formation of Tycho and Copernicus and the formation of the smaller craters.

A noteworthy feature of many of the lunar rings is the manner in which their walls are broken. This imperfection is usually upon their northern side, of which Fracastorius to the south of the Sea of Nectar is an example. It would seem that this peculiarity belonged to a period when the moon's crust was in a fit condition to be the abode of life—solid and sufficiently cool. For we have seen that the southern region was the region of greatest disturbance, whence it follows that the gases imprisoned in the moon's interior would, refused an exit elsewhere, be forced by the pressure of the contracting crust to this outlet. Manifestly these struggling gases in the course of their progress to the point of escape would alternately elevate and depress the various regions under which they passed. It will be easily seen that the vibratory wave-like motion thus resulting would raise the ground gently (comparatively speaking), and cause it to recover its position of rest, so to speak, with a jerk. This jerk would tend to overthrow that portion of a crater first elevated. Thus, if we suppose the underground forces of some northern zone to be seeking an outlet further south, it is obvious that the southernmost portion of the crater walls, being supported, as it were, by the sides, would be prevented from falling, whereas the northernmost, lacking such support, must collapse, granting the disturbing forces to be sufficiently strong, and the crater walls sufficiently weak for this purpose. This may be illustrated by experiment.

Assuming, then, the lunar disturbances to have taken place in the order detailed above, let us see how the question of water is affected by them. There is no reason to reject Mr. Proctor's opinion that the great plains upon the moon are either old sea-bottoms, or that the substances which last remained fluid, while the moon's surface was consolidating, were of darker materials than the rest. A very short study of the moon's surface will show that these greenish-grey plains seem to avoid the mountainous portion of the moon and to confine themselves to the lower levels. It will be noticed also that mountains, craters, or ridges of sufficient altitude in a number of cases determine their boundaries. This is precisely the required arrangement, upon the assumption that they are nothing more or less than old sea floors. Of course, it is possible that this greenish-grey tint marks the regions formerly occupied by molten matter. In this case, however, we must acknowledge that the lunar disturbing forces must have been exhausted long before the moon

occupied a stage of planetary life in anywise similar—so far as heat is considered—to the present condition of this earth. For example; a few craters stand upon these plains. If these craters belonged to the later periods when the moon's crust was solid it is not conceivable that they should be covered with this green tint—granting that the plains *were* formed by molten matter. On the contrary, the most likely supposition is that they would be surrounded by the whiter substance similar to that surrounding some of the older craters. Consider the case of Fracastorius, the incomplete ring already mentioned. If we are to suppose the formation and subsequent levelling of one portion of its wall to have occurred (as seems in the highest degree probable) in the order indicated, this molten matter must have remained in a liquid condition for years after the time when, in the ordinary course of nature, it should have solidified. Otherwise (which seems incredible) the breaking up of this fine structure must have occurred very shortly after the formation of the giant ray systems. But conceding that the grey plains are, in reality, dried-up sea-beds, it is manifest that, upon the overthrow of the wall, the water formerly laving the outer wall would find its way through the *débris* into the crater plain itself.

Such, then, are a few considerations anent the question of the former existence of lunar oceans. It seems probable that if careful measurements of the altitude of craters on and about the great plains were taken, much light might be thrown not only on the question I have briefly discussed, but it might be possible to determine approximately the depth of whatever liquid substance formerly lay there. J. R. Sutton.

OPTICAL PHENOMENON.

[1185]—Any one holding a red-hot poker and whirling it quickly round in a *dark* room will, after two or three turns, see a blue flame or light following in the circular path of the red-hot iron. I dare say this is a well-known and commonly-observed appearance, but I have never seen an explanation of it. S. E. De Morgan.

[Is not the bluish ring the complementary image of the red one —in other words, a subjective phenomenon?—R. P.]

FRUIT-TREES IN IRELAND.

[1186]—At page 197 of your paper of March 28, in Mr. Mattieu Williams's article on Cookery, I find the writer stating that when travelling in Ireland he was "surprised at the absence of fruit-trees in the small farms where one might expect them to abound." On speaking of this, he goes on to say, "the reason given was that all trees are the landlord's property; that if a tenant should plant them they would suggest luxury and prosperity, and therefore a rise of rent;" and Mr. Williams proceeds to express a hope that the passing of the Land Act will have put an end to such *legalised brigandage*.

I venture to think that Mr. Williams, if he must needs introduce political matter into a scientific discussion, ought to have made some inquiry as to the truth of the alleged facts. If the tenants to whom he spoke were leaseholders, the trees would have been their property, by the Irish Statute 23-4 Geo. III., cap. 39 (see Dr. Richey's "Irish Land Laws," 117), provided the tenants took the trouble of registering their plantations. If a tenant were not a leaseholder, then, under Mr. Gladstone's Act of 1870, he could claim compensation for improvements if his landlord "disturbed" him by raising his rent. Even under the law before 1870 the tenant was no worse off than in England, where trees are still "the property of the landlord," and we do not hear of any reluctance to plant. I do not know at what date Mr. Mattieu Williams "explored Ireland rather exhaustively," but I am surprised that a man of science should have overlooked a natural and physical difficulty in the culture of fruit-trees in this island. The climate, as every one knows, is always damp and often cold. The spring season, especially, is most variable, and at all seasons of the year steady sunshine is very rare. Surely this is sufficient to account for the scarcity of fruit-trees, without having recourse to any theory of "legalised brigandage." There are few subjects about which there have been more impudent mis-statements than this of the Irish law of Land Tenure. I do not ask you, or Mr. Williams, to study law, but I think I may fairly ask men of science, above all other men, not to commit themselves to statements of which they have not examined the evidence. I do not expect you, or Mr. Williams, to know that it is the habit of a certain political party in Ireland to attribute all the poverty and other misfortunes of the country to "Saxon misrule" and "landlord tyranny"; but I think I am entitled to expect that when men of science have a *vera causa* of observed facts, in the shape of an ungenial climate, they will refrain from denouncing a class which has too many enemies already. In conclusion, sir, perhaps I ought to state that I am not interested in any way in Irish landed property, though I am interested in justice and fair play. I am neither landlord, land agent, nor agriculturist, and I am a tenant only as regards the house I dwell in. I append my name, address, &c., but not for publication. A Resident Irishman.

SCIENTIFIC MORALITY.

[1187]—It is with great pleasure that we read the articles on the "Morality of Happiness." They are more valuable than whole courses of ordinary University lectures on ethics and political economy; for persons of quite average ability may attend regular courses of such lectures, and yet neither learn the fallaciousness of popular altruistic theories, nor get hold of the true scientific principles of conduct now so admirably set forth by Herbert Spencer.

Ethical theories involve the most important issues; they may determine the whole course of a person's life. It is curious and interesting to look at some of the means adopted in order to carry out to their logical conclusion erroneous altruistic principles, popularly regarded as more sacred even than theological dogmas.

For those who have independent means, erroneous theories as to duty are not so very serious. Such persons will mostly confine themselves to agitation; they may, however, make some such experiment as that of Brook Farm, or may even, like Thoreau, in Walden Woods, retire somewhat from "unjust" civilisation, and go to prison rather than pay certain taxes. On the other hand, for persons who must labour for their subsistence, the task of reducing erroneous ethical theories to practice is a very serious matter. They will review the various occupations, putting most of them aside as "dishonest"—not for the benefit of every member of the community, but only for the advantage of the rich. Agriculture will be regarded as one of the most "honest" occupations, supplying as it does, most of the necessaries required by every one. Some of the able-bodied who wish to put in practice their theories, may for a time devote themselves to this occupation, probably on the free-grant land in the colonies, or they may squat on unsurveyed land, where they will not be troubled by taxes for purposes which they disapprove. They may, moreover, unite in communities, and form such settlements as those at Oneida, New Harmony, and elsewhere.

This is the peaceful side of the picture; though it may entail considerable suffering on the individuals themselves, yet it does little harm to society at large. But there is a dark side. Those who regard altruistic principles as rules for practical conduct, and who are without means, probably for the most part remain in large cities, living contrary to their principles by whatever occupation fortune may throw in their way. In their uncongenial pursuits they brood deeply over their supposed wrongs and the supposed tyranny of the rich. It is among such people that revolutionary ideas are readily propagated and conspiracies hatched. Socialists, Communists, and Nihilists are all most strongly imbued with altruistic principles, and yet many of these same persons are ready to overthrow civilization, regardless of the cost of life and suffering which it would entail. Hardly a week passes but we hear of some diabolical attempt to destroy life or property, all in the name of altruism.

If the popular altruistic theories are correct, Socialism and Communism are their logical results, and it is only by attacking the principles from which Socialism and Communism are deduced that these errors can be successfully combated. Attempts made by persons of altruistic opinions to combat Socialists are almost worse than useless, since they involve fallacious reasoning. The "Prophet of San Francisco" may indeed laugh to himself if he encounters no more redoubtable antagonist than some of those whom he has recently met.

Knowledge cannot be engaged in more important work than in diffusing scientific ethical theories. Scientific morality is the true antidote to revolutionary ideas. T. Common.

MAN IN THE GLACIAL PERIOD.

[1188]—Mr. R. B. Cook, in his article in Knowledge, of March 28, p. 199, says:—"It is now an acknowledged fact that man existed during the Glacial Period (the discovery of his tools and implements in Glacial formations having settled that question)."

Though several lines of argument point to this conclusion, I was under the impression that direct evidence was still wanting. Certainly such was Mr. Boyd Dawkins' opinion not very long ago. If Mr. Cook will kindly state the locality where these tools have been found in Glacial deposits, and give his authority, he will greatly oblige the writer of these lines. C. Kirkdale.

AN UNSOLVED TRICYCLE PROBLEM.

[1189]—Can any of your tricycling readers tell me why a rubber tyre leaves a double rut in dust, and a single one in mud? The fact is indisputable. The impression of the wheel has a small sharp ridge along its centre dividing it into two parallel ruts. I can hardly find any riders who have noticed it! Plenty have offered amusing suggestions, all hitherto inadequate. In extremely thin dust, the ridge is less perfect. In dust 2 in. deep, it is overwhelmed by the falling in of the sides. D. M.

ERRATA.

[1190]—Kindly allow me to correct a mistake of your compositor in letter 1174, p. 232, April 4.

"All-seeing" should have been "all seeing."

"Orthoscopic and pseudoscopic" should have been orthopic and pseudopic," as applicable to the phenomena indicated.

Orthoscopic and pseudoscopic are applied to certain phenomena produced in the stereoscope; the latter, when by interchanging the two stereoscopic pictures the effect is reversed,—the foreground seems to retire, and the background to advance, and the solid cube to become a hollow box, &c.—Yours, respectfully,

William Wilson, M.A., LL.D.

[We willingly insert Dr. Wilson's corrections. The first mistake was obvious. But we should like to know precisely what "orthopic" and "pseudopic" may mean. We changed the words ourself, though we know there is another technical meaning for the words we substituted. Our reason was that the words "orthopic" and "pseudopic" seemed to us impossible.—Ed.]

LETTERS RECEIVED.

J. Roberts. Anster's translation of "Faust," recently republished, we believe, by Macmillan, is a good one. But we should not like to say which is the best. *Fors Clāvigĕra* may mean either Club-bearing Fortune or Key-bearing Fortune,—*utrum horum mavis accipe:* the quantities are as indicated.—J. Rogers. The tide is not always at the same height midway (in time) between low and high tides; but there is no recognised difference between mid-height at neap and spring tides. On the average the mid-height would be the same at neap and spring.—Louisa Kennedy. I certainly regard the thirty-five guesses as happy hits only, and the pretended star-reader as a charlatan and a rogue, unless (which is unlikely) he chanced to be an idiot.—Zadig. The misprint at p. 191, 2nd col. 4th line from bottom, consists in the substitution of the word "area" for "side": of course if the diagonal of a square is 1, the area of the square, being half that on the diagonal by Euc. I., 47, must be ·5.—G. Lounak. If the last term of an arithmetical progression and the sum are all you know, you cannot possibly determine the arithmetical mean and the number of terms, seeing that there are any number of progressions having a given last term and sum.—Dædalus. Fear we could not give a list of works bearing on Aërostation.—Anxious. No occasion for anxiety; the Great Bear and the earth are all right, both of them. At 8 p.m. six months ago the earth was differently posed by half a rotation from her pose at the same hour now. Naturally then the Great Bear or any other celestial group appears half a rotation away, now, from the position she had, at the same hour, six months ago.—G. Williamson. Do not know where or how the large map of the moon by Dr. Schmidt of Athens is published. The British Association catalogue is I believe out of print.—W. J. P. I suppose because in St. Stephen's parish.—W. W. Sardtell. Distilled water would be more apt than any to give you a taste and more than a taste of the lead. I am no authority on the subject, but I should imagine that all such advice as was given in that article, like the elixirs of life which some folk promise, should be taken *cum grano salis*, not with lead, and with scruples of another sort.—T. M. I should say your New Geological Theory was very new indeed,—more new than true. I do not think it will be "criticised by scientific men."—R. Gerner. Many thanks, but we could not spare so much space for the manufacture of india-rubber. Our space does not possess india-rubbery elasticity.—Anderson. Cannot you sympathise with the effect of that great mystery, the existence of multitudinous wrong-doing, in such a mind as his? Confusion no softening, and what thinking man is there that has not been perplexed and confounded by that mystery?—Alice E. Wellson. Fear I said nothing about the decay which follows death, because I know nothing of that stage.—H. Garside, Jun. It is quoted in my "Universe of Stars," and in Mitchel's "Planetary Worlds."—W. H. Lloyd. The more I have tested Professor Loisette's system the more value it.—H. C. Wilders. Fear can make no exception to the rule that we do not work sums. We really cannot afford the time.—G. St. Clair. Have unfortunately no available space for that subject.—T. H. I think "A. McD." meant—and would be understood to mean—by conscience only the internal recognition of a certain view in regard to doctrine. In all ages there have been but here and there two or three who have analysed conduct at all. The people in those ages *did* receive, through those who did the reasoning for them, the assurance that conduct conduces to happiness.—Early Reader. Secondary batteries have been applied to tricycles; but, so far as we are aware, only experimentally. Electro-motors for such purposes are but luxuries.—E. S. In the Brush machine those coils which are on the diameter of least inductive effect are automatically disconnected. The other coils are in one common circuit with the field magnets. The wire on the Griscom is about No. 20.

A very distinguished audience assembled at the Parkes Museum on Thursday evening, March 27th, to witness Mr. Watson Cheyne's demonstration of pathogenic micro-organisms. The chair was taken by Sir Joseph Lister, Bart. After stating that the great group commonly called Bacteria might most conveniently be subdivided into four classes —1, Micrococci (round bodies); 2, Bacteria (small oval or rod-shaped bodies); 3, Bacilli (large rod-shaped bodies); and 4, Spirochœtie and spirilla (rods spirally twisted)—and dwelling on the great variety as well as importance of the various parts played by this great group in the economy of nature, Mr. Watson Cheyne demonstrated numerous micro-photographs taken by Dr. Robert Koch, as well as some drawings by means of a lime-light apparatus. He observed that great differences existed among the various bacteria in their behaviour towards the human body; some could be injected without causing any injury; others could not grow in the living body, but could develop in dead portions of tissue and the secretions of wounds, giving rise to poisonous products. The true pathogenic organisms were able to attack the living body and multiply in it; they included the organisms which found entrance through some wound, giving rise to the traumatic infective diseases, and others which could obtain entrance without observable wound. Further, certain organisms, such as the *B. anthracis*, were capable of growing outside the body in dead organic substance; while others, such as *B. tuberculosis*, were apparently only capable of development in the living organism or under artificial conditions which reproduced to some degree those existing in the tissues of warm-blooded animals, though capable of long retaining their vitality in the dry state. With regard to the traumatic infective diseases, he thought that the most absolute proof had been furnished that the bacteria found in them, and nothing else, were the causes of the diseases; to establish such a proposition it was necessary that an organism of a definite form and with definite characteristics should always be found in the blood or in the affected part; the blood or the affected part, when inoculated into another animal of the same species, must produce the same disease; when the blood or the affected part was inoculated on a suitable soil outside the body the micro-organisms grew and must be indefinitely propagated on similar soil. When in this manner the organisms had been separated from the remains of the materials in which they were embedded, their inoculation in an animal must produce again the same disease, the same organisms being found in the diseased parts. These conditions had now been fulfilled with regard to anthrax, septicœmia of the mouse, erysipelas, tuberculosis, glanders, and acute pneumonia. With regard to typhoid fever, relapsing fever, cholera, and ague, the evidence was very strong, but not conclusive. Mr. Watson Cheyne concluded by dwelling on the importance of surrounding circumstances, chiefly those summed up in the phrase un-hygienic conditions, as concomitant causes of disease by preparing the blood for the attacks of these micro-organisms. The chairman, Sir Joseph Lister, dwelt upon the important fact that the organisms which produced particular diseases were only able to develop under very special conditions, instancing the bacillus, which caused septicœmia in the house-mouse, but which was unable to produce any deleterious effect on the field-mouse. He thought this fact, which showed that the very slight difference in the blood of these two animals was sufficient to alter the conditions favourable to the development of the bacteria, might prove of very great interest, as it was possible to conceive that by the administration of some medicines, sufficient alteration might be produced in the blood of the human system to kill off or to prevent the development of any special bacteria on the first appearance of the symptoms of the disease in the patient. Sir Joseph Lister concluded by referring at some length to the importance of Pasteur's researches on modified virus. Prof. Humphry paid an eloquent tribute to the great work which Sir Joseph Lister had already achieved, and looked forward with a large hope to the future of medicine.

Our Paradox Column.

We have received from Dr. Birley's solicitor a statement to the effect that proceedings have been instituted against Mr. H. Ossipoff Wolfson in regard to remarks respecting Dr. Birley in our Paradox Column, and a warning that (until such proceedings are decided, we presume) we should insert no further remarks of Mr. Wolfson's about the earth-flattening Parallax. We willingly accede to the suggestion, and we should even more willingly insert any counter-statements which Dr. Birley may desire to make. His silence as to details leaves us in doubt as to the particular remarks to which he objects or the particular statements which he denies. We find on reading over Mr. Wolfson's remarks that he describes the philosopher who uses the *nom-de-plume* (and *de guerre*) of Parallax, as being no other than the Mr. S. Goulden of 1849, the Mr. Rowbotham of 1864, who at another time went by the name of Tryon, and is now known—we had supposed—as Dr. S. Birley; he speaks of Parallax as one who claims to know how life is shortened and how it may be prolonged; he expresses the opinion that Parallax is not in reality master of these secrets; and he uses words implying that Dr. S. Birley is not a registered medical practitioner. We infer from the communication which has reached us that Dr. Birley is not to be identified with Mr. Goulden, or with Mr. Tryon, or with Mr. Rowbotham, or with the writer who under the name of Parallax has described certain rather singular and very doubtful experiments, who has advanced a theory of the earth's flatness, which to say the least is untenable, and who has issued a work claiming to show how men may attain patriarchal longevity. [Some of these circumstances in regard to Parallax have come prominently under our own notice, through the issue, by Parallax or his agents, of a circular (widely distributed at Memorial Hall on the occasion of Mr. Proctor's lectures) in which the Zetetic Astronomy and the treatise promising patriarchal extension of life were advertised, in company with a rather absurd account of a challenge to Mr. Proctor.] If Dr. S. Birley is not the same person as Mr. S. Goulden, Mr. Tryon, and Mr. Rowbotham, we shall hear with regret that we have so far wronged him. We supposed we had seen in a circular of the Balloon Society the name Dr. Birley definitely identified with that of Parallax as a lecturer for that society. But we must have been mistaken,—or there may be two Dr. Birleys, since there seem to be at least as many Parallaxes. Be this as it may, our columns are open to any statement Dr. Birley may wish to make here. Should we find we have done him injustice we will make the fullest apology. Or should he prefer to seek legal remedy we shall be well pleased, if we have done him injustice, to make such reparation as may be held right. But of whomsoever it may be who wrote and published the Zetetic Astronomy and the promise of extending life to patriarchal length, we must continue to assert our opinion in the clearest terms as occasion may arise. We regard this as a public duty, and it is a duty from which we assuredly shall not flinch.

Lastly, let me here for the moment abandon the editorial "we" and speak for myself, noting that I and I alone am responsible for whatever is said in these columns. If wrong has been done to Dr. S. Birley in identifying him with Parallax, apology and reparation will be made by *me* (though legal formality may appear to make the publishers responsible); if Parallax should even turn out to be no other than Mr. Goulden-Tryon-Rowbotham Birley, and if his public advertising of a treatise promising hundreds of years of life should be held to justify the unfavourable comments passed upon it, I shall still be most ready to insert any corrections he may have to make on Mr. Wolfson's statements, and to express my sincere regret for anything erroneous or misleading in them.

Richard A. Proctor.

Errata.—In both Mr. Wolfson's papers some words occur which need correction—though for a foreigner he writes excellent English. Among these are the words "recompensate" and "recompensation" for "compensate" and "compensation." It is hardly necessary perhaps to say that the word "can" in the tenth line from the bottom, p. 233, should have been "may," Mr. Wolfson's meaning being manifestly that the secret for prolonging life which Parallax claims to possess is not altogether to be trusted, and may tend rather to shorten life, as the best-intended prescriptions have been known to do. The warning is a very necessary one in all such cases, elixirs of life having sometimes proved very mischievous in their effects.—R. P.

Errata.—Page 230, col. 2, "Sydon" should be "Sydow." Same column and page, line eight from the bottom, "a map of cognate information" should be "a mass of cognate information."

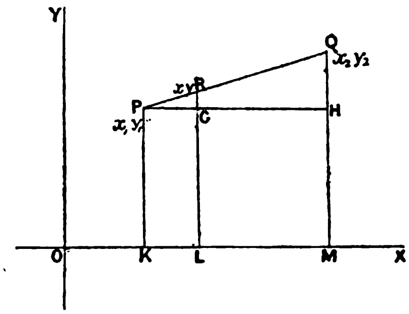
Our Mathematical Column.

EASY LESSONS IN CO-ORDINATE GEOMETRY.

By Richard A. Proctor.

(*Continued from page 214.*)

12. *To find the co-ordinates of the point dividing the straight line joining two given points in a given ratio, the axes being rectangular or oblique.*



Let P, Q be the given points; x_1, y_1 the co-ordinates of P, and x_2, y_2 those of Q. Suppose $m : n$ the ratio in which the required point R divides P Q, so that P R : R Q :: $m : n$. Let x, y be the co-ordinates of R. Draw P K, R L and Q M parallel to O Y to meet O X and P G H parallel to O X, to meet R L and Q M in G, and H respectively; then

$$m : n :: \text{PR} : \text{RQ} :: \text{PG} : \text{GH} :: \text{KL} : \text{LM},$$

that is $\quad m : n :: x - x_1 : x_2 - x.$

Hence $\quad m(x_2 - x) = n(x - x_1),$

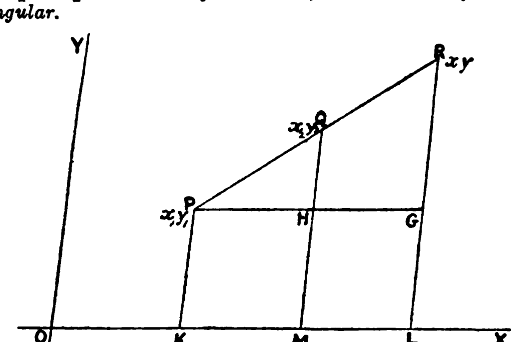
and therefore $\quad x = \dfrac{mx_2 + nx_1}{m+n};$

and similarly $\quad y = \dfrac{my_2 + ny_1}{m+n}.$

If P is at the origin, so that $x_1 = 0$, and $y_1 = 0$; then

$$x = \frac{mx_2}{m+n}, \text{ and } y = \frac{my_2}{m+n}.$$

13. *To find the co-ordinates, dividing the straight line joining two given points produced, so that the whole line thus produced bears to the part produced a given ratio, the axes being oblique or rectangular.*



Let P, Q be the given points; x_1 y_1 the co-ordinates of P, and x_2 y_2, those of Q. Suppose the straight line joining P Q produced to R, so that P R : R Q :: $m : n$, the given ratio. Let x_1 y_1 be the co-ordinates of R. Then with the same construction as in 12, we have

$$m : n :: \text{PR} : \text{RQ} :: \text{PG} : \text{GH} :: \text{KL} : \text{LM}$$

that is $\quad m : n :: x - x_1 : x - x_2$

hence $\quad m\,(x - x_2) = n\,(x - x_1)$

and therefore $\quad x = \dfrac{m\,x_2 - n\,x_1}{m-n}$

and similarly $\quad y = \dfrac{m\,y_2 - n\,y_1}{m-n}$

Cor.—If P is at the origin, so that $x_1 = 0$, and $y_1 = 0$; then

$$x = \frac{m\,x_2}{m-n}, \text{ and } y = \frac{m\,y_2}{m-n}$$

We see that these results differ from those of 12, only in the sign of n.

P Q in 12, is said to be divided *internally* in the ratio of $m : n$;

in the present 13, P Q is said to be divided *externally* in the ratio of $m : n$.

(To be continued.)

EASY RIDERS ON EUCLID'S FIRST BOOK.

With Suggestions for Solution.

Prop. 12.

32. Two straight lines are drawn from a given point. From another given point it is required to draw a straight line which shall cut off equal parts from the given straight lines.

33. From two given points on opposite sides of a given straight line draw two straight line which shall meet in that given straight line and include an angle bisected by that given straight line.

34. From two given points on the same side of a given straight line draw two lines which shall meet in that line and make equal angles with it.

35. A B, A C are two given straight lines, and D is a given point; it is required to find a point E in A B and a point F in A C such that the lines D E, F E shall make equal angles with A B, and the lines D F, E F with A C.

Notice that D *and* F *bear the same relation to* A B *that is investigated in* 34; *and in like manner* D *and* E *bear the same relation to* A C. *These considerations will suggest a construction requiring the use of Prop.* 12.

36. A B, A C are two given straight lines, and D is a given point so placed that the perpendicular from D on A C cuts A B. It is required to find a point E in A B and a point F in A C, such that D F and D E make equal angles with A C, and A E bisects the angle D E F.

Apply 33 *and* 34.

Prop. 13.

37. In the second figure to Prop. 13, if lines be drawn bisecting the angles A B C, A B D, these lines shall be at right angles to each other.

Prop. 14.

38. If there be three lines A B, A C, and A D meeting in a point, and if the lines bisecting the angles B A C, C A D are at right angles to each other, then shall A B and A D lie in the same straight line.

Our Chess Column.

EASY NOTES ON THE OPENINGS.

By Richard A. Proctor.

In the Chess studies now commenced I propose to consider the game entirely from the point of view of the home Chess-player, who desires to play a sound and good game but not to devote more time than is reasonable to what (with him at least) is after all but a game, nor to take part in contests with recognised Chess champions.

The fault of most home players of Chess is that they open the game in a haphazard fashion and without any reference to certain principles which should guide us in the conduct of the opening, and will nearly always if followed lead to a sound game,—though, of course, there are cases known to all advanced Chess players, where certain lines of play which seem on the face of them sound enough lead directly to disaster.

The principle to be first noticed in opening the game is that the pieces should be brought as quickly as possible into effective action,—the Knights and Bishops first, and the Rooks generally next, the Queen, though she may sometimes be used with great efficiency even at an early stage, being usually kept in reserve and out of the way of mischief while the other forces are being developed. A sound player regards this question of development as influencing every move he makes. He is never troubled with the thought so often expressed by inexperienced players,—what can I do now? as though at any stage of the opening game there could ever be a position where there was not plenty of work in the way of developing the forces. Yet again, the question of development influences the sound player in estimating the value of each piece. He will not readily exchange a well-placed Knight or Bishop of his own against a Knight or Bishop of the enemy which has as yet no value derived from favourable situation. If the exchange is forced on him, he will perhaps prefer to make a developing move himself so that to the enemy is left the task of effecting the exchange, a new development of force perhaps following the return capture.

Now this question of the development of the forces at his command seems to have no existence for many players. They will think nothing for example of playing King's Bishop to Queen's third square to defend King's Pawn, when—the Queen's Pawn not having been as yet moved—they block by this one move Queen's Bishop, Queen, King, and Queen's Pawn,—partially blocking also the Queen's Knight which often comes effectually into play by Queen's second square.

Another principle of great importance is to avoid hasty attacks, or attacks which may be easily met by deploying moves on the part of the enemy. Bringing out the Queen over hastily is one of the ways in which inexperienced players run counter to this sound rule. They get her ladyship into positions of danger from which it may be difficult to extricate her. Even where she is not thus lost, or perhaps even seriously endangered, she may yet be repeatedly attacked by hostile pieces which at each attack occupy more favourable positions.*

Checking for checking's sake is another fault of the inexperienced,—purposeless checks being not only wasted but often leading to a most effective development of the hostile forces. Another vanity of the inexperienced is the bringing forth of Queen's Knight to attempt the forking check on adverse King and Queen's Rook—a plan easily defeated if necessary, but often most effectively allowed to culminate in success, by which a piece worth more at the beginning than the captured Rook is taken out of play, several moves wasted, and probably the fate of the all-important King sealed. I have no doubt every fairly good Chess player has repeatedly allowed this masterpiece of infantile Chess strategy to succeed when he has already given the odds of a Queen,—having a won game either when his Queen's Rook has been captured or but a few moves thereafter.

Passing over these forms of weak or unwise play, let us now proceed to consider a game commenced on the good old-fashioned line,— $\frac{\text{1. P to K4}}{\text{1. P to K4}}$ 2. $\frac{\text{Kt to K B3}}{}$. This is probably the best line for young players to open upon, and the best also for those who wish to train young players to start with. It introduces us most quickly to the practical consideration of sound rules of Chess play.

How shall the attacked King's Pawn be defended? Of course a counter attack may be made by attacking the first player's King's Pawn, either with King's Knight, constituting the powerful Petroff counter attack, with King's Bishop's Pawn constituting the Greco countergambit, or with Queen's Pawn. These replies will have to be considered later. But they are less suitable for the learner than the direct defence of the Pawn which we must now consider.

The King's Pawn may be defended by 2. P to KB3, a move which looks safe enough, but in reality scarcely defends the Pawn at all. For the Pawn may be immediately captured with the Knight, and if Knight be taken, a first player checks with the Queen, and Black's game is hopelessly compromised. If Knight is not taken, although Black can recover the Pawn by playing Q to K2, White will have far the better position. In fact it is a good general rule that the move P to KB3, on either side, is a bad one.†

The King's Pawn may also be defended by 2. KB to Q3. Of this move I have already spoken. It is contrary to all sound Chess principles, and leads to a disastrously cramped position on Black's part. White replies by 3. $\frac{\text{B to QB4}}{\text{Kt to KB3}}$, 4. $\frac{\text{P to Q4}}{\text{Kt to QB3}}$, 5. P takes P, and however Black may play White can maintain a superiority owing to the superior freedom of his men.

Thirdly, the pawn may be defended by 3. Q to KB3, a bad move, because the Queen is exposed to attack, and White's forces may be effectively developed while she is being driven hither and thither for safety.

But, fourthly, Black may defend his King's Pawn by 3. P to Q3, a defence rendered famous by the advocacy of Philidor. It is in my opinion the safest defence for the young player, especially in playing with odds against a stronger player. For though it retards the development of Black's men in some slight degree, yet it gives him a sound defensive position. If we consider carefully the various lines of play on which it has been shown that Black loses from the employment of this defence, we shall find that in every case his

* A favourite attack of the tyro is to play B to B4 and Q to B3 or R5, threatening the KBP which, however, is mere waste of time, as the P is easily defended. In the Scotch Gambit Q to R5 is sometimes played on the 4th move.—Mephisto.

† Except *when the King's Bishop is exchanged early in the opening*, as occurs for instance in the English Opening, where Steinitz introduced P to KB3 at an early stage.—Mephisto.

trouble arises from his attempting a bold attacking line of play after adopting at the outset a defensive policy. If he is thus in two minds as to his play, he naturally comes to grief; but if having instituted a defensive policy, he adheres to it, he can successfully maintain an equal defence, and when the opening attack against him is exhausted he will be found to have as good a middle game as his opponent.

White may now continue an attacking course by 3. P to Q4, or proceed to the steady development of his game by 3. B to QB4.

We will take the former course first. The game then starts

1. P to K4 / P to K4 2. Kt to KB3 / P to Q3 3. P to Q4 / P takes P 4. Q takes P.

Black may decline to take the proffered Queen's Pawn at move 3, playing either 3. Kt to Q2 which is sound, or 3. Kt to KB3 which is weaker, or 3. B to Kt5 which is much weaker.

We shall consider these lines later, as also 3. P to KB4, Philidor's old line of play (by which he considered that Black got the better game); at present we follow what is probably Black's best play 3. P takes P, to which White may reply either by 4. Q takes P or 4. Kt takes P.

From the former line come the following line, which the student may severally follow as far as he pleases, making a good evening's study, the results of which he may with advantage commit to memory by playing several games out on these lines.

Black.

I.

1. P to K4 / P to K4

2. Kt to KB3 / P to Q3

3. P to Q4 / P takes P

4. Q takes P

The piece last moved is set sideways.

White.

Position after White's fourth move (4. Q takes P.

4.	Kt to QB3	B to Q2	B to Q2	B to Q2
	B to QKt5	B to QB4	B to KB4	B to KKt5
5.	B to Q2	Kt to QB3	Kt to QB3	Kt to QB3
	B takes Kt(a)	Q to K3	Q to Q2	B takes Q
6.	B takes B	B to K2	Kt to KB3	Kt takes Q
	B to KKt5	Q to QKt3	B to Q3	Kt takes Kt
7.	Kt to KB3	White has the better game	B to K2	K takes B
	Kt to QB3		Kt to QB3	B to QB4
8.	B to K2		Castles	Kt to KR3
	Castles Q side		Castles Q side	Kt to QB3
9.	Castles		White has the better game	B to K2
	Equal game			Equal game

(*a*) Staunton recommends here retreat of Queen to her square.

The second and third columns show that 4. B to Q2 is not so good a move as 4. Kt to QB3. But I am inclined to think that White does not do well to endeavour to avoid the retreat of his Queen; 5. Q to K3 seems to me better than 5. B to QKt5. There may, however, be objections to the move which I have not noticed.

(To be continued in next number but one.)

Correction.—We regret to say that some mistakes have occurred in our last week's copy. There should have been a Black Rook on Black QB sq, when the ending would have read as follows:—1. B × Kt, BP × B. 2. Q × P, Q × Kt. 3. P to K5, P × Q. 4. P × B (ch), R to B2. 5. P × Q, and wins. In the game following, Black's 6th move should have been given as Kt to R4 instead of K4.

Our Whist Column.

A WHIST-PLAYER'S WAIL.

By the Author of "Bumblepuppy."

(*Continued from p. 236.*)

To show that I am not making random accusations, I give three examples—there are others in stock, but these appear sufficient for my immediate purpose:—

I. "The cards are cut. In taking up the packs, I join the two packs, but leave one card on the table; this card would have been the middle, not the bottom card. I claim a fresh cut; my adversaries claim that it is a misdeal. Am I entitled to a new cut or not?" Answer, No. 1. "We think you cannot make your adversary cut a second time. We do not think that when you left a card on the table it could be said that there was any confusion in the cutting, and unless you can make out that what you did amounted to confusion in the cutting, it is a misdeal."

Answer, No. 2. "The claim is void. There is nothing in the laws or the custom of the table to make this a misdeal." Both these decisions are by the same authority. A more recent authority says, "According to the old rules, a misdeal might have been claimed; but not now, under Law 34." The explanation is ingenious, if not ingenuous; but it is open to the objection that, as the first decision is dated December, 1873, nine years after the present laws came into force, it is scarcely water-tight.

II. If A asked B whether he had any of a suit in which B had renounced, and B, instead of replying, turned and quitted the trick, and was subsequently brought to bed of one or more, his silence, combined with turning and quitting the trick, was ruled to be an answer in the negative within the meaning of the Law and he had revoked.

This is a decision of Clay's; and though disputed at the time, was the settled practice of Whist for fourteen or fifteen years.

Three or four years ago this decision was reversed, and authority has now taken its stand upon the literal interpretation of Law 74.

III. Some little time since my opinion was asked on this point. It was sent to me by a friend in Australia. "A and B *v.* Y and Z. Eleven tricks have been played. At the twelfth trick A leads a Heart, Y plays a Club, B plays a spade. Before Z has played, Y throws down his last card, which turns out to be a Heart. Has he revoked?"

Being mortally afraid of putting my foot in it, I much prefer to leave the mysterious borderland between sanity and insanity to experts in lunacy; however, in the sacred cause of friendship, I screwed up my courage, and, with considerable trepidation, gave an opinion to this effect. "It appears to me that Y certainly—this sounds unpleasantly like slang, but such is not my intention—revoked if the Club was a Trump, and, probably, if it was a card of a plain suit, for in playing his last card he either led or abandoned his hand, which has always been held to be an act of play establishing the revoke."

The question was next submitted to three of the best-known and most-respected authorities in this country—all champion deciders—whom we will call P. Q. R. P. replied, "Unless Clubs are Trumps I do not think Y. has revoked. He has not played again. He has exposed a card. If Clubs were Trumps I think he has played again (am not sure). The case is not sufficiently stated for a positive opinion."

Q. and R. did not regard it as insufficiently stated in any way, and they had no hesitation in saying that Y. had not revoked.

When by the next mail it turned out that Hearts were Trumps, when, consequently, the revoke was a shade more doubtful than before, while P. made no further sign, Q. and R. came to the unanimous conclusion that Y. had revoked. The authorities at the Antipodes who ruled originally that there was no revoke, remains in the same mind up to the present time.

Is this "vacillating and inconsistent," or is it not?

Here is a not very complicated difficulty—if only there was any agreement on first principles. We have,—

(*a*) A benighted outsider thinking a revoke is established, because a well-known decision overrides the law;

(*b*) An intelligent Colonist thinking it is not established, because he considers the law to override the decision.

(*c*) Authority No. 1 giving a somewhat uncertain sound, but on the whole inclining to the belief that it is either a revoke or it is not, evidently a man of judicial mind.

(*d*.) Authorities 2 and 3, while never in doubt for a moment, first affirming a thing to be white, and afterwards, when it has been bleached and is to some extent whiter than before, with unabated

confidence affirming it to be black; and there an important question, involving the highest penalty known to the law, rests.

If the force of absurdity can go beyond this, then "it can go anywhere and do anything."

The facts are in a nutshell. Either *Y*, when he threw his card up, abandoned his hand, or he did not. If he did, and *if that is an act of play which establishes a revoke*, then he revoked; if he did not, he had merely to say so, *cadit quæstio;* the card is an exposed one—"just that, and nothing more." Only we have one, or rather two little difficulties to get over. Does abandoning the hand establish a revoke? and, if it does, is the decision authoritative—that is to say, of compulsory obligation?

Who the original decider was, or who gave him authority to make a penal enactment in the teeth of Laws 58 and 73, I do not know. All I do know is that the decision must not be fathered on Clay, for his case 8, "*A* has revoked; *his claim of the game* and throwing down his cards must be held as against himself as an act of playing," is not on all fours; it occupies much firmer ground.

Here are two well-matched decisions, "Silence is an answer." "Throwing down the cards establishes a revoke,"—of course, with the proviso that one has been made—both strain the law; both entail the revoke penalty; the only difference is that one is in the *ipsissima verba* of Clay, the other is a mangled excerpt; if the strong one has been quietly and surreptitiously burked, why, in the name of ordinary patience, does the weaker survive?

If decisions are retreating all along the line to a safer standpoint on the letter of the law, well and good; only tar them all with the same brush, and take some means to let the public know it.

Before the lamented demise of the Westminster Papers, disputed points were argued at length; whether in the number of counsellors there was wisdom, or whether too many cooks spoiled the broth, in either event we heard both sides. Question and answer could be found together, and if the decision did not invariably commend itself to our intelligence, we at any rate knew what the decision was, and that was the main point; but now our position has changed greatly for the worse. The present practice of Whist—a direct incentive to gambling—is this; whenever any doubt arises, instead of being able to lay their hands upon the recorded decision and settle it at once, the parties concerned first make a bet of one or more sovereigns and then write to the *Field*. On the ensuing Saturday afternoon a certain amount of money changes hands; two people are wiser, but the increase of wisdom is confined to themselves, and at the very next table the same process is repeated; while numerous quiet, well meaning people like myself, who never bet, never know anything at all; for such answers as these, "X. It is a revoke," "A. S. S. You cannot call on Z to pass it," partake very much of the nature of Valentines in that, however interesting they may be to the recipient, they arouse no corresponding emotion in the world at large.

(*To be continued.*)

SOLUTION OF DOUBLE DUMMY ENDING, p. 193.

1. *B* plays C. *A* puts S. Ace. Won by *A*.
2. *A* plays D Ace. Won by *A*.
3. *A* plays S 2. Won by *B*.
4. *B* plays S K. *A* discards D. Won by *B*.
5. *B* plays C. *A* discards D.

And *Y* must discard either D or H, and *B* plays accordingly.

Correct solutions received from E. F. Lewin, Q. T. V., W. H. M., A. V. Merriman, J. Hartop, M. M. L., John Martindale, A Whist Student, C. D. V. We invite those whose solution differs from the author's to examine the problem afresh.

E. W. Y.—The play of Queen second hand from Queen and another not in sequence is almost universally rejected by Whist-players. You say *even* the King may be played. The old saying was King ever, Queen never. Then came Mr. Clay's rule, King in trumps, small card in plain suits. Now the generally-accepted rule is, Never play the honour from King or Queen and a small card unless for a special purpose, as to get a lead. It would have paid *Y* no doubt in the game at p. 216 to have played Queen to trick one. But it would have been bad play all the same.

WM. JACOBSON.—We do not undertake to decide questions relating to the laws of Whist and their operation. Our Whist papers, games, &c., are intended rather for home players than for those who are likely to be much interested in "decisions." Still there can be no objection to our answering your questions. Undoubtedly an exposed card is only one which can be called when playable without revoke, not one the playing of which can be forbidden. Opponents may defer calling for the exposed card as long as they please, but they cannot prevent its being played when they do not call for it.

A. J. E.—You should certainly cover Queen led with Ace, second hand, unless you have reason to think your partner probably holds the King,—as when third player has discarded from the suit. In the latter case it is better to hold up the Ace. Suppose, for instance, you hold Ace and two others, second hand, and third player has discarded from the suit,—then the lead is presumably from strength, Queen, Knave, Ten, and another, and the chance is small that third player has helped to unguard King: therefore your partner probably holds King; but if he does not, you are still sure of not being ruffed second round, and if third player discarded from King two little ones, your Ace will catch the King on the second round of the suit.

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CONTENTS OF No. 129.

SOLAR SURROUNDINGS.*

BY RICHARD A. PROCTOR.

THE LATEST SCIENTIFIC DISCOVERIES ABOUT THE SUN'S CORONA—PHOTOGRAPHS OF IT AT LAST—A GREAT SCIENTIFIC TRUTH SEEMINGLY ESTABLISHED —FUTURE OF THE INQUIRY.

DR. WILLIAM HUGGINS, the eminent physicist and astronomer, entertains the confident belief that he has succeeded in photographing the solar corona without the aid of a total solar eclipse. I am myself not quite convinced that what he has photographed is really the corona, though I would fain hope so. And yet the evidence seems strong enough. His method is simple and probably well known not only to men of science in America, but to most of that large population there which, without being scientific, takes interest in scientific matters. I therefore only sketch it, and that lightly.

He takes advantage of the fact that a large proportion of the light of the corona belongs to the violet end of the spectrum, and uses absorptive media which allow this sort of light and this only to pass freely through. Then, when the photographing telescope is turned toward the sun and the coronal region, the violet light of the corona, which is relatively strong, only has to contend against the violet light from the sky around the sun's place, and has at least a better chance of making its presence known—in other words, a better chance of recording a recognisable picture of the coronal streamers on the photographic plate, on which necessarily the light from the sky is combined with the light from the corona. It seems clear that, if the image is taken first near the centre of the telescopic field and then near a side, any optical effects due to the structure of the telescope itself must be detected and eliminated. Streamers simulating a coronal appearance could not possibly be alike in both positions. So any other purely instrumental peculiarities can, it would seem, be corrected. As for any coronal streamers caused by our own atmosphere, they must be corrected if we take pictures on different days or at different hours. If, under such varying conditions, we find that still certain streamers remain which can be recognised as the same in all the different pictures, it certainly seems as though there must be true coronal streamers. This is what Dr. Huggins claims to have done, and it is what his pictures really seem to show that he has done. So that I scarcely know how to justify the doubts which yet I cannot help entertaining. These streamers are so faint and shadowy (though that of course they could not but be), it is so easy "to make believe a good deal," as Dick Swiveller puts it, in looking at appearances so delicate, especially when (as in my own case) we wish very much to believe that a great scientific triumph has been achieved, that an excess of caution comes over me, and despite the agreement of men so competent to judge as Dr. Huggins, Professor Stokes, and Captain Abney, my mind in this matter "asks for more."

Perhaps one would not be so ready to entertain still a little doubt were it not that the matter is one which can so very readily be tested. Dr. Huggins's method is one which can be applied under especially favourable conditions in the clear skies of America. There are also in America magnificent instruments for testing the method. I should be glad to learn that the mantle of my late most esteemed friend Dr. Henry Draper had fallen on a successor as zealous in the cause of science as he was; nay, even that the instruments he employed so successfully had been directed again to the class of work for which he made them, but with a slight change of subject. Solar photography is making great progress in England; but we have not the favourable conditions which exist in America. It has even been said by a French author, who under the *nom de plume* of Max O'Rell (impossible name!) has recently discussed John Bull and John's Island, that we photograph the sun in England whenever we get the chance lest we should forget him. Without being quite so bad as that, our atmosphere is certainly not the best suited in the world for the very delicate and difficult problem attacked by Dr. Huggins. (Professor Daniel Draper has indeed shown that out of 4,449 possible hours of sunshine, New York had 2,936 actual sunshine hours in 1878, and 3,101 in 1879, say in round numbers 3,000 hours; whereas at Greenwich, with only two hours less of possible sunshine, there were but 1,245 hours of actual sunshine in 1878 and 977 in 1879, an average of 1,111 hours only.)

It may perhaps be thought, by some who have noted the supposed discovery made by Dr. Hastings and Professor Holden during the eclipse of May last, that astronomy ought to assure itself that the corona exists, before attempting to photograph it. If the corona has really been proved to be merely a phenomenon of diffraction, as has been so confidently and also so strangely asserted, the diffraction taking place at the moon's edge, then, of course, when the moon is not there to produce that diffraction corona, it is idle to attempt to photograph what—in that case—has no existence even as an optical phenomenon.

It has been with not a little surprise that the news of this noteworthy discovery has been received by astronomers. An observation which, if it prove anything, proves only what everyone knew must be the case—viz., that light passing close by the moon in total eclipse undergoes diffraction—is astoundingly accepted as explaining the solar corona with its complex structure, its long streamers, its faint extension along the zodiac even beyond the streamers five millions of miles in length seen by Professor Cleveland Abbe in 1878, and by General Myer in 1869. It is perfectly well known that diffraction could account only for a fine coronal ring of light, not even for the inner bright corona, still less for the structured corona

* From the *New York Tribune*.

near the sun, and least of all for the long streamers. Yet the mere circumstance that Dr. Hastings saw what it was practically certain beforehand he would see if he looked for it—viz., evidence of diffraction—is at once taken as full and complete evidence about matters with which it is not in the remotest degree connected.

The theory that the corona is not a solar appendage was not altogether an unreasonable, though it was a demonstrably wrong theory, fifteen years ago. It was clear even then to those who considered the matter attentively that none of the non-solar theories which had up to that time been advanced (including the diffraction theory discussed half a century ago by Baden Powell) were sound. But even the scientific world has been slow to accept the results of mere reasoning; so that in 1869, when the celebrated American eclipse occurred, astronomers were beginning to hope that photography would dispose of the solar corona as it had already disposed of the solar prominences. There had been some who denied that the coloured prominences could belong to the sun, pointing to difficulties akin to those which Mr. Larkin urges, I see, in *The Kansas City Review*, against the doctrine that the corona is solar. Then photography, showing in successive views of the totlaity in 1860 how the moon passes athwart the coloured flames, disposed definitely of the lunar and atmospheric theories of the garnets round a brooch of jet, as the coloured flames had been poetically called. Photography in 1870 did the like for the corona. An American photographer at Xerez, in Spain, and an English photographer at Syracuse, in Sicily, showed in their views the same radiations, rifts, gaps, and general structure in the corona, which could never have happened if the atmospheric glare, the lunar explanation, or any other but the solar theory of the corona had been sound. In 1871, in India, six photographs taken at Baikul, close to the sea-shore and six taken at Ootacamund, some 10,000 feet above the sea-level, showed the same coronal features, all twelve of them. This was not so much a demonstration as *the first easy proof* of the solar nature of the corona,—for even a schoolboy (not Macaulay's schoolboy, who knew everything, but a real one) could see that, unless the corona were far beyond our atmosphere and far also beyond the moon, it could not possibly show the same features as seen, not only from stations hundreds of miles apart, but also at the beginning of totality when the moon's eastern edge is just hiding the corresponding edge of the sun, and throughout totality to the last moment when the western edges of the globes are in apparent contact. Consider the mighty shadow of the moon sweeping along past and over the observer, remembering what the shadow in our air really is, a great cylindrical (really a frustum of a cone but very nearly cylindrical) region of darkness from fifty to a hundred miles or so in diameter; and see the impossibility that when the observer is on the extreme eastern side and on the extreme western side of that shadowed region he should see the same appearance in the air, or anywhere but in a region many millions of miles away, as around the sun. Add the impossibility that at stations two or three hundred miles apart the same appearances are seen, and not only seen but pictured by the unerring pencil of photography.

Yet it must be admitted that a certain interpretation of the corona as a solar appendage is so full of difficulties that one cannot wonder at its having proved a stumbling block to many. I mean the view that the corona is a solar atmosphere. The existence of gaseous matter in the corona does not any more prove, as some seem to imagine, that the corona is an atmospheric envelope, that the existence of gaseous matter in comets, demonstrated over and over again, proves that comets form an atmospheric envelope of the sun. The whole aspect of the corona seems to me to show unmistakably that the several parts of that solar appendage are as free from atmospheric association with the sun as are meteor streams and the heads and tails of comets. I doubt even for my own part whether what we call the visible surface of the sun indicates the extension of a continuous solar atmosphere to that distance from the sun's centre. And that the sierra (which some still call the "chromosphere," a word as correct and pleasing as "photograph" to a classical ear) is not really an atmospheric envelope, in the correct sense of the expression, seems clear when we consider its depth and the inconceivable pressures which would exist at the base of such an atmosphere under the solar gravity, exceeding more than twenty-seven fold that at the earth's surface.

If Dr. Huggins's photographs of the corona are real, the doubts even of those not capable of understanding the photographic evidence already obtained will be dispelled; and to question the solar nature of the corona will be held as obviously absurd as it would now be in the presence of the daily study of the solar prominences to maintain that *they* are only phenomena of diffraction.

Rather strangely in the same number of the *New York Tribune* the following remarks from Professor Young, of Princeton, N.J., appear:—Granting for the moment that the corona is in part and largely composed of an envelope of exceedingly rare gaseous matter around the sun—then we may call it an atmosphere, because being gaseous, and attached to a cosmical body, it bears to that body a relation analogous to that borne by our atmosphere to the earth itself. So far the term is a proper one. But now, further, and on the contrary, the term "atmosphere" carries with it to most persons certain ideas as to the distribution of temperature, density, &c., in its different parts, which are based on the fact that our terrestrial atmosphere is nearly quiescent and in static equilibrium under the force of gravity, with a temperature not more than two or three hundred degrees above the absolute zero, while the density of the portion accessible to human observation is very considerable. On the sun the conditions are immensely, and almost inconceivably, different, so that the term "atmosphere" becomes a very misleading one. There the equilibrium, so far as there is any, is dynamical, not statical, and the density, temperature, and condition of the gaseous substance is far more nearly that of the residual gas in a Crookes's vacuum tube through which an induction coil is sending electrical discharges; so different from that of ordinary air that Crookes thought he had found a fourth state of matter, bearing some such relation to the gaseous state as the gaseous does to the liquid.

EFFECTS OF THE GLACIAL PERIOD.

II.—ORGANIC EFFECTS.

By Robert B. Cook.

When we remember that the climate of the Glacial Period was preceded by an unusually warm and temperate one, evidenced by the remains of a sub-tropical fauna and flora found in the Eocene and Miocene deposits of England and the Continent, it is very obvious that the great cold of the Glacial climate must have produced many and various effects upon the organic beings which then inhabited those parts of the world.

In the first place, many species which were unable to adapt themselves to the changed condition of the climate by migration or otherwise would, undoubtedly, perish and become *extinct;* the absence from our present fauna and flora of a great number of animals and plants which existed here in pre-glacial times being accounted for in this way. Besides, if the supposition alluded to in our last article, that the close of the Glacial Period was accompanied by great floods, due to the melting of the ice, be correct, such floods would account for the extinction of numerous other forms of life which existed in plenty during that period before their occurrence. The total absence at the present day from countries like Siberia of such animals as the mammoth has been explained in this manner. But, however such extinction may have been brought about, it is certain that numerous animals which once roamed the plains of Europe, during the Glacial Period, have now become extinct, and are found no longer in any part of their ancient abodes. Among such animals may be mentioned the mammoth, and other species of elephant, rhinoceros, hippopotamus, bear, lion, hyæna, and many smaller animals now extinct.

More important, however, than any direct or indirect effect of the Glacial Period in the *extermination* of species, are the great changes which that period produced in the *geographical distribution* and *modification* of species.

First, let us endeavour to picture very briefly the *changes in geographical distribution* effected by the Glacial Period. Before the commencement of that period we have evidence that the inhabitants of the northern part of the world were temperate, and in many cases sub-tropical in character, thus indicating a much warmer climate in the northern hemisphere generally. But as during Pliocene times the temperature gradually diminished, these temperate and sub-tropical forms of life would be compelled to migrate southwards, their places being supplied by the more northerly forms which would then invade the temperate regions, until, by the time that the cold had reached its greatest intensity during the Glacial Period, an Arctic fauna and flora would have spread itself over the whole of the north temperate zone. Then, when the amelioration of the climate began, and very slowly and intermittently progressed, would begin and progress in a similar manner the gradual retreat of animals and plants to the north. But such retreat would be by no means such a simple matter as the invasion had comparatively been. Some of the invaders, no doubt, became themselves adapted to the change of climate, and formed settlements in the south, from whence their modified descendants have since spread into adjacent countries; others, in the neighbourhood of mountains, instead of taking part in the general retreat to the north, gained the same object of adaptation to climate by ascending such mountains and settling there. Thus, as we saw in a previous paper, when we considered this same subject as an "evidence" of the Glacial Period, we now have a number of Arctic forms of life, isolated on several mountain summits in the temperate regions, having been left by their retreating brethren in such situations, where they have become surrounded by temperate forms, which now cut them off from their allies who have reached a home in the Arctic regions of the north. The similarity which also characterises some of the so-called "representative species" of Europe and America is also explicable by a common southward migration, caused in both continents by the glacial climate, of forms of life which formerly lived together and spread over the whole of the circumpolar land, which in pre-glacial times enjoyed a comparatively mild climate. The whole subject of the effect of the Glacial Period upon the geographical distribution of species is one which, owing to the complex relationship of one species to another, it is difficult adequately to understand, for we may rest assured that, in addition to the well-ascertained effects that we can see, the Glacial Period also produced numerous other effects in distribution, both direct and indirect, which it is difficult, if not impossible, for us after such a lapse of time to discover. It is certain, however, that the present geographical distribution of animals and plants has been, in a great measure, determined by the geological episode of the Glacial Period.

With regard to the effects of the Glacial Period in the *modification* of species, such subject is also attended, to a considerable degree, by the difficulties just alluded to. There are, however, one or two instances in which such effects are sufficiently obvious, and these will now claim our attention.

When we consider that Arctic animals are generally *white* either all the year round or during the winter season, I think it is by no means improbable that many of them first obtained during the Glacial Period the white colour which they now possess either always or seasonally. For example, the polar bear and hare, living in regions where snow is always on the ground, are always white; while the ptarmigan, ermine, and Arctic fox, which inhabit countries where the snow is melted during summer, possess their white covering only so long as the snow remains. This white colour, which serves in snow-covered lands as a protection against enemies, and enables its possessors to approach their prey unobserved, has doubtless been gained by the agency of natural selection, or the survival and propagation of the white varieties, which by their colour possessed an advantage in attacking or escaping from their enemies, and the extermination of the dark varieties, which by their lack of such advantageous colouring were exposed to early destruction either by starvation from inability to capture their prey, or by the raids of their enemies. Probably, however, the Glacial Period was the cause which set this agency in motion, for before that lengthy period of ice and snow, there would be no need, during the warm, pre-glacial times, for the special white dress of Arctic species.

But perhaps the most interesting effect of the Glacial Period in the modification of species has been the wonderful *instinct of migration in birds* to which it in all probability gave rise. The vast numbers of swallows and other summer visitors to our islands which with the approach of autumn depart to the South in search of a more abundant food supply, and their regular return in spring, furnished for a long time an inexplicable mystery to those who were content with the confession of ignorance involved in the belief that the migratory instinct had been mysteriously implanted in some species and not in others at the time of their creation. But since the birth of the doctrine of development a more satisfactory answer has been found to a problem which it was formerly considered useless, if not impious, to attempt to solve. We have seen that there is evidence from fossil remains that a warm climate, during which evergreens and insects flourished in high northerly latitudes, preceded the Glacial epoch. Such being the case, we may fairly assume that the birds which now migrate in winter then resided permanently in the regions where they now breed, for, so long as there was sufficient warmth and food supply all the year round, there would be no motive for migration. But the gradual decrease of temperature which culminated in the Glacial Period would, in all probability, by first cutting off the food supply during mid-winter, compel the birds to migrate at that time of the year, returning by force of habit to their breeding-places in the spring. Probably at

first such migrations were performed over very short distances; but as the cold gradually increased, and the winters, even in the temperate zone, became more severe, the winterly migrations would of necessity have to be gradually extended further; and as the habit of returning during the summer to the original breeding-ground had no doubt become fixed by inheritance, it would still continue, and so would arise the phenomena of birds breeding during the short summer in the north, and undertaking long migrations in search of food and warmth during the remainder of the year. Both migrations, the return in spring and summer to the original breeding ground, and the winter departure to other climes for food and warmth, continuing through a period of several thousands of years, would become firmly established by inheritance as fixed habits; and so we may explain, not only why the swallows and other birds leave us in autumn —a failure in the supply of their insect food, which compels them to migrate or else perish, combined with the inherited habit, rendering an explanation of their departure comparatively easy—but also the much more difficult question why in spring they leave their warm abodes in the south to visit again for breeding purposes the old northern homes of the respective species to which they belong.

In conclusion, I would repeat that it is almost impossible now to ascertain all the effects which the Glacial Period has produced upon the world in which we live and its inhabitants. Those, however, which we can ascertain are full of the greatest interest, and may afford us unlimited pleasure in our contemplations on Nature, during our holidays among the mountains, the lakes, or in the country.

EVOLUTION.*

By Professor Marsh, of Yale College.

Modern science and its methods may be said to date back only to the beginning of the present century; and at this time the first scientific theory of organic evotion was advanced by Lamarck. During the twenty centuries before, a few far-seeing men, from Aristotle to Buffon, seem to have had glimpses of the light, but the dense ignorance and superstition which surrounded them soon enveloped it again in darkness.

Before the beginning of the present century, it was impossible for evolution to find a general acceptance, as the amount of scientific knowledge then accumulated was too small to sustain it. Hence, the various writers before Lamarck who had suggested hypotheses of the development had based them upon general reasoning, or upon facts too scanty to withstand the objections naturally urged against new ideas.

With the opening of the nineteenth century, however, the new era in science began. Here, at the very beginning, the names of Cuvier and Lamarck stand forth preeminent; and the progress of natural science from that day to the present is largely due to their labours. Cuvier laid the foundation of the study of vertebrate animals, living and extinct, but with all his vast knowledge he was enslaved by the traditions of the past. Although the evidence was before him, pointing directly to evolution, he gave the authority of his great name in favour of the permanence of species.

Lamarck made a special study of invertebrate animals, and his investigations soon led him to the belief that living species were descended from those now extinct. In this conclusion he found the germ of a theory of development, which he advocated earnestly and philosophically, and thus prepared the way for the doctrine of evolution, as we know it to-day.

The methods of scientific investigation introduced by Cuvier and Lamarck had already brought to light a vast array of facts which could not otherwise have been accumulated, and these rendered the establishment of the doctrine of evolution for the first time possible. But the time was not yet ripe. Cuvier opposed the new idea with all his authority. The great contest between him and Geoffroy Saint Hilaire, the strongest advocate of Lamarck's views, is well known. Authority, which in the past had been so powerful in defence of tradition and creed, still held sway, and, through its influence, evolution was pronounced to be without foundation. This triumph of Cuvier delayed the progress of evolution for half a century.

During this period, however, the advance in all departments of science was constant, and the mass of facts brought together was continually suggesting new lines of research, and new solutions of old problems. In geology, the old idea of catastrophes was gradually replaced by that of uniform changes still in progress; but the corollary to this proposition, that life, also, had been continuous on the earth, was as yet only suggested. In the physical world the great law of the correlation of forces had been advanced, and received with favour; but, in the organic world, the miraculous creation of each separate species was firmly believed by the great mass of educated men. The very recent appearance of man on the earth and his creation independent of the rest of the animal kingdom were scarcely questioned at the close of the first half of the present century.

When the second half of the century began, the accumulation of scientific knowledge was sufficient for the foundation of a doctrine of evolution which no authority could suppress and no objections overthrow. The materials on which it was to be based were not preserved alone in the great centres of scientific thought, but a thousand quiet workers in science, many of them in remote localities, had now the facts before them to suggest a solution of that mystery of mysteries, the Origin of Species.

In the first decade of the present half century, Darwin, Wallace, Huxley, and Spencer were all at the same time working at one problem, each in his own way, and their united efforts have firmly established the truth of organic evolution. Spencer did not stop to solve the difficulties of organic evolution, but, with that profound philosophic insight which has made him read and honoured by all intelligent men, he made the grand generalisation that the law of organic progress is the law of all progress. To show how clearly, even in the beginning, he comprehended this great truth, let me recall to you one sentence which he wrote five-and-twenty years ago.

"This law of organic progress is the law of all progress. Whether it be in the development of the earth, in the development of life upon its surface, in the development of society, of government, of manufactures, of commerce, of language, literature, science, art, this same evolution of the simple into the complex, through a process of continuous differentiation, holds throughout."

How completely the truth of this statement has since been established you all know full well.

The evolution of life and of the physical world are now supplemented by the evolution of philosophy, of history, of society, and of all else pertaining to human life, until we may say that evolution is the law of all progress, if not the key to all mysteries. These profounder depart-

* A speech delivered at the Spencer Banquet by Professor O. C. Marsh, the eminent American geologist, acting president of the National Academy of Sciences in America, a year and a-half ago.

ments of evolution I leave to others, for, in the few minutes allotted to me, I cannot attempt to give even an outline of the progress of evolution in biology alone.

If, however, I may venture to answer briefly the question, What of evolution to-day? I can only reply: The battle has been fought and won. A few stragglers on each side may still keep up a scattered fire, but the contest is over, and the victors have moved on to other fields.

As to the origin of species, once thought to be the key to the position, no working naturalist of to-day who sees the great problems of life opening one after another before him will waste time in discussing a question already solved. This question, so long regarded as beyond solution, has been worked out by that greatest of naturalists, whose genius all intelligent men now recognise, and whose recent loss the whole civilised world deplores.

Not only do we know to-day that species are not permanent, but every phase of life bears witness to the same general law of change. Genera, families, and the higher groups of animals and plants are now regarded merely as convenient terms to mark progress, which may be altered by any new discovery.

All existing life on the earth is now believed to be connected directly with that of the distant past, and one problem to-day is to trace out the lines of descent. Here embryology and paleontology work together, and the results already secured are most important. The geneaologies of some of the animals now living have been made out with a degree of certainty that amounts to a demonstration, and others must rapidly follow.

In this, and in all other departments of natural science, the doctrine of evolution has brought light out of darkness, and marks out the path of future progress. What the law of gravitation is to astronomy the law of evolution is now to natural science. Evolution is no longer a theory, but a demonstrated truth, accepted by naturalists throughout the world.

The most encouraging feature in natural science—indeed, in all science to-day—is the spirit in which the work is carried on. No authority is recognised which forbids the investigation of any question, however profound; and, with that confidence which success justly brings, no question within the domain of science is now believed to be insoluble; not even the grand problems now before us—the antiquity of the human race, the origin of man, or even the origin of life itself.

THE SPREAD OF THE KRAKATOA DUST-CLOUD.

By A. Cowper Ranyard.

(*Continued from page 178.*)

The great outbreak of Krakatoa commenced on the afternoon of Sunday, the 26th of August, 1883, and continued with extraordinary violence till about mid-day on Monday, the 27th. There had been a minor outbreak three months previously, on the 20th of May, and the mountain continued smoking and throwing out pumice in small quantities till the great outbreak of Sunday, the 26th of August. On that evening and the following morning an immense amount of material was thrown into the air, so that on the 27th in the neighbourhood of Anger it was as black as night at noon. Capt. W. J. Watson, whose ship was at the time 30 miles from Krakatoa, says, "At noon the darkness was so intense that we had to grope our way about the decks, and although speaking to each other on the poop, we could not see each other."

A great deal of pumice and mud fell in the Sunda Straits and over parts of the islands of Java and Sumatra, and, for a distance of three or four hundred miles round the volcano, a whitish dust was deposited on the decks of ships. On the island of Java it covered the trees and soil so thickly that the Dutch settlers were reminded of the winter appearance of the landscape in Holland.

The cloud of smoke and dust in the upper air seems to have extended with the greatest rapidity towards the west. By the evening of Tuesday, Aug. 28, it had travelled to the Mauritius, a distance of over 3,000 miles. Mr. Meldrum writes in the "Mauritius Meteorological Society's Proceedings" * for Oct. 27, 1883, p. 10: "Shortly after sunset on Aug. 28, the sky and clouds were coloured yellow and red up to the zenith, and before sunrise, on the 29th, the whole of the eastern sky had a fiery-looking appearance, as if there were an extensive conflagration below the horizon. On the three following days the colorations were still more intense."

About the same time an abnormal appearance of the sun was observed at the Seychelles, over a thousand miles to the north of the Mauritius, and three thousand miles due west from Krakatoa. "At the Seychelles, on the morning of the 29th, the sun was more like a full moon than anything, and at sunset on the 28th it looked as it does through a fog or on a frosty day in England."† It will be noticed that at the Seychelles only the dimming of the sun's light was observed; the red colour of the sunsets was not noticed, nor is there any mention of the blue or green appearance of the sun's disc, either here, or at the Mauritius. Probably the particles floating in the air on the 28th and 29th were too large to give rise to the blue colour of the sun which was seen at a greater distance from Krakatoa, and after an interval of time in the neighbourhood of the eruption.

It seems that the haze was much more dense at the Seychelles than at the Mauritius, which may be accounted for by assuming that the Seychelles were near to the centre of a broad stream of dust-laden air, which flowed away from the Sunda Straits, while the island of Mauritius lay on the southern edge of the stream.

We next hear of an unusual appearance of the sun having been observed on the morning of Saturday, August 29, at Cape Coast Castle, more than seven thousand miles away from Krakatoa, nearly in the same direction as the Seychelles. Mr. W. B. Griffith, writing to the *Times* of Dec. 5, gives the following extract from the *Gold Coast Times* of Sept. 14:—

"On the 1st and 2nd of this month the sun was described as being blue in the morning. It seems it rose as usual, and that the clouds which passed over it, from their greater rarity or density, gave it different apparent shades of rose colour, pink, and so on. After the passage of the clouds its appearance through the haze was white, like the moon. In fact, an Englishman is said to have taken it for the moon."

By the morning of Sunday, September 2, similar phenomena were observed twelve thousand miles away from Krakatoa, at Barinas, Venezuela, in South America. Mr. G. J. Symons, in his letter to the *Times* of December 1 (which was the first communication that suggested that dust from the Krakatoa eruption was the cause of the extraordinary sunsets seen in England), states that at Barinas from sunrise to noon the sun looked like burnished

* My thanks are due to Mr. A. Ramsay, secretary of the Royal Society's Krakatoa Committee, for this reference.

† This quotation is taken from Mr. Lockyer's article in the *Times*, of Dec. 8. He does not give the authority from which he quotes.

silver, and from noon to 3 p.m. it was bluish-green. Similar phenomena were also observed on the Sunday afternoon in the island of Trinidad. In a letter from Dr. Arnold to the *Times*, he states that about five o'clock the sun looked like a blue globe, and after dark the bright redness of the heavens gave rise to the idea that there was a fire in the little town of Port of Spain.

It will be noticed that Barinas and Trinidad do not lie very far from the line drawn from Krakatoa through the Seychelles, and produced onward past Cape Coast Castle. This line does not run due west from Krakatoa, but is inclined somewhat northward. Krakatoa is in latitude 6° 9′ south, and Port of Spain is 10° 40′ to the north of the equator. Possibly the central line of the stream was somewhat less inclined to the equator. The silvery appearance of the sun seen at Barinas, but not seen from Trinidad, seems to show that Trinidad was on the northern edge of the stream, and that the dust-cloud was thicker at the more southern station.

If it is assumed that the phenomena observed at Barinas and Trinidad were caused by dust thrown into the air on the previous Sunday in the Sunda Straits, it will follow that matter was carried half round the earth with a velocity of 1,700 miles a day, or 71 miles an hour, kept up for a whole week. Such a supposition at first appeared incredible, especially as there seemed to be evidence to show that the prevailing winds were in the opposite direction.

The barographs or automatic traces made by barometers at the chief meteorological observatories on Aug. 27 and the next five days show a series of well-defined disturbances, the first of which is a rise, followed by a subsequent depression, which, at the European and American stations, corresponds to a difference of more than a tenth of an inch of mercury. At some observatories, as many as seven of such waves, gradually decreasing in magnitude, can be detected, while at other observatories, where the instruments were not so sensitive, only five can be traced. General Strachey has, in a paper communicated to the Royal Society, collected the observations made at all the chief meteorological observatories, and has pretty conclusively shown that a great atmospheric wave started from the region of Krakatoa at about 9 h. 30 m. a.m., on Aug. 27. It spread outwards in all directions, and travelled round the globe, the parts of the wave which had travelled in opposite directions passing through one another somewhere in the antipodes of Java. The wave then returned upon itself to the region of the explosion, and again passed round the globe. The seven irregularities which are recognisable in the barograph curves of several observatories show that the wave must have passed three and a quarter times round the earth before it was sufficiently reduced to make it too weak to affect the instruments.

General Strachey has shown that the velocity of the waves in miles was, for those which travelled from east to west, 674 miles per hour, and for those passing from west to east, 706 miles per hour. The velocity of sound in air, at a temperature of 60° Fahr., is 771·1 miles an hour; with a temperature as low as zero Fahr., the velocity will only be reduced to 724 miles an hour, which is still considerably in excess of the velocities observed. This would seem to indicate that the waves travelled at a great height in the atmosphere where the temperature was considerably below zero. The excess of the velocity of the waves which travelled in the same direction as the earth's motion of revolution, that is from west to east, over that of those which passed in the opposite direction, is about thirty-two miles an hour, which, General Strachey suggests, may be accounted for "by the circumstance that the winds along the paths of the waves would, on the whole, be from the west, which would cause an increase in the velocity of the one set and a diminution in that of the other, so that the observed difference of thirty-two miles would correspond to an average westerly wind of sixteen miles an hour," which he thinks is not improbable.

The path of the wave which passed through Toronto approached very near to the north and south poles, and the velocity in both directions is somewhat less than in the waves which passed over central Europe. The wave which passed northwards over Asia travelled, according to General Strachey, at the rate of 660 miles an hour, or about 15 miles an hour slower than the wave which passed over Great Britain from east to west. This reduction of rate, it is suggested, may be accounted for by the slower velocity of the waves in the cold air of the polar regions.*

* Six hundred and sixty miles an hour is equivalent to 968 feet per second, which corresponds to the velocity of sound in air at a temperature of −76°·5 Fahr. The mean of the velocities of the waves which passed across Europe in an easterly and westerly direction was 690 miles an hour, which corresponds to the velocity of sound in air at about −42° Fahr. The difference of pressure at a great altitude would make no difference in the velocity of the sound-waves. The absence of aqueous vapour might make a small difference, but the velocities of sound, determined experimentally by Capt. Parry during his third voyage to the Arctic Regions at temperatures varying from −38°·5 to +35° Fahr., agree so nearly with the theoretical values derived from the formula I have used in calculating the above-mentioned velocities that it seems probable that the difference caused by the absence of vapour in cold air may be neglected. Theoretically, loud sounds, where the displacement of air is large, should travel a little faster than sounds that are not so loud. The constants in the formula used have been derived from experiments made by observing the flash of a cannon fired at a distance—no doubt the displacement of the air caused by the Krakatoa explosion was very large compared with the displacement caused by the sound-wave from the largest cannon. If such great displacements travel much more quickly than the sound-waves from cannon, we should be forced to assume that the air in which the waves from Krakatoa travelled was colder than the above mentioned temperatures. Capt. Parry and Mr. Fisher, who observed the velocity of sound together, during Parry's second voyage, make a curious remark with regard to their experiments, which bears on this subject. The stations were so near that the human voice could be heard, and they mention that the officer's word of command, "fire," was heard about one beat of the chronometer (⅖ of a second of time) *after* the report of the gun. Sir G. B. Airy remarks in his treatise on Sound, that he cannot imagine that the acceleration could, at the distance at which Parry observed the cannon, amount to a space of 200 feet. He thinks it more probable that the phenomenon was physiological, and adds: "When the report of a gun or any other violent and sudden noise is heard, it is preceded by the perception of a shock through the bodily frame, the interval in time being a large fraction of a second. From the voice there would be no sensible shock; but the shock from the cannon-explosion might be sensible, and might precede the auditory perception of the report by a time sufficiently long to present itself to the observer's mind before the auditory perception of the voice." But if this were the case, distance from the cannon ought not to have made any difference, and the officer giving the order, as well as the man firing the cannon, ought to have heard the report of the cannon ⅖ of a second before the officer's voice was heard. Judging from my own experience, I do not think that this can be the true explanation. Such experiments might now be repeated with much greater precision. A microphone might be used to register automatically on a chronograph the arrival of a sound made by a clock a second before an explosion of dynamite on a distant mountain, and the time of the flash, and the arrival of the great sound waves could also be registered automatically. The *first* and *second* of the series of the great air-waves from Krakatoa are, in almost all the Barographs, well defined and generally similar in form, commencing with a distinct rise, followed by a fall, the fall being shorter than the rise. These features are followed by a less definite rise, succeeded by a shallow fall, after which there is again a rise, which gradually passes into the more regular trace. The *third* and *fourth* of the disturbances can be traced in all the curves, but they no longer exhibit the same characters, and are usually nothing more than a sudden, sharply-defined rise, though in front of some of these there is a more or less distinct trace of a hollow. The *fifth*

Mr. G. J. Symons, in his interesting letter to the *Times*, of Dec. 1, endeavoured to account for the rapid spread of the cloud of ejected matter in a westerly direction by suggesting that the dust would be left behind by the rotating earth when thrown into the upper regions of the atmosphere, but if matter was thrown to a height of seventy miles above the earth's surface (the height at which the majority of meteors become visible), and was not carried forwards by the air at the higher level, it would only fall behind about 440 miles in the twenty-four hours, whereas we have to account for a westerly velocity of 1,700 miles a day. Mr. Symons has since suggested that the matter might have been carried altogether outside the earth's atmosphere, and so have got a whole hemisphere behind. But this theory is seen to be untenable when we remember that meteoric particles, with an initial velocity of many miles per second, do not, unless they are exceptionally large, get through the outer half of the earth's atmosphere without being driven into vapour. If the height of projection were the cause of the westerly drift, the ashes and large particles would have been left further behind than the fine dust, for the larger particles would experience less resistance, and consequent loss of velocity in passing outwards through the earth's atmosphere than the dust, the larger particles would consequently have been projected upwards to a greater altitude, and have fallen further behind than the dust; but, in fact, we find the larger debris falling near the volcano, and the dust carried to great distances.

It is evident, therefore, that the finer particles must have been carried upwards by heated air, and afterwards have been borne along by the wind. Krakatoa is situated in latitude 6° 9′ S. On Aug. 26, the sun had a northern declination of 10° 20′, so that in its daily course it passed vertically over the region 10° north of the equator, heating it more intensely than the zones to the north or south. The phenomena of the trade winds indicate that there is an ascending current in the region where the sun is vertical, and that the air which rises is chiefly drawn from regions to the north and south, where the velocity of rotation of the earth's surface is less than the velocity of rotation where the heated air rises. We should consequently expect to find the ascending current of heated air lagging behind the surface of the earth beneath it, and giving rise to a strong westerly wind in the upper regions of the atmosphere where the sun is vertical at noon.

The heated air having been drawn from the north and south on the earth's surface flows out again towards the poles in the upper regions, and in time must arrive at a latitude where its rotational velocity is equal to the rotational velocity of the earth's surface beneath it. Beyond this region the current of upper air would appear to turn backwards and flow in a contrary direction relative to the earth's surface—that is, from the west towards the east. If at the time of the eruption the equatorial current had a westerly velocity of 1,700 miles a day, its motion of rotation or velocity round the earth's axis would correspond with the velocity of the earth's surface in latitudes 22° north and south, and further north and south the upper current would be blowing backwards, or in an easterly direction.

It is therefore evident that waves starting from Krakatoa and travelling in the upper air towards the north-west so as to pass across Europe and America would in their whole course round the earth from east to west encounter more adverse winds than waves passing in an opposite direction round the earth from west to east, while in passing round the earth in the equatorial regions the waves would travel 3,400 miles a day more rapidly from east to west than from west to east. What at first appeared contradictory is now seen to be a necessary result of the rising of heated air in the neighbourhood of the equator.

The direction "slightly to north of west" in which the cloud of dust from Krakatoa was carried is also explained, since the sun was vertical in latitude 10° north at the time of the eruption, and the heated air would be rising fastest to the north of Krakatoa, the smoke and lighter dust borne away by the wind would be carried into the main stream flowing backwards or in a westerly direction with greatest velocity in latitude rather more than 10° N., and it would then gradually spread outwards towards the north and south.

We find that the blue and green appearance of the sun and red sunsets were first visible in Ceylon and India on Sept. 9, which is about the time that the great stream of dust and smoke which stretched out to the westward from Krakatoa would have occupied if it had passed entirely round the world and had travelled at the rate of 1,700 miles a day. But the news received from Japan renders it probable that the dust-cloud spread more directly towards the north-east. An extract from the *Japan Gazette* for Sept. 21, 1883, published in *Nature* for April 3, 1884, describes phenomena observed at Tokio on August 30, 31, and Sept. 1, similar to the phenomena observed at Cape Coast Castle on 29th August, 7,000 miles westward from Krakatoa. Tokio is about 2,500 miles from Krakatoa, and the spread of the dust-laden air must consequently have been at the rate of about 600 miles a day towards the north-east.

From the *Chilian Times* of Oct. 27, 1883, I find that the red sunset phenomena were first observed at Carrizal Bajo, about 300 miles north of Valparaiso, about Oct. 5. The sunset phenomena did not attract general attention in England till Nov. 9, 1883.

and *sixth* of the series become less distinct, and are lost at several stations, being usually rises; while the *seventh* faint disturbance is a shallow hollow that can be traced on a few of the curves.

The late Mr. Earnshaw, in an important paper published in the *Philosophical Transactions* for 1860, investigated the velocity of transmission of different parts of a sound wave, and has shown that in the case of a violent sound, generated with extreme force, every part of the wave, with the exception of its rear, is perpetually gaining on the front, and the result is a *constant change of type*, the more condensed parts hurrying towards the front, with velocities greater as their densities are greater. Mr. Earnshaw remarks that "this cannot go on perpetually without its happening at length that a *bore* is formed"; that is, the wave will break forward somewhat as waves break when advancing on a gradually shallowing shore, or up a narrowing tidal river. Mr. Earnshaw also shows that a negative or rarefied bore would be formed in the rear of a wave of rarefaction. The great air-waves from Krakatoa form an excellent illustration of the results theoretically arrived at by Mr. Earnshaw. The vast scale of these waves may be judged of from the fact that the first main-wave or rise took about an hour to pass over the meteorological stations; that is, it was about 700 miles in length. The whole length of the first series of disturbances was between 3,500 miles and 4,000 miles. The series occupies between five and six hours on the time-scales of the Barographs. If the time of the breaking up of the wave and the first formation of the bore could be determined, we could, knowing the length of the wave, determine the acceleration of its central parts, and could form a more accurate estimate of the mean temperature of the air in which the wave was transmitted.

THE TELEGRAPH IN A GALE.

By W. Slingo.

It has been truly said that the present age may be described as that of electricity, for of a surety in no other branch of science have such rapid and gigantic strides been made as those which have resulted from the scrutiny of many great minds into the whence, the wherefore, and the

whither of electricity. Apart from these considerations, the very subtlety of the force, its incomprehensibility, and the mystery that surrounds it at all times and under all conditions, may well be calculated to imbue one with a peculiar sensation of wonder and amazement. "How wonderful!" is the expression of every visitor to a large and busy telegraph office. The miles of wires about the room, the multitude of mysterious instruments, the thousands of brass "terminals," each of which is known to the engineer by name and number, alike generate feelings of astonishment. It is at once apparent that the telegraphist's operations have no counterpart in the whole domain of social enterprise. And yet all the work that is seen to be going on, the multitude of appointments that are being telegraphically made or broken, the millions of pounds' worth of business that are being similarly transacted, are all more or less at the caprice of the wind and weather. To my mind there is no occupation so interesting, and few things so exciting or so calculated to upset one's mental balance, as to be in charge of a large system of busy telegraph wires during a gale. As I write my mind goes back to the afternoon of Saturday, Jan. 26, 1884, when one of the most violent storms which we have experienced for some years passed over the British Isles. The day had been a quiet one, and those on duty in the central office were composing themselves for the evening. It was known, however, that dirty weather had shown itself in the West; but the effect on the wires had been small, and little danger was feared. At four o'clock in the afternoon all their calculations were upset, and one of the busiest evenings ever known set in. What the effect would have been had the day been any other than on Saturday is inconceivable. However, the clock had struck four, and information was received that one of the nine Glasgow wires was stopped. No sooner had steps been taken to ascertain the locality of the stoppage than another and another gave out, and in a few minutes it became known that the whole of the Trunk line on the Carlisle and Glasgow road, which carried seven wires to Glasgow, was down. Simultaneously the five wires on the Caledonian Railway, between Carlisle and Beattock, gave way, leaving London without any communication to Scotland by the western routes, and stopping also the Belfast wires (going *viâ* Carlisle and Stranraer). Such a catalogue was in itself sufficient evidence, were there no other, to indicate the existence of a terrific gale. But it speedily showed its existence over the whole length of the land, wires running north and south falling not in ones and twos, but in batches. By six o'clock the storm had travelled from the Bristol Channel down the Thames valley to London; the gale increased in strength as time went on, and reached its height in London at about 7.30 p.m. But those locked up in the office, while having evidence of the devastation that was going on around, heard and saw nothing of the storm itself. Too busy to look out of window, they did not see the rain, nor did they hear the roaring of the wind. While all this had been going on, while wires were being tested north and south, the centre of the gale which wrecked the West coast wires travelled across the country, and eventually reached the East coast. Running through Newcastle were nine wires to Glasgow, Edinburgh, Dundee, and Aberdeen, and these shared the fate of their brethren on the other side of the Pennine chain. Down they came, wires, insulators, and poles; some pulled out of the ground, some snapped off short at the ground, some broken in the middle, and then hurled by the fury of the gale into the road, or more generally into adjoining fields. When this feat had been accomplished the word was passed, "Stopped to Scotland and the North of Ireland." In all there were, of the wires running out of London, upwards of 130 known to be broken down. As the wind has no respect for persons, "cross-country" wires (or wires connecting two provincial towns) suffered equally with the London wires, so that, for example, Scotland had no communication whatever with the south. Space will not permit a detailed review of all the stoppages, but a few comments on the general features of the breakdown may not be out of place. One remarkable point worthy of observation is that the general nature of the stoppages, and the routes principally affected indicate pretty clearly the direction and limits of the gale, its approximate force, and the line of greatest energy. From past experience it is easy to conceive that a gale has some one central line or belt, along which it travels, and in the path of which the greatest damage will be wrought. There were presented, however, in this belt or zone features of a most remarkable nature. At and previous to the commencement of the storm sleet fell somewhat heavily, but it soon gave place to snow. This, on coming into contact with the wires, froze on to them, and formed, as it were, heavy ropes of ice two to four inches in diameter. Great assistance was thus rendered to the gale in its destructive efforts. Two or three days afterwards long tubes of ice were found on the ground under the wires, each tube having a longitudinal slit which had been made by the wire in thawing—if I may use such a word—itself out. It is a noteworthy fact that in the whole history of telegraphy the only occasions on which these features were exhibited were in 1866, when the central belt, accompanied by snow and frost, passed south of London; in 1876, when it traversed the Midlands; and lastly, in 1884, when, as shown above, it passed along the Border. The belt in this particular storm was about 25 miles broad, and passed from west to east, being bounded on the north side by Ayr, Hawick, and Alnwick, and on the south side by Girvan, Dumfries, and Morpeth. Residents in the South of England have a vivid recollection of the violence of the storm, but strong as it was it is a remarkable fact that comparatively very few of the wires travelling east and west were affected. This fact, coupled with the circumstance that wires running at right angles to these were almost invariably impaired, afforded a sure indication to those on the spot of the direction of the storm, and the practical annihilation of all the three Scotch routes confirmed the inference, besides testifying to its greater virulence in the north. The comparative immunity subsequently shown to have been enjoyed by the wires north of Ayr helped to define the limits of the gale's central zone. To that zone a brief consideration will now be given. Among the laws of telegraph construction is one that requires the poles, where it is practicable, to be fixed with reference to the prevailing winds, on the leeward side of the road, the object being that in the event of a storm breaking the wires or poles they shall fall into adjacent fields instead of blocking the road. Trees falling from the opposite or windward side of the road would also be less likely to reach the wires and drag them down. Falling trees are a prolific source of injury to telegraph wires, and no amount of care and forethought is sufficient to guard against them. Sometimes their large surface cause them to be carried a considerable distance. In one instance, at Edgerston, on the Newcastle and Jedburgh road, a large tree fell, and "knocked one double pole* and all its fittings to pieces." In another portion of the road the large trees sheltered the wires; but, at Langlee, a tree 9 ft. in girth, broke off "about 6 ft. from the ground

* A double pole is in reality two poles fixed side by side, braced together above and below the ground, and inclining towards each other.

and turned a summersault over the line, only breaking one wire." While, however, trees did a deal of damage, the greater part of the injury sustained resulted from the pressure of the wind on the wires. Near Capheaton, "for about thirty-five to forty pole-lengths, the line was wrecked, fifteen poles being broken off short and many wires, insulators, and arms broken. Struts* were largely used, and proved to be quite useless." At Kirkwhelpington, where even worse damage was wrought, "seven double poles had been blown out of the earth, one actually on the other side of the road having drawn its stay,† and snapped its cross-brace in the middle." Near Horsley, "nine double poles were actually smashed to pieces, breaking at various points between the ground-line and 6 ft. above." So goes the record throughout the breadth of the zone, the number of breakages only varying, sometimes one or two poles, sometimes twenty-five became *hors de combat*. In one curve the seventeen wires were pulling against the wind with a united breaking strain of at least 22,000 lb., but all to no purpose.

The work involved in repairing such a wreckage is inconceivable, but the wires were got through in batches until, on Tuesday, the 12th of February, all the East-coast wires were "in working order." Had there been room, a few comments on the phases of the Western breakdown would have found a place. Suffice it however, to say that the breakdown on the Carlisle and Glasgow road had much in common with what was noticed on the Newcastle and Jedburgh road, while on the Caledonian Railway the wires were torn down for miles by passing trains. A fortnight or three weeks were spent in making reparations, but in the mean time the London staff was often at its wits' end in the effort to establish communication with Scotland. Usually there are 19 wires, all of which were stopped on Saturday evening. The first line was made up to Edinburgh on Monday afternoon. This channel of communication was soon closed, and another wire was got through to Edinburgh on the following evening. On Wednesday afternoon five or six wires were made up, some of them by utilising the good portions of several bad wires. Eventually the normal condition of things was restored, but many a year will go by ere the gale of Jan. 26, 1884, will be forgotten by telegraphists.

* Struts are wooden supports placed as a buttress against the weaker side of the pole to prevent it being pushed over.

† Stays are strands or ropes of stout iron wire fastened to the pole, and to an underground mass of stone, &c., for a similar purpose.

HOW TO SELECT A LIFE ASSURANCE OFFICE.

FROM the office having a long list of directors of well-known names, or with high-sounding titles? From the glowing statements in a report or prospectus? From the "advice gratis" of some kind friend or persuasive agent? From the offer of an exceptionally low premium? From the fact of large bonuses having been distributed? No! by none of these "royal roads"—that is, if you wish to make your investment to the best advantage, and to have no half doubts and fears after the deed is done. No, but from the facts given in the yearly returns to Parliament, collectedly published in a Blue Book, at the price of a few shillings, and a patient investigation thereby of the actual financial position of the office, and from other inquiries as to the status of the office. Certainly not very inviting work this—this digesting of figures and making of calculations—but for those who are pertinacious enough to do it, and know the value of the maxim that "Blessed are the hands that help themselves," I will point out what are the facts their inquiries should aim at ascertaining.

First—and this is the question of paramount importance—what security is offered, what ability has the office of meeting its death claims? Now, those that read my former article will remember that on principle an office receives what it has to pay back, and the premium it receives from an assured is larger or smaller according as the death claim will the sooner or later presumably have to be met. Hence the office ought to have a fund in hand—a life-fund proportionate to the premiums they are receiving, and the inquiry on this head may most conveniently be answered by ascertaining the amount of the life fund of the office for every £1 of premiums received. This can easily be done by dividing the amount of the life fund by the amount of premiums. Say, for instance, the premiums are £242,618, and the life fund is £4,337,492, the answer would be £17. 17s. 7d.

Secondly, how is the office managed? Are the assets well invested and easily realisable? In other words, taking the item "Interest and dividends," what rate of interest does it show the assets to be returning? What, too, is the amount of outstanding interest? How much is being spent in touting, puffing, advertising, printing, office rent, and expenses? That is, putting the question in a convenient form, what amount is spent in "commission and expenses of management" for every £1 of premium received?

If these facts are not satisfactory, we need go no further. If, however, they be satisfactory, we may proceed to ascertain those of lesser magnitude, the answers to most of which will not be found in the Blue-books.

Thirdly, how long has the office been established? Is the method of distributing the bonus good; or does that method offer the intending assured unusual advantages? What is the rate of premium? Has the office proprietors, or is it founded on mutual principles? If preference be given to an office having proprietors, to what extent do such proprietors participate in the profits, and by how much does the amount they receive exceed the amount of the current interest on the capital, or, to use other phraseology, what are the proprietors making out of the business, and the assured accordingly losing?

Fourthly, questions that may be asked *cœtaris paribus*. Is the office large and popular? This will be shown by the amount of premiums. Does it employ agents and give commissions?

Of course, it were useless to find out these facts respecting an office, without having data with which to compare them. It is therefore necessary to extend our inquiries further and ascertain the practice of the best offices, by ascertaining similar facts respecting some half-dozen other offices of the best reputation. All the Parliamentary returns being to one form renders it possible to obtain the same facts respecting any office without difficulty. When the necessary facts as to these offices have been obtained, it is well, in order to counteract the effect of the opinion of any particular board of directors, or the circumstances of any special office, and to obtain a better standard for comparison, to strike averages of the answers to our calculations. It must not, however, be thought that the answers to our questions are exact. To make them so would require the most minute inspection of the office books and securities, and a most careful valuation of the liability on each policy. But the answers we are enabled to obtain are sufficiently approximate to make them practically reliable.

The Telephone for Divers.—Orders have been issued that telephones for the use of divers are to be supplied to Government ships carrying such men.—*Electrician.*

CYCLING AND CYCLES.

By "Axle."

The art of cycling has bestowed a new faculty upon the human race. This is the opinion of the eminent physiologist, Dr. B. W. Richardson, expressed in an article recently contributed to *Longman's Magazine:* and in support of his assertion he has written, "By the simple machines, bicycles and tricycles, we are returning to first principles. We are endowing every person who can use these machines with a new and independent gift of progression; and to what extent this art will proceed in a quarter of a century, if it makes the same progress that it has made in the past twentieth part of a century, he would indeed be a bold man who would venture to predict."

Until quite recently, cycling has depended principally for the maintenance of its interest on the "sport" to be derived from its pursuit, but we are now becoming alive to the fact that it is to be viewed from much higher standpoints—those of its health-giving qualities and its usefulness as a medium for rapid conveyance. As regards the former, much has been said and written both for and against. Says Dr. Richardson — and he has devoted considerable attention to the subject —"I really know of nothing that has been so good for health." In the second case, there is no doubt that the cycle—more especially the tricycle—may be largely utilised as a "carrier." Our contemporary the *Standard* is already employing the tricycle in this capacity—for the delivery of newspapers within the metropolitan area,—and the vermilion-painted "carriers" used for this purpose are now familiar objects in the London streets. And obviously, in this connection, there are manifold spheres for the useful employment of the three-wheeled machine.

Regarded as a pastime, cycling is not of such recent introduction as many may be led to suppose, for as far back as 1769 we find recorded a description of a vehicle to be propelled by the action of its rider; and later, about the year 1820, a contrivance popularly known as the "dandy-horse" (inasmuch as it was principally patronised by the dandies of the period) was the subject of much public caricature and criticism.

It would be superfluous to attempt any detailed description of these early machines; and it will suffice to say that the "dandy-horse" was constructed with two wheels, one at either end of a horizontal bar, in the centre of which the seat was placed at such a height as to enable the rider to touch the ground with his toes without alighting, and by a similar action, as in walking, to propel the machine along. A cross-bar was fitted just in front of the seat, upon which the rider leaned with his hands; and a simple steering arrangement was attached. With practice, it was possible to travel upon this primitive bicycle at a fairly rapid pace; but the vibration consequent upon its use was, as may be imagined, considerable, and not at all calculated to impress a rider with the idea of comfortable progression. For this reason, and also, perhaps, on account of the ludicrous appearance presented by a person when mounted upon the machine—which called forth the ridicule alluded to above —the "dandy-horse" soon fell into desuetude; and though various improvements were from time to time attempted, nothing of importance was accomplished until 1856, in which year the afterwards famous "Velocipedes" began to attract considerable attention.

These velocipedes were constructed to carry one or two persons (and in some cases even three, *à la* tandem), and were made principally of wood, with light wooden wheels, iron-tyred. Their axles were cranked, and connected with the pedals by an iron rod, and were sometimes jointed to the front part of the machine, thus producing a lever action somewhat suggestive of that of the itinerant scissor-grinder's apparatus. In other cases, propulsion was secured by a "backwards and forwards" motion of a lever handle. The seats of these machines were usually commodious—and correspondingly cumbersome—saddles for seating purposes not having then been adopted. On level roads a very fair rate of travelling could be obtained with velocipedes, but great physical exertion was required to drive them up hill, in consequence of their weight, which was nearly double that of machines of the present day. Indeed, it was found more prudent, and certainly less fatiguing, to dismount and push them up-hill, and even this was no mean task. And the vibration, too, was scarcely less diminished than in the case of the "dandy-horse." Long-distance records were not then, we need hardly say, often to be met with, twenty miles being considered a very respectable day's journey.

Eventually a new era in cycling was marked by the introduction of a bicycle (the forerunner of the perfect machines of to-day) which was considered at the time to be an extraordinary improvement, though now—being in possession of such marvels of completeness as were recently exhibited at the Stanley Show—we contemptuously speak of it as the "bone-shaker." It was much lighter in weight than its predecessors, and the saddle was placed in the "backbone," between the fore and hind wheels; these more nearly approached each other in size than those of recent manufacture. As we have said, the "bone-shaker" was considered to be a great success, and when the initial difficulty of maintaining their balance had been overcome by riders, the machine made rapid progress in favour. Long distances could be accomplished upon them with comparative ease; and bicycle races became of frequent occurrence. One of the earliest of these was witnessed by the writer of this article, at a charming West of England watering-place. The occasion was a public holiday, and the race formed a leading feature of the sports promoted for the amusement of the holiday visitors, who, with the townsfolk, mustered in large numbers, great curiosity having been aroused by the novelty of the contest. The course was through the principal thoroughfares of the town, and as lap after lap was finished, the excitement of the onlookers became intense, and culminated in irrepressible enthusiasm as the riders approached the winning-post suffering from evident exhaustion. Many of the competitors were obliged to withdraw from the contest long before the finish, the race being, by reason of the clumsy and weighty construction of the machines, much more a test of stamina than of speed.

The foregoing incident is related merely to show that even in years long past cycling was a subject of general interest to the public; and from the time of the advent of such competitions, it has been regarded as almost the principal recreation of the time. Year by year, some improvement has been introduced, the result of scientific study, inventive genius, and patient research. The great desideratum was a maximum of speed to be obtained with the least possible exertion in riding, and to accomplish this it was necessary largely to reduce the weight of the machines without imperilling the safety of the riders. The heavy wooden wheels with iron tyres were discarded, and replaced in 1867 by a new style of wheel—originally the invention of a Frenchman—lightly constructed of metal, and tyred with india-rubber in order to minimise the objectionable "bone-shaking." One of the first of these wheels manufactured is still to be seen at the works of the Coventry Machinists Company, where it has been preserved as a specimen of the first step towards the perfection of cycles.

It has only been during the past eight or ten years, how-

ever, that anything like the eager competition at present evinced by manufacturers has arisen; and this has been occasioned by the really scientific knowledge of motive-power displayed by some of the more enthusiastic devotees of "the road." Countless improvements have been effected, even in the smallest details, all of which tend to the comfort of the cyclist when riding. It would be invidious to particularise either of the many manufacturers who have lent their aid to the furtherance of this end; but we hope occasionally to be able to describe some of these inventions in the pages of KNOWLEDGE, that our many readers interested in the subject may be afforded the opportunity of judging for themselves which, for their own requirements, is the most suitable machine.

HYMN ON THE UNIVERSE.

I.

ROLL on, thou sun! for ever roll,
Thou giant, rushing through the heaven!
Creation's wonder; nature's soul;
Thy golden wheels by angels driven.
The planets die without thy blaze;
And cherubim, with stardrop wing,
Float in thy diamond-sparkling rays,
Thou brightest emblem of their King!

II.

Roll, lovely earth! and still roll on,
With ocean's azure beauty bound;
While one sweet star, the pearly moon,
Pursues thee through the blue profound;
And angels, with delighted eyes,
Behold thy tints of mount and stream,
From the high walls of Paradise,
Swift wheeling, like a glorious dream.

III.

Roll, planets! on your dazzling road,
For ever sweeping round the sun;
What eye beheld when first ye glowed?
What eye shall see your courses done?
Roll in your solemn majesty
Ye deathless splendours of the skies;
High altars from which angels see
The incense of creation rise.

IV.

Roll, comets! and ye million stars;
Ye that through boundless nature roam;
Ye monarchs on your flame-wing cars;
Tell us in what more glorious dome—
What orb to which your pomps are dim;
What kingdom but by angels trod—
Tell us where swells the eternal hymn
Around His throne, where dwells your God?

GOETHE.

[It is earnestly requested that the insertion of this poem be not regarded as an occasion for critical comments by those who, chancing to have no poesy in their souls, are apt to regard poetic fancies as matters for prosaic argument. The glowing thoughts of the poet must be "taken warm." —R. P.]

OCCULTATIONS IN A THREE-INCH TELESCOPE.

By a Fellow of the Royal Astronomical Society.

THERE are few more curious, instructive, nay, even startling, sights in the heavens than the Occultation of a fixed star, or (more rarely) of a planet by the Moon. When this occurs at the dark limb of our satellite, its suddenness is such as not infrequently to extort an exclamation from, as it invariably causes a start by, the observer who witnesses it for the first time. The Moon, as everybody knows, completes a sidereal revolution round the earth in about 27·32 days; in other words, that period elapses from the time of her quitting any given star to her return to it again. It may be worth while to mention incidentally that while the Moon has thus been travelling in an easterly direction through the sky, the Sun has also (apparently) been moving, much more slowly, in the same direction; so that if we assume that the Sun and Moon are in conjunction (the "New Moon" of the Almanacs), at the end of the 27·32 days the Moon will not have overtaken him; in fact, she will have to go on for 2·21 days before she comes up with him, and it is New Moon again. It is this period of 29 53 days which forms the Lunar or Synodical (Greek, Σύνοδος, a meeting) Month of the books on astronomy. In thus describing her monthly path over the celestial vault, it is quite obvious that she must pass between us and such stars as lie in her course; the stars being—for our present purpose—at an infinite distance, while she is only some 239,000 miles from us. Her orbit is, however, very far indeed from being a fixed circle in the sky. Its mean inclination to the Ecliptic is about 5° 9′; but its Nodes (the points where it cuts the Ecliptic, or plane of the earth's orbit) are perpetually shifting. The Moon's perigee, or nearest point to the earth, is shifting, and, in fact, to put it shortly, at the end of any month the Moon does not return accurately to that point of the sky from which she set out. Were the path of the Moon a definite and unalterable one in the heavens, she would, of course, occult the same stars over and over again, month after month. As a matter of fact, she only does this, and that but approximately, after 223 lunations—a period known to the Chaldæans of old as the Saros. Very well, then, travelling thus as we have said from west to east, her eastern limb is, of course, the leading one, or that which covers, hides, or occults the objects lying in her path. From New Moon to Full Moon this limb is unilluminated, and the effect of the extremely sudden extinction of a star when the dark limb hides it is, as we began by saying, of an absolutely startling character. "In a moment, in the twinkling of an eye," the star which shone as a brilliant point in the sky is blotted out; and its place seemingly knows it no more until it reappears from behind the opposite or illuminated edge of the Moon. After Full Moon, of course, the eastern limb is illuminated, so that the disappearance takes place at the bright edge, and the star on its reappearance starts instantaneously from behind the dark limb. A few days on either side of New Moon, when the dark limb is visible by earthshine—or, in the popular form of expression, we can see the Old Moon in the New Moon's arms—a new charm is added to the spectacle of an Occultation, inasmuch as before Full Moon the faintly-lighted dark limb can be actually seen approaching the star which it is soon to obliterate. Now it fortunately happens that Occultations are phenomena peculiarly within the range and capability of a three-inch telescope. Moreover, should the owner of such an instrument also happen to possess

a trustworthy chronometer or regulator, he may not only derive great personal pleasure and amusement from the observation of the phenomena of which we are treating, but may render real and enduring service to science by the publication of his observations of the Occultations which are predicted in the *Nautical Almanac*. It may encourage the student and young observer to be assured that observations of Occultations made with a three-inch telescope and an accurate chronometer, may be of real service in correcting the lunar tables, and the theory generally. As an illustration of the preceding remarks, we will take the Occultation of λ Geminorum by the Moon, which happened on Thursday, March 6. Our sketch represents the aspect of affairs at 10h. 10m. 4s. p.m., at the instant of the star's disappearance at the Moon's dark limb, as seen in a three-inch telescope armed with a Huyghenian (inverting) eye-piece magnifying 80 diameters.

Occultation of λ Geminorum by the Moon, March 6, 1884, power 80.

The reappearance happened at a point in the bright limb which may be found in the engraving by opening the legs of a pair of compasses 0·55 in., and placing one leg on the lowest illuminated point of the Moon's disc; then will the other leg cut the bright limb at the spot at which it took place. It occurred about 11h. 12m. 9s. p.m. Let us now turn to p. 435 of the *Nautical Almanac* for the current year, and see in what form this phenomenon is there predicted, with a view to explaining and utilising such prediction for the information and instruction of the student.

Occultations Visible at Greenwich.

Month and Day.	Star's Name.	Magnitude.	Disappearance.				Reappearance.			
			Sidereal Time.	Mean Time.	Angle from N Point.	Angle from Vertex.	Sidereal Time.	Mean Time.	Angle from N Point.	Angle from Vertex.
			h.m.	h.m.	°	°	h.m.	h.m.	°	°
Mar. 6	λ Geminorum	3½	9 10	10 10	107	134	10 11	11 11	226	262

*** The angles are reckoned towards the right hand round the circumference of the Moon's image, as seen in an inverting telescope.

The first column gives the date; the second, the star's name; the third, its magnitude; the fourth, the sidereal time—or that shown by an ordinary observatory clock—at which the disappearance takes place; and the fifth, the corresponding instant of mean solar time at which the star vanishes. The sixth and seventh columns require more detailed explanation. The "North Point" of the table is that point of the Moon's limb cut by a circle passing through the North Pole and the Moon's centre. It is in real truth the *South* point of the Lunar limb; but it is uppermost in the field of view and is known technically as the N. Point. From this, angles are measured right round the Moon's circumference, in the direction in which the hands of a watch move. Our second figure will illustrate this.

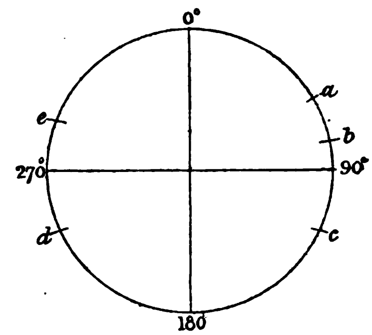
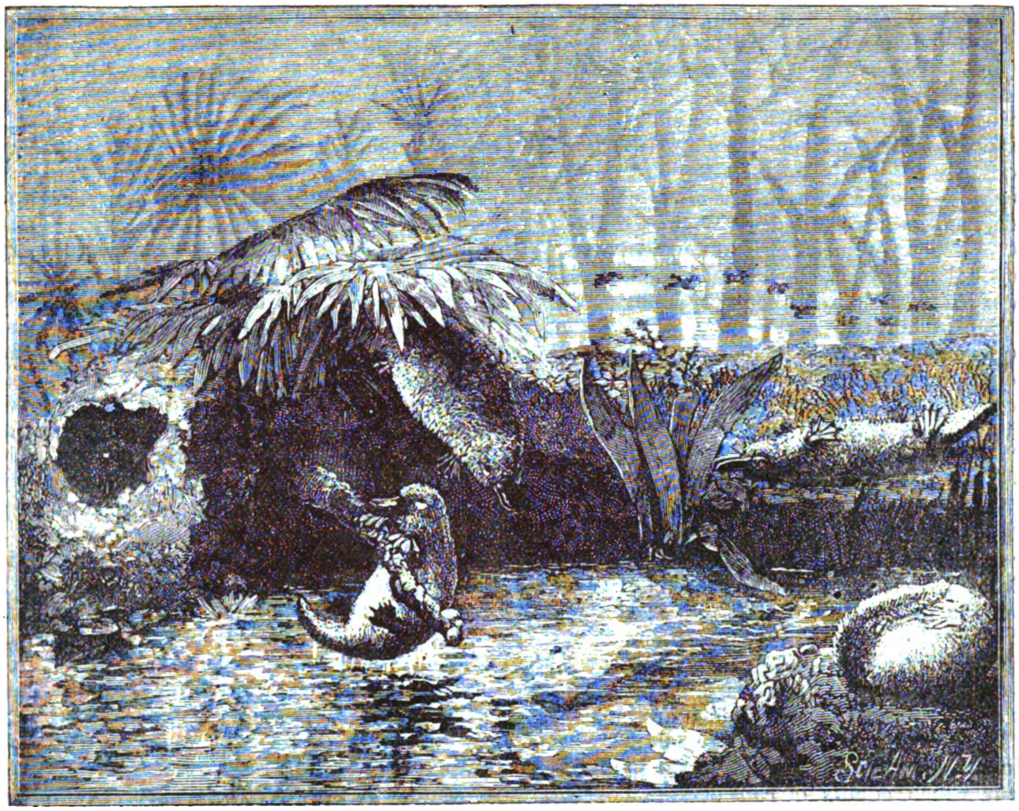


Fig. 2.

This will render the mode of measurement intelligible at a glance. Here, evidently, 0° is the Moon's (technical) N Point, and the measurement of angles from it is indicated by the figures. Suppose, for example, that we found from the tables that the angle from N Point of some star at disappearance was 60°; then should we know that it would be occulted at the point marked *a* in our diagram. Were the angle 79°; it would disappear at *b*, 116° at *c*, and so on. Were, on the other hand, the angle of reappearance given as 245° from N Point, *d* would be the point in the limb from which it would emerge, as would *e* were such angle 292°. These "angles from North Point" are employed with telescopes mounted equatorially (Vol. I. p. 201). When the observer has only an altazimuth mounting (*loc. cit.*), as is usually the case with a three-inch telescope, the angles must be measured from the Moon's vertex. This in effect is the point in her limb at the top (in an inverting telescope) cut by a plumb-line passing through her centre. The method of deducing the angle from the vertex, that from the North Point being given, will be found (in Letter 277) on p. 210 of Vol. III. of Knowledge. We have so far spoken as though the disappearance of stars was in all cases instantaneous, and so, as far as our own experience goes, it is. Other observers, though, have seen the very curious phenomenon of the apparent projection of the star on to the (almost invariably bright) limb of the Moon. It is a noteworthy fact that this curious appearance has been practically confined to red stars, like Aldebaran; but this goes a very little way in helping towards a solution of so anomalous an effect. The Occultations of planets are comparatively rare phenomena; and should be sedulously watched whenever they occur. An Occultation of Venus occurred about 3 o'clock in the afternoon on February 29; but no intimation or prediction of it whatever was given in the *Nautical Almanac!* Occultations of Saturn and Jupiter afford delightful spectacles to the observer; the extreme sharpness of their superficial detail, where actually in contact with the Moon's limb, entirely negativing any suspicion of a lunar atmosphere. Irrespectively altogether, however, of the mere beauty and interest of the phenomena of Lunar Occultations, and their entire suitability for observation with our instrumental means, we would, in conclusion, once more insist on their scientific value. Made simultaneously at stations, the longitude of one of which is well determined, they afford excellent (if somewhat operose) means of deducing that of the other. Moreover, as we have before said, if the student be the possessor of a chronometer indicating accurate Greenwich Mean Time, he may by his own unaided exertions render real help towards the improvement of the Lunar Theory, and perchance earn a niche in the Temple of Fame in days yet to come.



THE ORNITHORHYNCHUS.

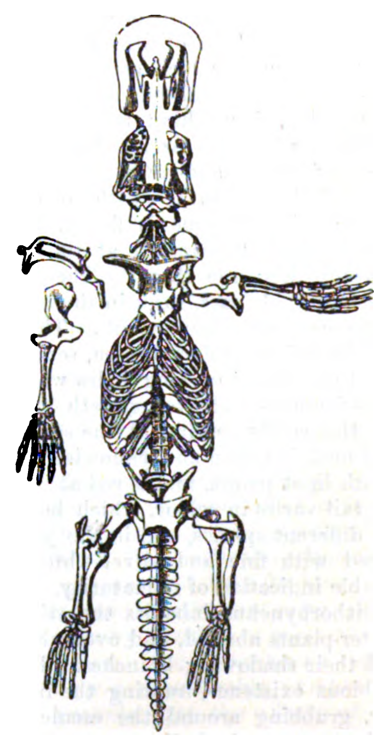
(*Ornithorhynchus paradoxus.*)

By L. P. Gratacap.

THIS interesting animal has proved both a perplexity and a delight to naturalists. Its little body is so curiously constructed as to remind the student of structural affinities in animals of three types of life—the mammals, birds, and reptiles. It undoubtedly belongs to the former, but it enters that class at its lowest point, and brings along with it features and reminiscences of more degraded forms than itself. It is a welcome gift to the evolutionist, and he has not been loth to emphasise every indication it gives of its intermediate and connective character. In spite of these suggestive resemblances the ornithorhynchus is essentially *mammalian*, though holding the humblest position in this group. With its singular ally, the *Echidna*—the porcupine—anteater—it forms the division of *Ornithodelphia*, and is especially characterised by a strange provision in its economy, by which the fæces and young are extruded through the same passage, as the spacious cloaca is common to the rectum, genital, and urinary organs. Hence, the technical appellation of *Monotemata*.

The features which ally it to the amphibia or reptiles are chiefly found in the skeleton, and are the following, among others less obvious:—A projection of the second neck vertebra, called the "odontoid process," remains for a long time disconnected from the vertebra itself, upon which it is normally soldered by a long growth between the surfaces. Some of the cervical ribs in a similar way remain free.

The coracoid bone, which in man is a process only of the scapula, or shoulder-blade, but in birds is a separate bone, as also in reptiles, and which is a large bone in this animal, articulates with the sternum or breast-bone directly. This is a positive amphibian and avian feature. There is an ossification in front of this bone, called the *epicoracoid*, which resembles a similar portion of the reptilian frame. In these



Skeleton of the Ornithorhynchus.

mammals alone there is a T-shaped bone supporting the shoulder-blades or clavicles. The *acetabulum*, or cavity, into which the head of the femur is thrust as in a socket,

remains unossified at its centre, thus resembling birds and crocodiles. Other points in its anatomy and physiology strongly suggest its indeterminate and dependent character, but its nature and functions place it beyond appeal among the mammals as a class.

The ornithorhynchus, by its grotesque union of the externals of a duck, beaver, and mole, its restricted range geographically, and the singular and unwarranted tales told of its habits by natives of Australia, has always formed a natural curiosity, and been regarded with mingled feelings of amusement and astonishment. The first skin of this animal sent to England presented such anomalous features that it was regarded as the playful hoax of some ingenious collector. A duck's bill and a mole's body presented a zoological complication which at first could not be considered seriously.

The ornithorhynchus is about the size of its congener the echidna, having an average length of 50 cm. (1 ft 7·6 in.), 12 of which measure the normal length of its tail. The males are larger than the females. The flattened body is not dissimilar in some aspects to that of the beaver or fish otter. The bones are short, each foot or paw is provided with five claws, which are webbed, and this integument on the front feet is developed to such a degree as to extend beyond the extremities of the claws; it folds or draws back at the will of the creature, permitting it to use its serviceable talons or nails for digging and excavating. The short hinder feet are turned backward, and are usually placed in that position, and the nails, which are longer and sharper than those of the front feet, are similarly bent backward. In the males, above the toes of the hinder feet, there is a spur which admits of considerable movement. The tail is flat, broad, abruptly terminated, and in the younger specimens provided plentifully with hair, which disappears with age.

The head is quite flat, and forms the most distinguishing feature in its appearance. It is small and furnished with a duck bill, at the base of which a leathery apron-like expansion is developed, which acts as a shield, protecting the eyes when the animal burrows in the ground, and guarding the fine fur behind it from the slime of the muddy bottoms where it searches for its food. The jaws are prolonged forward, and carry no teeth; the margins of the duck-like bill are sheathed with horn and crossed with horny plates. The tongue is fleshy, armed with horny caruncnlations, and terminated at its base by a ball-like swelling which closes the throat. The eyes are small, and the barely noticeable ears, sunk in the head near the outer angle of the eyes, are closed at will. The fur on the upper surface of the animal is dark brown, sometimes reddish; it is composed of one set of long hairs which are somewhat stiff, and of another shorter growth of fine grey hairs, similar to the woolly coating of the seal. The fur on the breast and neck is silken and yellowish. The bill is black, spotted with light points, and is red at its extremity. The fur of the tail varies in colour, which has given rise to suspicions of different species, and in the younger individuals it is coated with fine and silver-white hairs, an almost unmistakable indication of immaturity.

The ornithorhynchus inhabits the still pools of streams where water-plants abound, and over whose serene expanse trees bend their shadowing branches. Here in pursuit of its amphibious existence, hunting the insects which haunt the water, grubbing around the esculent roots of plants, building its home, and eluding pursuit when the natives, who regard it as a delectable morsel, watch patiently for its appearance, spear in hand, upon the banks of the pond. The traveller who is fortunate enough to surprise these animals when actively engaged in their pursuit of food, must remain preternaturally still, if he wishes to enjoy the novel spectacle. If the water is clear and the light favourable, he will see them moving rapidly beneath the water, avidly inspecting the soft banks for beetles; they will rise to the surface every two or more minutes, again disappear to emerge later at a distant point. The slightest movement or noise is instantly detected, and the shy, strange animal is put to flight, and the chagrined spectator must endure a prolonged watch before it reappears.

The nest of the ornithorhynchus is located under ground, and is placed at the end of a long, underground, devious, passageway, which may be, in exceptional cases, 45 feet long, although more usually 10 feet. This avenue of approach is strewn with dead leaves, as is also the kettle-like hole in which it ends. The homes of the ornithorhynchus are entered by two passages, one above and the other below the surface of the water. Almost invariably the nest is placed beyond danger of the infiltrating water of the highest tides.

At rest the animal assumes various positions; the two most familiar are shown in the accompanying illustration. It rolls itself up in a ball, with its fore feet tucked under its bill, its hind feet pressed tightly over it, and its tail drawn down over all, or else its lies on its back, with its four feet stretched upward in languid delight.

The natives aver that the female lays eggs, and that the male inflicts poisonous wounds with its spur, both of which stories, formerly received with credulity, have been abundantly disproved.—*Scientific American.*

RIVER ACTION ON LAND.*

AMONG the many important and complicated questions with which the practical hydraulic engineer has to deal, there are few more important and few perhaps more complicated than that of the discharge of water through open channels. Naturally, therefore, it has engaged the attention of engineers from an early period, and the formulæ laid down on the subject by Dubuat, Eytelwein, and others date from centuries ago. Their experiments, however, were mostly conducted on channels of very small section; and it has long been recognised that to extend these to natural rivers, or even to channels of different dimensions, is to commit a grave error. Numerous formulæ have since been introduced in the hopes of improving on the results of these early experimenters. Turning, for instance, to the well-known pages of Molesworth's "Pocket-book," we find that the mere list of such formulæ occupies almost a page of small print, and that at least twenty authorities are there mentioned by name as having contributed to the subject. But as a striking commentary on their success a small table is given at the bottom of the same page, where four cases of actual discharge, varying from 24 to over 1,000,000 cubic feet per second, are compared with the calculated results for the same conditions, derived from nine approved rules on the subject. The differences are very striking. In some cases they amount to nearly 100 per cent., and in many cases to at least 50 per cent. Nor is this a solitary instance. Major Cunningham, in his recent work on the Roorkee hydraulic experiments, arrives at very much the same result. Again, in papers published in our issue of Nov. 1, 1872, p. 293, the results given in the classical experiments of D'Arcy and Bazin are compared with the calculated results derived from several of these same formulæ, the object being to test the value

* From the *Engineer.*

of the formulæ obtained from theoretical considerations by the late Canon Moseley. Any one who will turn back to these figures will see divergences startling enough to prove that we must at least use considerable caution in adopting any formula for such purposes, and yet the conditions were here unusually favourable, as the experiments were carried on, not in the difficult and irregular channel of an actual river, but in an artificial conduit specially prepared for the purpose. In spite of these facts, which it is impossible to deny, nothing is more common that to see the most confident and offhand statements made as to the discharge of all sorts of rivers under all sorts of circumstances. To take one example, derived from the science of geology. It is frequently stated in geological works—as, for instance, by Mr. Alfred Wallace, and by Professor Geikie in his late admirable "Manual of Geology"—that a certain definite thickness of earth is removed from the surface of the land every year by the rivers which flow through it, and is by them emptied into the sea. The actual thickness in thousandths of an inch per annum, is given with all the confidence of an approved scientific fact. We have never been able to trace this statement to its original source; but we have no hesitation, as practical engineers, in pronouncing it to be altogether visionary. It is worth while to consider for a few moments what data would be requisite before such a figure could be laid down, even to the very roughest degree of approximation.

Of course, the only possible method of determining it lies in measuring, by some mode or other, the quantity of mud—or silt, to use the most general term—which all the rivers, say, of this country, carry into the sea during the course of a single year. What would any engineer do if he was given the commission of carrying out such an inquiry? He would have, in the first instance, to ascertain the mean annual discharge of every river or stream emptying itself into the tidal estuaries of Great Britain. It would be worse than useless to conduct observations upon these estuaries themselves, because it is perfectly well-known that the silt suspended in such waters at any moment gives no evidence whatever as to the amount of silt which is conveyed into these estuaries from the uplands during each year or day. Therefore, it would be vain, for instance, to compute the silt carried down by the Thames from observations taken below Teddington Weir; we must examine the Thames itself above that weir, and all the streams which enter into it lower down must be subjected to a separate investigation. Let us consider next what will be necessary in the case of any one of those streams.

In the first place, our engineer must have the means of measuring its discharge with sufficient accuracy on any particular occasion he may desire. We have said enough already to show that this is an exceedingly difficult matter, that he will be unwise if he relies even upon the best of the dozen or so of formulæ amongst which he may take his choice; and that he is in duty bound to make an accurate determination in the case of each river, in order to take full account of local conditions. Let us assume that he has done so, and that he is thus able by a series of observations to ascertain with fair correctness the discharge taking place on any particular day. It is obvious that to measure this discharge for a single day only would be utterly futile. During a winter flood many rivers, even in England, will send down 50 to 100 times the water in an hour, compared to that which dribbles over their bottom towards the end of a summer drought. Our engineer must, therefore, measure all the great floods which occur during the year he has selected, and must also take a large number of measurements both in the wet and dry seasons. This done, he must search carefully the meteorological records of the district—if he can find any—in order to ascertain whether this particular year may be taken as a fair average example, and if not, he must make such addition to, or subtraction from, his results, as his own judgment shall direct him. Failing this, he will have no resource but to renew his observations from year to year, until, in the lapse, say, of a generation, a true average can be struck. At the end of this time, provided there are no indications of a progressive change in the climate and rainfall, he may fairly be allowed to state what the mean annual discharge of this particular stream may be.

But his work is only begun after all; it is not the quantity of water discharged which he is in search of, but the quantity of silt. He has to determine not only the number of cubic feet of water which have flowed through his channel in a particular year, or a particular half-century, but how much solid matter each of these cubic feet held in suspension while it passed. To do this, it will by no means suffice to pick up a bucketful every time that he makes an observation, have it carefully evaporated, and weigh the residue which remains. We know scarcely anything of the laws of distribution of suspended matter within the waters of a stream. It may, indeed, be assumed that the quantity per cubic foot will be larger towards the bottom than towards the top of the current; but the law according to which this varies is quite unknown. All that we can be sure of is that this law itself will vary, and probably very largely, with the depth of the current, with its velocity, with the contour of the bottom, with the material of which that bottom is composed, and probably with many other local circumstances. It will not do, therefore, to trust to anything less than the collection of a large number of samples, say 50 to 100, from all parts of the cross-section, on each occasion when the discharge is measured, or, at any rate, on each occasion when the river is in any abnormal condition. Suppose this to be done, and the weight of silt per cubic foot at each of those places to be ascertained by the slow method of evaporation and weighing, then it will by no means do to strike an average of all the fifty, and multiply this by the number of cubic feet discharged per hour; for the velocities at different parts of the cross-section are very different, and this will clearly modify the results. Thus, if the velocity at the bottom is half that at the top, whilst the weight of silt per cubic foot is double, it will be seen that the amount of silt carried down per square foot of area at the bottom and at the surface will be really the same. Hence, we must know the average velocity in each of the fifty divisions, say, into which our cross-section has been partitioned off for convenience, and we must also know the average weight of silt per cubic foot corresponding to that division. Multiplying together each pair in these two series of numbers, and adding the products thus obtained, we shall arrive at some sort of approximation towards the quantity of silt which our stream was carrying down per hour, on the day when this particular observation was taken, and the same process will have to be repeated for every one of the separate observations which have been described as being necessary in order to solve the problem of mean annual discharge. The experiments must necessarily be conducted over a considerable number of years, because it is quite possible that the effects of drainage, denudation, or other causes, may produce a progressive increase or diminution of the average quantity of silt borne down, and of this it will be necessary to take account.

So much for our single river; we have now to perform

the same task for every other river and stream, from the largest to the smallest, which empties into a tideway round the coast of our island. The smaller streams will, no doubt, give less trouble than the larger, but they must by no means be neglected. We have no means at hand of estimating the number of such streams, but assuming that there is merely one for each mile of coast, it will be evident that it will be considerably over 1,000. Supposing, however, that the whole of these have been gauged, and the discharge of silt calculated in the manner just described, we may perhaps imagine that our engineer's task is at an end. Not a bit of it. He has to ascertain not merely how much solid matter has been carried into the sea per annum, but how thick a layer of solid matter has been subtracted from the land. Now the quantity of the solid matter upon the land is being added to every year by the operation of certain very obvious causes, such as the falling of leaves, the decay of plants and animals, the application of foreign manures, &c. Possibly some one may object that organisms, whether plants or animals, can only build up their substance from materials already existing in the earth; but a moment's reflection will show that a large part of their substance is derived either from water or from air. It is precisely this decaying organic matter, lying, as it does, at or near the surface of the ground, which will be washed off in the greatest proportion by rain and by rills, and will so find its way into the rivers and thence to the sea. Therefore, our engineer must of necessity do one of two things: he must either analyse carefully every ounce of silt recovered in his observations in order to ascertain beyond a doubt, first, how much of it is due to organic and how much to inorganic matter, secondly, how much organic matter was derived from the earth, and how much from air and water; or, failing this, he must by some means or other calculate the whole volume of matter which has been added to the earth by the causes above mentioned, he must measure the quantity which remains at the end of that period, and he must subtract the difference—or rather the difference less that part of it which is due primarily to inorganic constituents—from the total amount which he has already ascertained to form the burden of the rivers as they fall into the sea.

We have, perhaps, said enough to show, however faintly and inadequately, the nature of the task which an engineer would have before him if he were set to ascertain the correctness of the figure which geologists quote so confidently, viz., the thickness of the layer of soil which is removed annually from our British Isles by the operation of what is called sub-aerial waste. A feeling of longing and regret steals over us as we close the record. What a pity that the determination of this figure is not a matter of paramount national importance, to be settled at any cost! and what a pity that we ourseves are not given the responsible task of settling it! It would resemble one of those magnificent Chancery suits which an attorney of the old school was wont to regard with so much complacency and satisfaction; a suit which he could slowly administer during his life-time, and hand on to his children with his blessing on his death-bed, certain that it would remain as a sure and comfortable source of income to them and their children yet unborn. And yet geologists quote this figure with perfect confidence, as if it was known to the 10,000th of an inch. They do more—they calculate on this basis the number of years which will elapse before Great Britain becomes a dead level, totally forgetting that the diminution of slope all over the country will wholly change the conditions of the problem. They do this in the name of science, and in the next breath inform us that science is measurement!

Editorial Gossip.

Some of the newspaper accounts of the boat-race were scarcely less absurd than the marvellous accounts of the practising. One paper finds that from Barnes Bridge Oxford had two "passengers" at least,—that is, men not pulling their weight, and stroke shirking by not covering his blade. Cambridge, by the same accounts, rowed this part of the distance magnificently. Rather an astounding feat for five Oxford men and a work-shirking stroke to row a boat with two "passengers" from Barnes Bridge to the "Ship" behind a splendidly-rowed eight, without losing more than half a length, if so much! I was at the "White Hart," Mortlake, and had the Oxford boat under my eye for some two hundred yards, and I venture to say that the whole distance was most honestly and pluckily rowed by every man in the boat. Not a man shirked the tenth part of a stroke.

All know that the Christian Easter is an adaptation, with altered symbolisations, from the Jewish Passover. It is nearly as certain that the Jewish Passover was an adaptation with changed meaning from an Egyptian festival. Even for the Sabbath Moses found a meaning connected with the departure from Egypt (Deuteronomy v., 15), and the Jewish observance of the Sabbath is known to be a relic of Egyptian ceremonial. But about the feasts of the Passover and of Tabernacles, there can be no more doubt than about the sunrise and sunset observances, and the new moon festivals, that they came originally from a race worshipping the heavenly bodies, as every race of man has worshipped those impressive orbs in one part or another of its progress from savagery to civilisation. The great Sun-God was for six months below the equator, for as many above that great circle of the heavens; Day was for half the year weaker than Night, for half the year stronger; the Sun-God Herakles or Samson was for six months triumphant, for six months held in bondage and shorn of the rays which are his might. That the time when he descends below the equator should still be observed as a time of lamentation by the Jews, though they are no longer sun-worshippers, is less wonderful, considering the conservatism of the human race, than that Christians should somehow have gotten rid of that particular fast which the Jews were enjoined to observe with mourning,—so strongly enjoined that whosoever did not afflict himself was to be slain. The other festival held of old in honour of the triumphant ascent of the Sun-God from the dark depths below the equator, the Jews retained as symbolising a passover of a different sort, while it has come down to our time and to Christian nations still further modified in form,—though still typifying triumphant return from darkness and death. Strangely enough the name we have for the festival (which has the same relation to rising that the word East has) came to us more directly, being the Saxon name for the festival of the Sun-God's return to the ascendant part of his annual career.

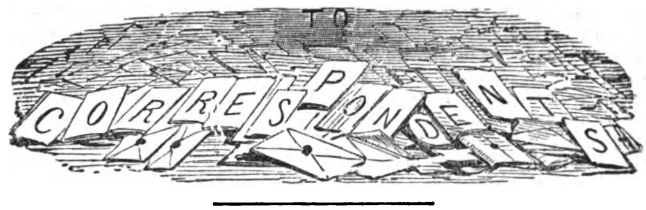
Reade, Trollope, Dickens, Thackeray,—within a score of years, these four have passed from among those for whom they afforded so much pure pleasure, among whom they wrought so much good. Looking around at the world as we find it, considering its sorrows and its joys and what share each one of us has or may have in contributing to one or the other, where shall we find four others of those who speak our tongue who have done more for good within the last half-century than these four, who lived in our midst untitled and unrewarded save by the direct product of their

very own labours? Truly when we consider the actual fruits of the actions of men, how paltry, where they are not absolutely mischievous, seem the doings of many who are regarded as the great people of the earth. Taking the balance of all the deeds of kings and potentates, warriors and statesmen, within the last two thousand years, on the one hand duly counting such good results as they have achieved but on the other estimating aright the multitudinous miseries they have wrought, it is not too much to say that the least of the four named above, the least among discoverers and inventors, the least among poets, historians, philosophers, and artists, has done work which outweighs all the deeds of war and policy which men in their weakness delight to applaud.

The death of such men as Charles Reade, even though he had reached the three-score years and ten allotted to man, and had done the best of his work ten years since, is a real loss to the world. He will not be buried with the pomp and ceremony which pretends to be national mourning, but he will be mourned and lamented by hundreds of thousands who personally never knew him,—and for the just reason that he did good which could be felt and measured. No compliments of mourning are needed and none need be uttered when such men die. No national tokens of grief need be ordered, and none need ape an unfelt sorrow. We need hang no heraldic blazonry over the tomb of the worthy dead, no tokens of qualities with which he was not gifted or of deeds which he never wrought. None need see the dignity of human nature degraded by false shows or pretended sorrow. The name of the man and the memory of what the man did, suffice. As he said himself of a great man dead, whose deeds and words live (centuries after his contemporaries the bluff and brutal Henry, the splendid and shallow Francis, the grand and grasping Charles, ended their conspicuous but worthless careers in their native dust and ashes),—"the words of genius are not born to die; their immediate work upon mankind fulfilled they may seem to lie torpid; but at each fresh shower of intelligence Time pours upon their students they prove their immortal race; they revive, they spring from the dust of great libraries; they bud, they flower, they fruit, they seed, from generation to generation and from age to age."

Eastward Ho! is the title of a new sixpenny monthly magazine, of which the first number is to appear on April 25. It will form a medium for conveying to the upper classes the views and suggestions upon social questions of those interested in the progress and well-being of the people. Among its earliest contributors will be the Bishop of Bedford, G. Manville Fenn, G. R. Sims, Rev. Brooke Lambert, W. G. Wills, &c. It will be published by Henry G. Davies, 73, Ludgate-hill, E.C.

The report on the mineral statistics of the United Kingdom for 1882, prepared by the Inspectors of Mines, shows that during 1882 156,499,977 tons of coal were raised, of the value, at the mine, of £44,118,409; 226 oz. of gold, realising £863 at the average market price; and 372,544 oz. of silver, of the value of £80,426. The quantity of iron ore raised was 18,031,957 tons, representing a value of £5,779,285 at the mine, and of metal contained in this ore 6,513,281 tons, of the value of £18,237,186. The total value of the minerals raised in 1882 was £54,879,507, and of the metal contained in the ores, £20,558,050.



"Let Knowledge grow from more to more."—Alfred Tennyson.

Only a small proportion of Letters received can possibly be inserted. Correspondents must not be offended, therefore, should their letters not appear.

All Editorial communications should be addressed to the Editor of Knowledge; *all Business communications to the* Publishers, *at the Office, 74, Great Queen-street, W.C.* If this is not attended to delays arise for which the Editor is not responsible.

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The Editor is not responsible for the opinions of correspondents.

No communications are answered by post, even though stamped and directed envelope be enclosed.

FRUIT TREES IN IRELAND.

[1191]—"A Resident Irishman" puts forth in No. 1186 a curious theory to account for the absence of fruit trees in the small farms of Ireland. He says that the *vera causa* is the "ungenial climate."

If so, the climate must be curiously variable, for on the very same estates it happens generally that fruit trees flourish in the "demesne" (*i.e.*, the grounds surrounding the house) of the landlord, but not on the holdings of his tenants. It also happens that fruit trees grow well enough on the tenant farms of exceptional landlords, such as the Duke of Devonshire, Earl Fitzwilliam, and others, whose natural sense of justice anticipated those provisions of the Land Act which, I expect, will transform the physical as well as the moral aspect of the most wretched parts of Ireland by enabling the worker to reap the fruits of his own industry.

Everybody, resident or non-resident, who knows anything at all concerning the climate of Ireland, is aware that it is more suitable for the growth of ordinary fruit trees, than that of either England, Wales, or Scotland; that all the Western side, from Cape Clear to Malin Head, has a climate which for geniality and equability has no rival in corresponding latitudes in any part of the world. It is similar to that of South Devon, Cornwall, and the Channel Islands, from which we obtain so much of our best and earliest fruit supplies. It is a climate tempered throughout the year by the unfettered floes of the Gulf Stream, which washes the whole of the Western coast.

Every botanist knows that plants are growing wild and luxuriantly on the hill slopes of this coast, which perish when transplanted to any part of England. One example will illustrate this. When I was on a visit to my friend, Professor Allman, of Galway, at his charming summer retreat in Roundstone Bay, Connemara, we strolled on the hills and gathered specimens of the "Connemara heath" (*Erica cantabrica* of Linnæus, now named *dabecia polifolia*, as it is not a true heath, though much like one). This is found nowhere else north of Spain, and every attempt to acclimatise it in England has failed. I planted some roots in a warm corner at Twickenham, but the first Twickenham winter killed them.

I shall never forget the avenue of fuchsias at Letterpack, the beautiful trees arching over the road like the nave of a Gothic cathedral, their crimson blossoms tinging the sunlight that glinted between them. In the grounds of Kylemore Castle, hard by, is one of the finest collections of exotic shrubs to be seen anywhere in Britain. Some of the Californian firs render the assertions of the "Resident Irishman" concerning the ungenial climate absolutely ridiculous. When I was there in 1877, and visited Mr. Mitchell Henry at the Castle, he told me that, in consequence of the remarkable mildness of the climate, he was able to make hedges of crimson fuchsias, which in four years grew thick enough to resist sheep. I saw these hedges in the course of growth, taking the place of the posts and wires, gorse hedges, and stone fences that had preceded them. Slips were then being rudely thrust into the ground, no further trouble being demanded for their propagation. I have heard other resident Irishmen assert that the general barrenness of the wilds of Connemara, which, if planted, would be a region of unrivalled scenic beauty, is due to climate. Mr. Mitchell Henry (an Englishman) has demonstrated the monstrous absurdity of this Irish notion by the luxuriant foliage with which he has surrounded his noble residence, and he has proved that the bogs, like the treeless wastes, are merely the results of shameful human

negligence; the negligence of landlords who will neither reclaim them with their own capital as he has done, nor give to the tenants that permanency of tenure that would induce them to do so.

The "Resident Irishman" jumps to a very false conclusion in supposing that I join in the denunciations of the Saxon. My explorations of Ireland, extending to every county in the island, have satisfied me that the Saxons have been, are, and will be yet, its true benefactors in developing the splendid natural resources of that shamefully neglected country.

He speaks of the provisions of the Act of 1870, but must surely know, as everybody else does, why they have been a dead letter. They provided that the poor tenant either should obtain the produce of his labour in planting trees, or otherwise improving his holding by going to law against his landlord, but provided no other machinery than the ordinary court for that purpose. My readers may imagine the case of a poor, potato-fed, ragged cottier, registering each of his gooseberry-bushes, and then going to law for their value against a territorial magnate who had a final appeal to the British House of Landlords. The resident Irishman must know how the Earl of Leitrim publicly announced his intention of defying and violating the law of 1870, and how he proved his feudal power over his tenants by commanding them to publicly and ostentatiously insult the Lord Lieutenant, and through him the Queen and the British Government; how he Boycotted (before Boycott) the Lord Lieutenant and his suite at Maume, in spite of legality, and how he drove them out of his dominions without bit or sup for horse or man. This was done to demonstrate his contempt for that same law which, according to the "Resident Irishman," any one of his poor tenants could enforce against him.

W. Mattieu Williams.

INFINITY.

[1192]—On reading the letter 1160 in your paper signed "H.," I am reminded of Dr. Büchner's remarkable chapter on Infinity of Matter either in greatness or smallness, extent in space, and divisibility. Büchner met, in Germany, for his now famous work on "Force and Matter," with quite as much hostile criticism as Mr. Herbert Spencer and other great thinkers have met with in this country. As usual, the teachers of the religion of "love" and "good will towards all men," are to the fore in their demonstration. How different is their teaching from their practice!

His words on atoms are (p. 97 "English Translations"):—"We have no real notion of the thing we term atom; we know nothing of its size, form, composition, &c. No one has seen it. The speculative philosophers deny its existence; as they do not admit that a thing can exist which is no longer divisible. Thus neither observation or thought lead us, in regard to the minuteness of matter, to a point where we can stop; nor have we any hope that we shall ever reach that point."

He further says (p. 97):—"We can, then, only say that matter and the world are infinite in minuteness; and it is of little consequence if our intellect, which is always accustomed to find a limit, is offended at the idea."

Büchner's "Force and Matter" (p. 97):—"If, then, we can find no limit to minuteness, and are still less able to reach it in respect to magnitude, we must declare matter to be infinite in either direction, and incapable of limitation in time and space. If the laws of thought demonstrate an infinite divisibility of matter, and if it be further impossible to imagine a limited space or a nothing, it must be admitted that there is here a remarkable concordance of logical laws with the results of our scientific investigations." F. W. H.

SOLAR SPOTS.

[1193]—I have read your paper in *Longman's* with great interest, because it is something like certain thoughts I have entertained respecting the origin of the solar spots. But I was led to abandon the idea of eruptive action from the sun's real globe because of the long duration of many that have been seen. You state there is about four days' difference in the rotation of spots situated near the sun's equator and those nearest the poles. I apprehend this to be fatal to a theory attributing them to eruptions from a globe so distant as upwards of 100,000 miles, and very probably rotating at a greater speed than its outer envelope (a solid globe cannot rotate at both rates). Still, I must imagine them due to eruptive action from below, although that action is still far above the sun's solid globe.

The density and pressure of the solar atmosphere intervening between the photosphere and the globe must be something enormous. If of the nature of gas, it must be very different in composition to any known to us, and therefore speculation concerning it would lead to no certain result.

What about the *waste* constantly streaming (or ascending) from the surface of the globe into its atmosphere?

Thomas Ayres.

[The greater part of such matter would return, and more matter would arrive from without than would be parted with. As for the varying rotation rates, they affect the cloud-laden region only. A spot would neither be originally, nor remain, vertically above the seat of disturbance.—R. P.]

GHOSTS AND GOBLINS.

[1194]—Although a disbeliever in everything supernatural, you will, perhaps, allow me to relate an incident which came within my experience, and which has some bearing on the interesting series of articles on "Ghosts and Goblins," appearing in Knowledge. Thirteen years ago I was a pupil in a Highland parish school in Inverness-shire. The school was not more than a hundred yards from a burying-ground, and two old maids resided in a thatched house quite near to the school-house. Both were regarded as model Christians in the district, and, as a consequence, were highly esteemed. Sometimes they visited the houses in the district, in the capacity of catechists, and took great pleasure in indoctrinating the boys and girls in the theology contained in the Westminster Confession of Faith. I recollect on one occasion, when one of them called at my father's house, being deputed to see her home. The night was pitch dark, and we had great difficulty in keeping the road. About half way I was walking along the centre of the road and she at the side. She all of a sudden rushed over to me and took a hold of my arm, and pulled me to the side, remarking, "Come out of the way, a funeral procession is passing." I replied that I could not see it, but she solemnly assured me that while I was speaking she was seeing the coffin carried shoulder high, and a number of men, many of whom she mentioned by name. "You will see," she said, "within three days a funeral procession pass along this way to the churchyard." I took a note of the names that she mentioned, with particulars of the dress they wore, and determined to test the accuracy or falsity of her story. The day after she had witnessed the procession of phantoms a boy was drowned while attempting to cross the river Dulvain, and his remains were interred in the burying-ground two days afterwards. I had the curiosity to go to the spot where the woman had seen the procession and watch it pass, and, strange to say, I saw the men she had enumerated, and attired in the dress which she described. I asked her afterwards about the supernatural power she seemed to possess, and she told me, with tears rolling down her cheeks, that it was the curse of her life. She could not go anywhere after nightfall without seeing ghosts and goblins. I have no explanation to offer, but I can testify to the accuracy of the account I have given, J. Cameron.

[1195]—Will the following, which happened to myself, be of any use in illustrating the causes of mysterious noises?

Many years ago, when we first took possession of our present home, a large country house, built in the reign of Queen Anne, my wife and I occupied a bedroom, the walls of which were panelled. After some time we became aware of a curious ticking noise, which we heard nearly every night soon after we had retired to rest—the sound was very like that produced by the beetle commonly called a "death-watch," but it was clearly not caused by that insect—it would last for a few minutes, and then cease; sometimes, but not always, recommencing after a short interval; occasionally several nights intervened without our hearing it, when, just as we were congratulating ourselves on having got rid of our torment, for such it was really becoming, it started afresh, and went on as before; but we could never hear it in the daytime.

At length the ticking became rather worrying, but being determined not to be driven out of our room, the only alternative was to make a serious effort to ascertain the cause, which was no easy matter; the room was large, and it was difficult to decide exactly the spot from which the sound proceeded—sometimes it seemed close to our bed, at other times in a distant part of the room—we were satisfied it was caused by neither an insect, mouse, or rat; at length, after much patience and careful listening, we fixed upon one particular panel as that from which the sound seemed to proceed.

The next morning I took my tools and cut a piece out, but on peering into the vacant space behind the panelling nothing was to be seen or heard. A three-foot rule was introduced, and, on withdrawing it, I felt sure I had caught the ghost. At the end of a stout line of cobweb was firmly secured a piece of hard mortar about the size of a pea! A little consideration explained all the seeming difficulties of the case. The direction of the wind, or the varying strength of the draught behind the panel, caused our pendulum to vibrate in different ways. Sometimes it struck fully

against the wood, sometimes barely grazed it, or swung clear altogether. On calm nights, of course, it was motionless. The various noises in the house prevented our hearing it in the daytime. At all events the "Ticking Ghost" was laid, and we afterwards slept in peace. M. B.

MEAL-WORMS.

[1196]—As a reader of KNOWLEDGE I have been much interested in your entomological articles, and I venture to ask whether you can include in your series some account of the creatures called "Meal-Worms," or, if your scheme will not admit of this, will you be so kind as to tell me of some book which will give me information on the subject?

I have attempted to breed them, after the recipe of the great German authority on cage-birds, Bechstein (I keep a few birds), but with very indifferent success (I fancy I drowned them with too much beer). But I have been greatly interested in trying to follow the stages of their development. They seem to be continually shedding their skins, and at length acquire a most quaint appearance—like tiny ghosts they look.

Now I want to know whether these queer-looking, corpse-like things are the pupæ, and if so, how long do they remain in this state? And again, how is it that the black, horny beetles, which I take to be the perfect insects, vary so much in size? I thought that perfectly-developed insects never grow.

If you will favour me with some information on these points, either in the shape of an article in KNOWLEDGE or a paragraph in "Answers to Correspondents" (which I am sure the Editor would grant) I shall be very grateful; and I may be allowed to add that in your hands the subject would prove generally interesting.

S. A. Butler, Esq. F. M. DUPLOCK.

CHEESE.

[1197]—Some little time ago Mr. Williams published in your paper an article showing how to treat cheese to make it easy of digestion. I procured some cheese and bicarbonate of potash, and treated it according to the directions given; but, instead of getting a nice creamy substance, the cheese formed a toughened gluten-like paste, that nothing with a stomach like a human being could digest, I should fancy. Would you kindly give publicity to my experience, so that Mr. Williams may have a chance of setting us right in the matter? J. P. CARTER.

AMERICAN BACON.

[1198]—I lately received a letter from a young man well known to me, who emigrated some months ago to the north-western part of the United States. After describing his work there, he adds, "Don't you ever eat American pork. Its education is infinitely worse looked after than that of the Indian (pig). Any cow, or other animal that dies of disease, is thrown to them. I have often been told to do this myself. Other feeding they get too bad to mention."

This would appear to show that there may be reason in the scare both in England and in Germany regarding American hams and bacon.

A day or two since, when speaking of this to a friend whose home is in Yorkshire, he said that the labourers in his neighbourhood have found from experience that it is more economical to pay the higher price for English bacon than to buy that which comes from America; the latter, as they express it, "does not stay in the frying-pan."

I am in hopes that Mr. Mattieu Williams, who has written such interesting papers on the "Chemistry of Cookery," may be able to explain whence this difference arises. I have observed that slices of American bacon, when cooked and served at table, seem almost to shrink into nothing. Is this due to the breed of pigs or to some other cause, or can it be that "American" is a trade name for inferior bacon? COSMOPOLITAN.

LETTERS RECEIVED AND SHORT ANSWERS.

R. N. It is easy to devise many geometrical constructions for such an approximation as that. They would have no scientific value, nor any bearing on the problem of the quadrature of the circle. —D. S. MILLAR. Valentia, not Valencia. Authority, every respectable atlas.—W. H. SHARP. The notice to which you take exception was not written by me; and my short article on the Mystery of Gravity was written two years since, long before your book appeared. If your views are just they will prevail. I have not read your book myself, but I have the fullest confidence in the reviewer's judgment.—ULTIMA THULE. Cannot promise to look out answers to all such questions, as KNOWLEDGE does not claim to be a question-answering paper. But the following information chances to be handy by:—Brorsen's Comet,—Least distance from sun 0·62, greatest 5·66, eccentricity 0·8098, inclination 29° 23′, longitude of node 101° 20′; distance from node to perihelion 14° 55′, periodic time 5·561. Next perihelion passage October in the present year. For D'Arrest's the corresponding elements are respectively 1·17, 5·72; 0·6278; 15° 43′; 146° 9′; 173° 0′; 6·39; 1890, March. —ROUND SHOULDER. Club exercises such as recommended in "How to Get Strong" for development of pectoral muscles would be better than any artificial bracing.—F. W. HALFPENNY. "Honesty is the best principle" may be grammatically correct; but it does not express the same idea as "Honesty is the best policy." The word "policy" means "the art of government," as you say; but it has another meaning, to wit, plan or system of managing. The proverb means that it is good for his own interest that a man should be honest. "Honesty is the best principle" would be a far better saying, meaning that whether good for a man's interest or not, honesty is the right thing.—H. But has any man ever for a moment imagined that men can conceive either infinite space or infinite time? Until some one has claimed to do this, or even imagined that it might be done, your letter seems wanting in point. We cannot reply about the wrong views till we know what are the views attacked and the reasoning urged against them. Attacks on straw-stuffed giants would hardly suit these columns.—A PERMANENT SUBSCRIBER. The question raised in your letter respecting Heis's Celestial Atlas would involve too much technical writing to be suitable for our Correspondence columns.

Our Paradox Column.

THE FLAT EARTH AND ITS FLATTENER.

I WAS somewhat surprised to read your remarks in this week's KNOWLEDGE, not so much at their tenour, which I cordially endorse, as at the circumstances that called them forth. I was, indeed, warned by Mr. Hampden in a very characteristic letter, that I should forthwith publish an apology, or, says he, "*if* Parallax *should* consult his solicitor," the latter is sure to advise him to institute criminal proceedings against me and you, sir, for my second article in your journal. This was all I heard from the Zetetic camp about proceedings. I presume, if such had really been instituted, I should have heard something about them by this time. But up to the present I have had no intimation whatever to lead me to suppose that Parallax will take such a step. If he is not ill advised, he will know as well as I do that it would be necessary to prove, in the first instance, that my articles were libels. If I had touched him in his private capacity, then the matter would be different. My comments were, however, directed against him in his public capacity, and as such could not be looked at otherwise than as a fair criticism, unless, indeed, they had contained false statements. Parallax will be aware what an action for libel can* lead to, and as an old friend—although [our friendship was] of short duration—I should suggest what you, sir, did in your remarks, viz., that he should make use of the columns of your Journal to offer an emphatic contradiction to my statements. Should he succeed in clearing up the mysteries in which he stands for the present enshrouded I will join with you, sir, and make the fullest apology. He is bound to take now either one or the other step. So far as I am concerned I am willing to stand or fall by my statements. But he should lose no time, as delay will justify you in inserting my further articles.

Thanking you for the corrections, which convey really what I meant to say, H. OSSIPOFF WOLFSON.

April 12, 1884.

[I also would venture to point out to Dr. S. Birley that it will be his own fault *if he libel himself by silence*, when space is here offered him for full contradiction of whatever in Mr. Wolfson's statements he may be in a position to deny. His solicitor, Mr. Howard Rumney, writes to the publishers of KNOWLEDGE that both Mr. Wolfson's letters contain untrue statements. What are they? Is he not Parallax? His solicitor says he is. Is Parallax not the same as Goulden, Rowbotham, and Tryon. But at Trowbridge in

* I leave this word unchanged, as it confirms my interpretation of the same word used for "may" in Mr. Wolfson's second letter. —R. P.

1849, the author of the Zetetic Astronomy, who is Parallax, was Goulden; and in 1864 the same Parallax was Rowbotham. Is Dr. Birley a registered medical practitioner? I have the "Medical Register" before me, and there is but one Birley, Mr. (not Dr.) C. Birley, registered in 1859, in its lists. Parallax is unquestionably the writer of a book promising particular longevity. And all Parallax has done or written he has widely advertised. What is it in Mr. Wolfson's letters he desires to contradict?—R. P.]

Our Mathematical Column.

NOTES ON EUCLID'S FIRST BOOK.

By Richard A. Proctor.

(*Continued from p. 234.*)

NOW if we collect the properties of isosceles triangles involved in the three last-named propositions, we see that they present themselves as in the following propositions:—

Prop. VIII.—*The bisector of the vertical angle of an isosceles triangle bisects the base also.* This is established in the proof of Euc. I., 10.

Prop. IX.—*The bisector of the vertical angle of an isosceles triangle is at right angles to the base.* This is established in the proofs of Euc. I. 10 and 11.

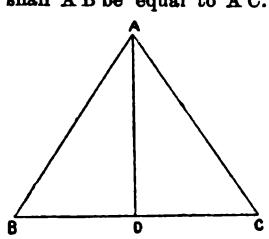
Prop. X.—*The line joining the vertex of an isosceles triangle to the bisection of the base is at right angles to the base.* This is established in the proofs of Euc. I. 11 or 12.

It is obvious that the direct converse of each of these three propositions is also true.

But there are three indirect converse theorems which are often useful. They may be called the three fundamental tests of an isosceles triangle:—

Prop. XI.—*If the bisector of the vertical angle of a triangle also bisects the base, the other two sides are equal.*

In the triangle B A C, let A D bisecting the angle B A C divide B C into two equal parts in D. Then shall A B be equal to A C. In the triangles B A D, C A D, we have the sides B D, D A, equal to the sides C D, D A each to each, and the angles B A D, C A D, which are opposite the equal sides B D, C D are likewise equal. Hence by Props. I. and II. the triangles are either equal in all respects, or else the angles B and C together make up two right angles. But the angles B and C, being two angles of a triangle are together always less than two right angles. Hence the triangles A B D, A C D are equal in all respects. Therefore A B is equal to A C.



Prop. XII. *If the line drawn from the vertex of a triangle to the bisection of the base is perpendicular to the base, the other two sides are equal.*

If (same figure) B D is equal to D C, and A D perpendicular to B C, the triangles A B D, A C D are equal in all respects by Euc. I. 4.

Prop. XIII. *If the bisector of the vertical angle of a triangle is perpendicular to the base, the sides are equal.*

If (same figure) the angle B A D is equal to the angle D A C, and A D also at right angles to B C, the triangles A B D, A C D are equal in all respects by Euc. I. 26.

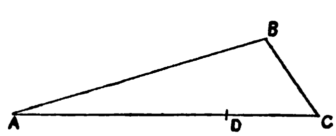
In Props. 13-15, Euclid exhibits properties of straight lines which are often useful in determining whether three or more points lie in a straight line, and also whether three or more lines pass through one point.

Props. 16-21 are of continual use in solving problems, as we shall see further on.

The following proposition is often useful:—

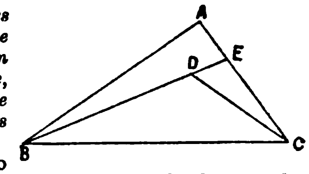
Prop. XIV.—*The difference between any two sides of a triangle is less than the third side.*

From A C, a side of the triangle A B C cut off D C equal to B C; then the remainder A D is less than A B. For if A D be equal (or greater) than A B, add D C to A D, and add B C, which is equal to D C, to A B; then A C is equal (or greater) than A B and B C together which is impossible, Euc. I., 21. And in like manner the difference between A B, B C may be shown to be less than A C; and the difference between A C and A B less than B C.



It is well to note that in place of the general theorem which forms the latter part of Prop. 21, we may substitute the following useful proposition:—

Prop. XV.—*If from the extremities* B C *of the base* B C *of a triangle* B A C, *the lines* B D, C D *be drawn to a point* D *within the triangle, then the angle* B D C *exceeds the angle* B A C *by the sum of the angles* A B D *and* A C D.

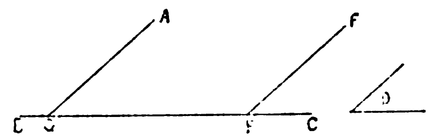


For the angle B D C is equal to the two angles D C E, D E C, Euc. I., 32; that is, to the three angles D C E, B A E, and A B E, Euc. I., 32.

The following proposition is as often applicable as Prop. 23:—

Prop. XVI.—*Prob. From a given point without a given line to draw a line which shall make with the given line an angle equal to a given rectilinear angle.*

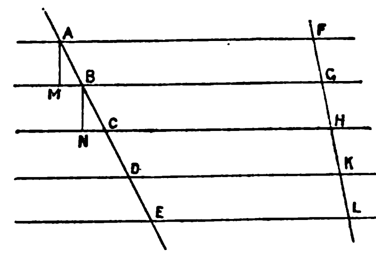
Let A be the given point, B C the given line, and D the given angle.



From any point E in B C draw E F so that the angle F E C may be equal to the angle D (Euc. I., 23). Through A draw A G parallel to F E. Then the angle A G E is equal to the angle F E C (Euc. I., 22) that is to the angle D.

Props. 27-31 exhibit the properties of parallels. To these the following very useful property may be added.

Prop. XVII.—*If there be any number of parallel lines* A F, B G, C H, *&c., and if any straight line* A E *meeting the parallels in the points* A, B, C, D, *&c., be divided into equal parts* A B, B C, C D, *&c., then any other straight line* F L, *meeting the parallels in the points* F, G, H, K, *&c., will be divided into equal parts* F G, G H, H K, *&c.*



Draw A M and B N parallel to F L. Then the angle C B N is equal to the angle B A M, and the angle A B M is equal to the angle B C N (Euc. I., 29), also A B is equal to B C. Therefore the triangles A B M, B C N are equal in all respects (Euc. I., 26). Hence A M is equal to B N. But A M is equal to F G, and B N to G H (Euc. I., 34); therefore F G is equal to G H. And similarly it may be shown that G H is equal to H K, H K to K L, and so on. Hence F G, G H, H K, &c., are all equal.

(*To be continued.*)

EASY RIDERS ON EUCLID'S FIRST BOOK.

WITH SUGGESTIONS.

Prop. 15.

39. If four straight lines meet at a point so that the vertical angles are equal, these straight lines are two and two in the same straight line.

40. If A B C, D B E are two straight lines intersecting in B, and A B is equal to B D, B E to B C; show that the quadrilateral A D C E is made up of four triangles whereof two are isosceles and the other two equal in all respects.

41. If with the same construction A B is equal to B C and D B to B E, the quadrilateral A D C E is made up of four triangles of which each opposite pair are equal in all respects.

Prop. 16.

42. In the triangle A B C, A D is drawn bisecting the angle B A C, and meeting B C in D, show that the angle B D A is greater than the angle B A D.

43. Through D a point in the base B C of an isosceles triangle A B C, a line E D F is drawn meeting A B in E and A C produced in F; show that the angle A E F is greater than the angle A F E.

44. In the figure of Prop. 17, show that the angles A B C and A C B are less than two right angles, without producing B C.

Join A to a point in B C, and apply Prop. 16 *twice.*

Prop. 17.

45. If two sides of a triangle are produced, show that the two exterior angles thus formed are together greater than two right angles.

46. Show that any three angles of a quadrilateral are together less than four right angles.

Prop. 18.

47. Each of the diagonals of a quadrilateral figure exceeds the greatest side, show that the sum of two opposite angles exceeds half the sum of the remaining angles.

48. A B C D is a quadrilateral of which A D is the longest side and B C the shortest; show that the angle A B C is greater than the angle A D C, and the angle B C D greater than the angle B A D.

Prop. 19.

49. In the figure of Euc. I., 5, show that F C is greater than B C.

50. Either diagonal of a rectangle exceeds the greatest side.

51. Either diagonal of a rectangle exceeds any line which has one extremity at an angle, and the other on a side of the rectangle.

52. The perpendicular is the shortest straight line that can be drawn from a given point to a given straight line; and of others that which is nearer to the perpendicular is less than the more remote; and only two equal straight lines can be drawn from the given point to the given straight line, one on each side of the perpendicular.

53. P and Q are points on the same side of the line A B; P C is drawn perpendicular to A B, and produced to D so that C D is equal to P C; show that Q D is greater than P Q.

Let Q D cut A B in E, and join E P: with this construction the proof is obvious.

Prop. 20.

54. The difference of any two sides of a triangle is less than the third side.

55. A point P is taken within the triangle A B C; show that the sum of the distances P A, P B, and P C is greater than half the sum of the sides of the triangle.

56. With the same construction as in Ex. 53, a point F is taken in A B; show that the sum of the lines P F and Q F is greater than Q D.

57. A B C is a triangle having the angle B obtuse. A point D is taken in B C, and in A D, D E is taken equal to A B, and E A is bisected in F. Show that C F and D F are together greater than C A, A B.

58. The diagonals of a quadrilateral are together less than the sum of any four straight lines that can be drawn to the four angles of the quadrilateral from any point whatever except the intersection of the diagonals of the quadrilateral.

59. The sides of a quadrilateral are together greater than the two diagonals together.

60. In the triangle A B C the line B D is drawn bisecting the vertical angle A B C. If any point E is taken in B D, show that the difference of the sides A B, B C exceeds the difference of the lines A E, E C.

From A B the greater of the sides A B, B C (suppose) cut off B F equal to B C the less; show that E F is equal to E C; and apply Ex. 54.

61. With the same construction, show that if the point E lies in B D produced either way, the difference of A B, B C exceeds the difference of A E, E C.

62. A straight line A D is divided into two unequal parts in the point C, and a straight line C D is drawn at right angles to A D. Show that the difference of the lines A D, B D is less than the difference of the lines A C, B C.

Prop. 21.

63. A point P is taken within the triangle A B C; show that the sum of the lines P A, P B, and P C is less than the sum of the sides of the triangle.

64. A B C D is a quadrilateral whose diagonals intersect in E, and a point F is taken within the triangle A B E. Show that the sum of the diagonals A C, B D together with twice the side A B exceeds the sum of the four lines A F, B F, C F, and D F.

(To be continued.)

Our Whist Column.

A WHIST-PLAYER'S WAIL.

By the Author of "Bumblepuppy."

(Continued from p. 256.)

Lastly, with regard to the authority.

Whist-players are law-abiding to a degree, and sufferance is the badge of all their tribe; but still they would like to know how the authority obtained what the imperfect Member for Northampton is so fond of calling his mandate; whether by divine or hereditary right, by competitive examination, by election, by appointment from the Crown, or whether he sits upon us by "the good old rule, the simple plan" *of force majeure*, as the Old Man of the Sea sat upon Sindbad.

Bartholomew Binns, an official with the highest credentials, after being selected from numerous candidates, and receiving a mandate from the sheriffs of London and Middlesex, has his decisions reviewed by twelve good men and true, and reporters are present who publish them through the length and breadth of the land? How is our executioner appointed? Who reviews his decisions? How are they promulgated? Not that it matters to me, personally. When my fatal Monday comes round, and *sus. per coll.* is written under my name in the family archives, I do not imagine it will trouble me much whether the operator was born great, has achieved greatness, or has had greatness thrust upon him. I do not object to the instrument, I object to the system; but many Whist-players are more fastidious, and protest strenuously against being treated worse than other criminals. They hold that the position of a functionary who takes upon himself to decide important questions of law, and to upset old-established precedents, and manufacture new ones on his mere *ipse dixit*, should be very clearly defined, and that if one man is to unite in his own proper person the attributes of prophet, priest, and king—three single gentlemen rolled into one—he should be duly anointed, consecrated, and crowned, *ad hoc.*

For questions involving common courtesy, for insoluble verbal quibbles, for ethical questions of this type, "Ought A to sit quietly at the table while his partner B picks Z's pocket? and if he ought, is it right for him to share the plunder?" and for the host of minor cases which constantly arise, and for which no law could possibly provide, no better management than the present could be devised. As long as maniacs exist in the land, klepto- dipso- homicidal or whist—offences must come, and in disposing of them—when a cadi is the only effective treatment that can be openly suggested—the editor of the *Field* is *facile princeps*,

In faith, he is a worthy gentleman,
Exceedingly well read.

Only if he is to be the *de facto* authority in *all* cases, why not give him the three sanctions just mentioned, and make him the authority *de jure?* Then—as the *Field* is not a Whist gazette, and can scarcely be expected to devote its columns to advertising gratuitously every legislative change, and any space it has to spare is used rather for elaborating the ceremonial than for settling the laws of the club—in token of our esteem, let us club together (I shall be most happy to contribute my mite) and present him with a piece of chalk, a duster, and a black board, to be set up in some easily-accessible spot—say, the middle of Pall-mall or St. James's-street. Make it the official notice-board! When new decisions are created let them be legibly inscribed upon it, *coram populo!* When well-known decisions are abrogated let them be carefully rubbed out at once. Since the Bastille was destroyed and *lettres de cachet* with it, there has been no authority without a notice-board; the Salvation Army has its "war cry," and the Pope himself, when he propounds a new dogma, propounds it *ex cathedrâ.*

That is one remedy. Though it is not perfect it has two advantages—it is inexpensive and if in future any of us should still remain in ignorance, we should be in ignorance by our own fault, and not by misfortune; and at any rate it is a more simple and less tortuous plan than upsetting well-known decisions in an unofficial newspaper, while new editions of our two standard Whist-books are subsequently brought out without one word of comment or warning.

The alternative remedy—by no means novel, it has been suggested, *usque ad nauseam*, and I only bring it forward again because at present confusion is worse confounded than it has ever been in my recollection—is for the leading clubs to appoint a small committee of representative Whist-players, with power to revise any decisions they may see fit; and when they have revised them either to append them to the laws of Whist, or to place each decision as a rider under its own particular law, and every such decision should be final.

Questions of strict law should never have been submitted to an

arbitrator at all; they should have been cleared up long ago by the legislators themselves; though important, they are not very numerous, and as they have been well threshed out, and all their difficulties are known, the entire matter might be completed in a few hours. Why should London wait? Pembridge.

Our Chess Column.

By Mephisto.

Solution of Problem No. 115, by E. N. Frankenstein, p. 94.

1. R to R5   2. R to K2, mate

if K to K6   2. B to Q3, mate

if K to B4   2. Kt to B6, mate

if K to Q4   2. Q to K5, mate

if P to Q4   2. B to Q3, mate

if QKt moves   2. Q × B, mate

if KKt moves   2. Q to B4, mate

if B moves

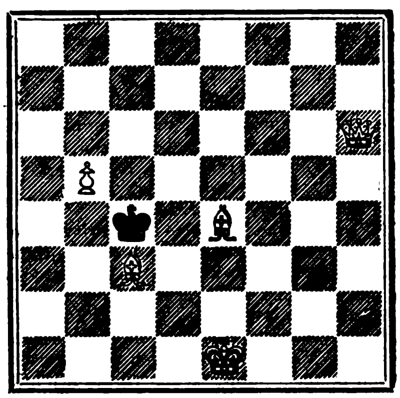
Correct solutions received (additional).—G. A. Porritt, H. P. F., W. Hanrahan, Senex, J. G. Barber, J. Wahltuch, Lawrence Small. Incorrect.—G. T. M. E., W. B. Hammond, Field House, W. P. H., A. T. J., Fred Lobnitz, W. C., R. H. Punshon.

SELECTED PROBLEMS.

I.

By S. Loyd.

Black.



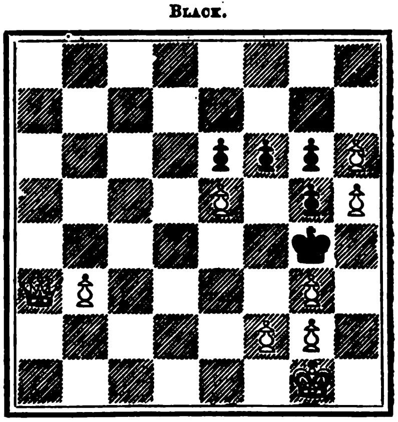
White.

White to mate in two moves.

II.

By S. Loyd.

Black.



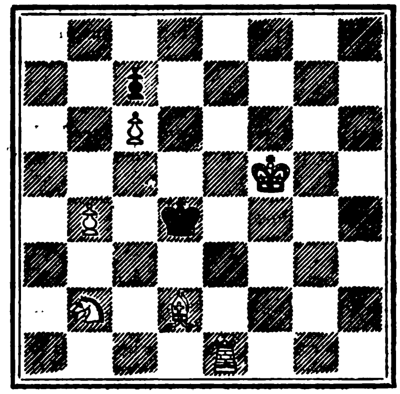
White.

White to mate in three moves.

PROBLEM No. 116.

By H. W. Sherrard.

Black.



White.

White to play and mate in four moves.

(Both neat and subtle.—Chess Ed.)

ANSWERS TO CORRESPONDENTS.

*** *Please address Chess Editor.*

J. Osborne.—The position you refer to is as follows:—White K on QR6, R on Q7. Black K on QKt sq. P on QB7. White draws by R to Kt7 (ch), K to Bsq (best). R to Kt5, P to B8 (Q). R to B5 (ch.) and if Q × R White is stalemate.

Watter.—We understand that a book on the openings will shortly be published. In reply to 1. P to K, P to K4, 2. Q to R5; besides Kt to QB3, you may also play Kt to KB3. A speedy development is obtained by the sacrifice of the P.

Contents of No. 128.

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AN ILLUSTRATED
MAGAZINE OF SCIENCE
PLAINLY WORDED-EXACTLY DESCRIBED

LONDON: FRIDAY, APRIL 25, 1884.

CONTENTS OF No. 130.

HOW TO GET STRONG.

(*Continued from page 243.*)

THE MUSCLES OF THE LEGS.

MANY seem to imagine that the muscles of the legs get all the development they require from walking, with such more active exercise as may be obtained from casual running, or an occasional game of lawn-tennis or the like. The result is that we seldom see the lower limbs nearly so well developed as they ought to be and very readily might be. Few muscles respond more quickly and satisfactorily to training than those of the legs, and though our costume does not, like that of the savage, allow the well-developed and shapely legs of the athlete to display their proportions very openly, yet even in that remarkable development of modern civilisation the "straight trousers" a well-formed leg (or preferably a pair) will manifest a marked superiority over ill-shaped lower extremities. It is not however, so much on the shape of the legs as on the grace and elegance of their movements that exercise tells most effectively. In the street and on the lawn, in the parlour and in the dancing-hall, the owner of active and lissome legs has a marked advantage over stiff and weak-legged beings. You will note the difference even in the way in which one or the other will stoop to pick up—let us say—a lady's fallen handkerchief. In all social exercises, within doors and abroad, the difference is marked. A man may be awkward with his arms, and no one will much notice the circumstance, but he who is stiff and awkward on his feet, shipwrecky about the knees, and cramped in the thighs, appears altogether ungainly and uncouth even if his arms, shoulders, chest, and waist are well developed and shapely.

THE UPPER THIGH.

Let us begin with the muscles of the upper thigh, taking first those in front.

The most easily obtained exercise for the front thigh muscles is fast walking. In this respect there is all the difference in the world between a quick walk and a slow one. Walking sharply, with the body held well upright, and taking hills with special energy, the thighs are well exercised; but in slow walking they hardly get any exercise at all. Running is good too, especially long distance running, with a long stride. Mr. Blaikie recommends that the heel should never touch the ground; but Mr. Muybridge's instantaneous photographs have shown that runners who believe that they run without bringing the heel to the ground do really touch the ground first with the heel, in the long striding run we are considering.

Maclaren regards jumping as the very best exercise for the legs. But it will be better to let each actual jump be an easy one, getting the exercise out of the number of jumps made rather than out of the effort required for each.

The following exercises should be practised daily by all who wish to have good thigh muscles.

First, standing upright with the feet close together, lower the body pretty sharply, till the tips of the fingers touch the ground on either side of the feet, on which spring sharply upright again. The heels rise in this exercise as the body descends. Repeat the dip from ten to twenty times. At first there is a tendency to topple over, but this will soon be corrected. (Note how the muscles and tendons in the knee regions are limbered by this exercise). The shoulders should be well squared as the body rises to the upright position. Gradually the dip may be carried farther until the whole inside of the finger part of the hands touches the ground, the body being sent up more sharply by the resulting pressure against the floor.

Secondly, go through similar movements, without lifting the heels from the ground. The hands now touch the ground a little farther forward, and as the dip increases, the backs of the fingers touch the ground, and even after awhile the backs of the hands as far as the wrists can be brought to the floor. The dips in this as in the preceding exercise should be made neatly and sharply,—in this way the knees get well exercised as do the calves and loins, while the quickened respiration shows how the chest takes part in the work.

Thirdly, both forms of exercise may be gone through with dumb-bells in the hands; and the dumb-bells thus used may be tolerably heavy.

Fourthly, there is a form of exercise for the upper thigh muscles which belongs rather to the nature of an athletic feat than to that of systematic exercise. It may therefore be used or let alone as may be preferred. It is not suited altogether perhaps to middle-aged gentlemen (or ladies) of obese tendencies:—Standing on the left foot, hold out the right leg at right angles to the body: now steadily sink till you are sitting on the left foot; you may perhaps sit unexpectedly on the floor, but persevere for all that, until you can comfortably sit down on one foot and steadily rise therefrom with the other leg all the time parallel to the horizon. When you can do this comfortably on either foot, you can invite your friends to do likewise, and if they have not practised they will interest you by their inevitable failures.

The upper thigh gets exercised also in the familiar feat of lowering the body on the left or right foot while the right or left foot is held by the right or left hand; the knee of the upheld leg being brought to the ground. This is in reality an easier exercise than the last; at least it always seemed so to me; though many who can do neither suppose this the harder: the difference between the two consists chiefly in the circumstance that in the former the sole of a foot rests all the time wholly on the floor, while in the latter the heel rises from the ground just before the knee touches it.

(*To be continued.*)

COINCIDENCES AND SUPERSTITIONS.

By Richard A. Proctor.

(*Continued from p. 238.*)

THE history of astronomy has in quite recent times afforded a very remarkable instance of repeated coincidences. I refer to the researches by which the theory has been established, that meteors and comets are so far associated that meteor systems travel in the track of comets. It will readily be seen from the following statements that the demonstration of this theory must be regarded as partly due to singular good fortune :—

There are two very remarkable meteor systems—the system which produces the November shooting-stars, or *Leonides*, and that which produces the August shooting-stars, or *Perseides*. It chanced that the year 1866 was the time when a great display of November meteors was expected by astronomers. Hence, in the years 1865 and 1866 considerable attention was directed to the whole subject of shooting-stars. Moreover, so many astronomers watched the display of 1866, that very exact information was for the first time obtained as to the apparent track of these meteors. It is necessary to mention that such information was *essential* to success in the main inquiry. Now it had chanced that in 1862 a fine comet had been seen, whose path approached the earth's path very closely indeed. This led the Italian astronomer Schiaparelli to inquire whether there might not be some connection between this comet and the August shooting-stars, which cross the earth's path at the same place. He was able, by comparing the path of the comet and the apparent path of the meteors to render this opinion highly probable. Then came inquiries into the real paths of the November meteors, these inquiries being rendered just practicable by several coincidences, as—(1) the exact observations just mentioned; (2) the existence of certain old accounts of the meteor shower; (3) the wonderful mastery obtained by Professor Adams over all problems of perturbation (for the whole question depended on the way in which the November meteors had been perturbed); and (4) the existence of a half-forgotten treatise by Gauss, supplying formulæ which reduced Adams' labour by one-half. The path having been determined (by Adams alone, I take this opportunity of insisting),* the whole question rested on the recognition of a comet travelling in the same path. If such a comet were found, Schiaparelli's case was made out. If not, then though the evidence might be convincing to mathematicians well grounded in the theory of probabilities, yet it was all but certain that Schiaparelli's theory would presently sink into oblivion. Now there are probably hundreds of comets which have a period of thirty-three and a quarter years, but very few are known—only three certainly—and one of these *had only just been discovered* when Adams' results were announced. The odds were enormous against the required comet being known, and yet greater against its having been so well watched that its true path had been ascertained. Yet the comet which had been discovered in that very year 1866—the comet called Tempel's, or I. 1866—was the very comet required to establish Schiaparelli's theory. *There* was the path of the meteors assigned by Adams, and the path of the comet had been already calculated by Tempel before Adams' result had been announced; and these two paths were found to be to all intents and purposes (with an accuracy far exceeding indeed the requirements of the case) *identical*.

To the remarkable coincidences here noted, coincidences rendered so much the more remarkable by the fact that the August comet is now known to return only twice in three centuries, while the November comet returns only thrice per century, may be added these :—

The comet of 1862 was observed, telescopically, by Sir John Herschel under remarkably favourable circumstances. "It passed us closely and swiftly," says Herschel, "swelling into importance, and dying away with unusual rapidity. The phenomena exhibited by its nucleus and head were on this account peculiarly interesting and instructive, *it being only on very rare occasions* that a comet can be closely inspected at the very crisis of its fate, so that we can witness the actual effect of the sun's rays on it." (This was written long before Schiaparelli's theory had attracted notice). This comet was also the last observed and studied by Sir John Herschel. Temple's comet, again, was the *first comet ever analysed with the spectroscope*.

It will be remarked, perhaps, that where coincidences so remarkable as these are seen to be possible, it may be questionable whether the theory itself, which is based on the coincidence of certain paths, can be accepted as trustworthy. It is to be noticed that, whether this be so or not, the surprising nature of the coincidences is in no way affected; it would be as remarkable (at least) that so many events should concur to establish a false as to establish a true theory. This noted, we may admit that in this case, as in many others, the evidence for a scientific theory amounts in reality only to extreme probability. However, it is to be noticed that the probability for the theory belongs to a higher *order* than the probability against those observed coincidences which rendered the demonstration of the theory possible. The odds were thousands to one, perhaps, against the occurrence of these coincidences; but they are millions to one against the coincidence of the paths as well of the November as of the August meteors with the paths of known comets, by mere accident.

It may possibly be considered that the circumstances of the two last cases are not altogether such as to assure us that special intervention was not in question in each instance. Indeed, though astronomers have not recognised anything supernatural in the series of events which led to the recognition of the association between meteors and comets, some students of archæology have been disposed to regard the events narrated by Dr. Young as strictly providential dispensations. "It seems to the reflective mind," says the author of the "Ruins of Sacred and Historic Lands," "that the appointed time had at length arrived when the secrets of Egyptian history were at length to be revealed, and to cast their reflective light on the darker pages of sacred and profane history. . . . The incident in the labours of Dr. Young seems so surprising that it might be deemed providential, if not miraculous." The same will scarcely be thought of such events as De Morgan recorded (and their name is legion); since it requires a considerable stretch of imagination to conceive that either the discovery of the name of a certain editor, or the removal of De Morgan's difficulties respecting the siege of Boston, was a *nodus* worthy of miraculous interposition.

For absolute triviality, however, combined with singularity of coincidence, a circumstance which occurred to me several years ago appears unsurpassable. I was raising a tumbler in such a way that at the moment it was a few inches above my mouth; but whether to examine its substance against the light, or for what particular purpose, has escaped my recollection. Be that as it may, the tumbler slipped from my fingers and fell so that the edge

* Leverrier, Schiaparelli, and others calculated the path on the assumption that the occurrence of displays three times per century implies a periodic circulation around the sun in about thirty-three years and a quarter; but Adams alone proved that this period, and no other, must be that of the November meteors.

struck against one of my lower teeth. The fall was just enough to have broken the tumbler (at least, against a sharp object like a tooth), and I expected to have my mouth unpleasantly filled with glass fragments and perhaps seriously cut. However, though there was a sharp blow, the glass remained unbroken. On examining it, I found that a large drop of wax had fallen on the edge at the very spot where it had struck my tooth, an indentation being left by the tooth. Doubtless the softening of the shock by the interposition of the wax had just saved the glass from fracture. In any case, however, the surprising nature of the coincidence is not affected. On considering the matter it will be seen how enormous were the antecedent odds against the observed event. It is not an usual thing for a tumbler to slip in such a way. It has not at any other time happened to me, and probably not a single reader of these lines can recall such an occurrence either in his own experience or that of others. Then it very seldom happens, I suppose, that a drop of wax falls on the edge of a tumbler and there remains unnoticed. That two events so unusual should be coincident, and that the very spot where the glass struck the tooth should be the place where the wax had fallen, certainly seems most surprising. In fact, it is only the utter triviality of the whole occurrence which renders it credible; it is just one of those events which no one would think of inventing. Whether credible or not, it happened. As De Morgan says of the coincidences he relates, so can I say for the above (equally important) circumstance, "I can solemnly vouch for its literal truth." Yet it would be preposterous to say that there was anything providential in such an occurrence. Swift, in his "Tale of a Tub," has indicated in forcible terms the absurdity of recognising miraculous interventions in such cases; but should it appear to some of my readers that, trivial though the event was, I should have recognised the hand of Providence in it, I would remark that it requires some degree of self-conceit to regard oneself as the subject of the special intervention of Providence, and moreover that Providence might have contrived the escape in less complicated sort by simply so arranging matters that the glass had not fallen at all. So, at least, it appears to me in this case; and similar reasoning applies to other cases of the kind.

(To be continued.)

THE PLANET MARS.

MARS was doubtless one of the first among celestial objects whose planetary nature was recognised by ancient astronomers. It would be a question of purely speculative interest to discuss whether Jupiter or Mars was the first discovered planet. Despite the superior brilliancy of Venus, one or other of the two planets named must have been the object which first, after the sun and moon, was noticed as a wandering orb. For the circumstance that Venus was known originally under two names, suffices to prove—what, indeed, might be anticipated independently—that the ancients failed at first to recognise the brilliant stars of evening and morning as one and the same object. Jupiter second only in brilliancy to Venus at her brightest, and, unlike her, resplendent during the darkest hours of the night, must have attracted the attention of men, as soon as they commenced to notice the stars at all. Yet his motion among the fixed stars might have escaped the notice of astronomers for some little time —because, although sufficiently conspicuous when attention is directed to the planet for several weeks, the motion does not in a few days produce a marked change in the position of the planet. Mars, less brilliant than Jupiter, yet shines, at times, more brightly than any of the fixed stars, and his ruddy tint serves to render his appearance more remarkable than it would otherwise be. Once noticed with attention, even for a few days only in succession, Mars must have been recognised as a planet, since he moves rapidly among the fixed stars. On this account he might possibly have preceded Jupiter as respects the epoch of his recognition as a planet. On the other hand, there is a circumstance in Jupiter's favour—that he comes to opposition at intervals of thirteen months and shines brightly for several months preceding and following opposition; whereas, Mars only comes to opposition at intervals of about twenty-six months, and the brilliancy with which he shines when in opposition is quickly attained and as quickly lost. On the whole, the probability is that the discovery of Jupiter's nature preceded by a few months that of Mars. The date of each discovery must undoubtedly be placed in the remotest antiquity.

At an early period the planet Mars was noted by astronomers as distinguished from his fellow-wanderers by several remarkable peculiarities. I have already mentioned the rapid variation of brilliancy recurring during the progress of each opposition—by which term I refer to the interval (of several months) in about the middle of which opposition takes place. But this is not the sole, nor the most noteworthy peculiarity affecting the brilliancy of Mars. For whereas in *different* oppositions the planet Jupiter was noticed to present always about the same degree of brilliancy, Mars was observed to be very variable in this respect: at some oppositions he approached the planet Jupiter in brilliancy, at others he scarcely exceeded Saturn. A recurrence of several brilliant oppositions followed by a recurrence of several in which the planet's light shone less brightly, was also a phenomenon noticed by ancient astronomers. They do not appear to have detected the fact that the brilliancy of Mars was always greater when he came to opposition in one particular region of the Zodiac. Nor do they seem to have noticed the period—on an average about sixteen years—which seemed to belong to the cycle of variation.

Another circumstance which at an early epoch attracted the notice of astronomers, was the variability in the interval separating successive oppositions of the planet,—or, in other words, the variable synodical period of Mars. Slight variations might have been noticed in the synodical periods of other planets,—but nothing approaching the remarkable variability of Mars in this respect. The consideration of the orbit of Mars, and of his motion therein, on which this peculiarity and those noted in the planet's variation of brilliancy will be found to depend, is left to another occasion.

Mars, shining with a ruddy light, which waxes and wanes in a striking yet mysterious manner, was selected in very ancient times as the planet of war. Astronomers selected for their symbol the shield and spear of the warrior of ancient times—a selection alluded to by Tennyson in the lines describing how in a vision Maud—

> She pointed to Mars
> As he glowed like a ruddy shield on the Lion's breast.

It may be remarked that that was said in 1863, and now again—

> Mars glows like a ruddy shield near the Lion's heart.

Astrologers assigned to Mars an influence corresponding to his association with war and warlike ideas. A person born when the influence of Mars was in the ascendant, was supposed to be characterised in a marked degree by those

virtues and defects which have been associated from time immemorial with the martial character. Alchemists considered the metal iron to be placed under the special influence of Mars. The association is probably very ancient, far preceding, doubtless, the period to which the invention of alchemic arts is usually assigned. In the present day the association seems very natural, when it is considered how largely iron enters into the construction of nearly every warlike weapon. But of old this was not so, and it is possible that other associations than those connected with the employment of iron were assigned to Mars. It is not improbable, however, that this happened later, and when the advantages of the use of iron in the construction of warlike weapons were beginning to be recognised.

PLEASANT HOURS WITH THE MICROSCOPE.

BY HENRY J. SLACK, F.G.S., F.R.M.S.

FAMILIES who possess a microscope should acquaint themselves with the appearance and structure of a variety of objects which are used in common life. Often an agreeable surprise is the result, and astonishment is always expressed when attention is directed to the scales of fish. There is a good deal of contradiction and inaccuracy in many books on this subject. The fish-scales were, and are still sometimes represented as products of the cuticle or epidermis, and a great and fundamental difference is affirmed between these formations and the scales of snakes. In some books it is said that snakes shed their scales with their skin; but the fact is, as stated by Mr. C. Cooke, in his useful little book, "Our Reptiles," that the cast-off skin is a thin delicate cuticle, which had covered all the projections and inequalities of the true skin, containing a little pouch for each of the scales." Mr. Edward Newman put the matter well when he explained that, while the outer skin of quadrupeds is clothed with hair, that of birds with feathers, of fishes with scales, in reptiles it is uncovered and naked. They are also not deciduous, as hairs, feathers, and some scales are. Newman was one of the first, if not the first, to point out that the snake's scales are projections, folds, or rugosities of the under skin. In the growth of the various fish-scales, Sir R. Owen tells us "the chief seat of vascularity, and greatest activity and variety of development, is at the periphery of the derm (true skin) between it and the epiderm (cuticle).

In the eel the scales are not seen, unless the skin is well cleaned and viewed as a transparent object, or the soft epiderm, or outer skin, is scraped off, and the scales taken out of the derm. They then appear very pretty objects with a magnification of 50 or upwards linear. They are only two or three lines long, and exhibit a network of cartilaginous fibres. Polarised light and selenite invest them with beautiful colours. The sand-eel skin is a very instructive subject when mounted in balsam. The scales are rather larger, some distance apart, and both skin and scale glow with blue, gold, and crimson, or other colours, under the polarising treatment.

Agassiz divided fishes in a manner very convenient for the geologist studying their fossil remains, into groups characterised by varieties of scale.

The pike affords a good example of the cycloid scale, which is roundish, with radiating and concentric lines of considerable beauty. The ctenoid (or comb scale) is found in the perch, and the sole, whose skin is remarkably beautiful as an opaque object, with its rows of scales exhibiting series after series of brilliant, glassy-looking and radiating teeth projecting out of the skin. Fig. 1, from "Micrographic Dictionary," gives some idea of its appearance, but engraving cannot render the effects of light upon it.

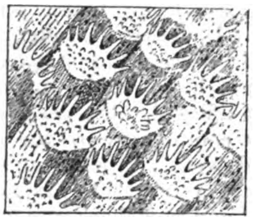


Fig. 1.

Fig. 2.

The ganoid (shining or glittering) scale is a decidedly bony formation, with an enamelled surface, which has been called ganoin. This substance, Owen says, is usually confined to two-thirds of the outer surface of the scale. Most of the fish with these scales are extinct, but common as fossils. Such scales often supplied a complete armour-suit for their wearers, and the small ones of Pholidophorus—a species common at Lyme Regis—make interesting objects for a microscope, and should be compared with a section of recent bone. The placoid scales in Agassiz's system are often of great beauty. They have projecting teeth springing from a firm plate. The skin of the spotted dogfish seen as an opaque object is extremely elegant. Innumerable rows of brilliant crystalline-looking projections reflect the light. Their shape is shown in Fig. 2. Some of the sharks with this kind of skin afford the well-known shagreen, now neglected by fashion, but which is a very strong and beautiful substance for many ornamental purposes, especially the fine green kind, with white projections.

Couch, in his "Fishes of the British Islands," describes the sturgeons—the general texture of whose skin is soft and thickly covered with mucous pores—as having bony plates of the placoid sort on their sides and head, and he quotes Owen, who considers this armature acts as ballast by its weight, as well as protecting the creature against blows from stones and other objects thrown by currents against it as it grovels amongst the decomposing matter at the bottom of shoal water. The brain is specially protected against injury, and the weight of the armour compensated by a large expansive air-bladder, which facilitates its rising in the water whenever it requires.

Dr. Mandl showed that the growth of the projecting teeth of fish-scales was carried on like that of real teeth. An account of his researches is given in Tod's "Encyclopædia of Analogy and Physiology," with a copy of his illustration as in Fig. 3, which exhibits these teeth from a rudimentary to a final stage.

An interesting account of fish-scales is given in Gosse's excellent "Evenings at the Microscope." As an illustration of the cycloid sort he takes the gold-fish, and gives sketches of their different shapes. On carefully detaching a scale, he finds, on the under side, a layer of soft gleaming substance, silver or golden, according to the hue of the fish. Spreading a little of the substance on glass, in a water-drop, and viewing it with a power of 300 linear, he discovers an infinite number of flat, oblong prisms, or crystals, so transparent as to be scarcely seen in transmitted light, but flashing in reflected light. The colour is not caused by them, but by pigment cells, amongst which they shine. When seen in the water-drop, he describes each spiculum

vibrating and quivering as if with spontaneous motion, and he supposes in the living creature a certain freedom of motion of these objects produces "the beautiful pearly play of light that marks these lovely fishes as distinguished from the light reflected by a uniformly polished surface."

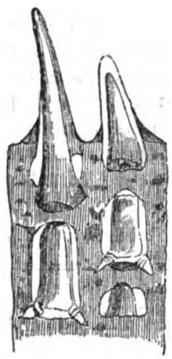


Fig. 3.

Mr. Gosse also describes the aspects of a scale of the green wrasse seen with × 600, both flat, and at the cut edge of a longitudinal section made with fine sharp scissors. The upper layer he regards as formed by successive deposits from beneath, "but after a few have been deposited, they begin to slit, probably by contraction in becoming solid; and the lower layer is formed after each upper one is hardened, exceeding its length by a little and filling up the slit; then this lower layer becomes the upper layer of the next course, slitting, and turning up its terminal edge as it hardens, and so on as long as the fish lives." In the pike he finds "the portions separated by slitting, instead of expanding and leaving spaces to be filled up, actually close over each other."

Owen describes the scales of the Lacertians (lizards, &c.) as thickened epiderm or horn. In crocodiles, he says, "the conversion of parts of the integument into bone is constant, and the osseous structure shows interlaced or crossing fibres, like that of the derm in and from which the scales are developed."

In dealing with fish-scales for the microscope, the more delicate kinds may be mounted whole in Canada balsam; others may be split; but the ornamental skin of the rays, dogfish, &c., should be well cleaned and mounted in open wooden cells, only covering a rare kind with glass. In some fishes a powerful weapon of attack and defence is formed in a manner like Dr. Mandl's sketch. The sting-ray of our coasts has, about two-thirds from the end of its tail, a most formidable jagged dagger, a wound from which is very serious, and was deemed fatal by the ancients.

THE MORALITY OF HAPPINESS.

A Study of the Scientific Aspect of Conduct.

By Thomas Foster.

CARE OF OTHERS AS A DUTY.

(*Continued from p. 242.*)

THE relations within a family present on a small scale a picture of the relations among the members of a race or nation, as these in turn present a miniature of the relations between the different races and nations which form the human family. As men rise in the scale of being, they pass from the sense of duty within the family to the sense of duty between man and man throughout society, and thence—though as yet this development is very limited—to the sense of right between different races and nations. We have seen that undue care of self is self-injurious and eventually must be self-destructive in the family. There is a corresponding law for undue care of self in social relations, as there is, (however persistently at present the vast majority of men overlook or fail to see the fact) for undue regard of self among the nations. We may mistakenly regard undue care of self in the body social as cleverness, aptitude for business, and so on; and we may mistakenly regard national selfishness as patriotism: but the process of evolution is as certainly working towards the elimination of one as of the other form of undue egoism.

The main condition of social welfare and of social progress, is that the union which society implies shall work for the benefit of those associated. If the balance of effects resulting from association be evil, the body social must inevitably dissolve in the long run.

Now by laws of greater or less severity the members of a race or nation may be compelled to recognise each others' claims. Or such recognition may be assured by the fear of retaliation if the claims of others are neglected. In such cases, however, the gain to each, or the egoistic advantage of association, is small. Enforced recognition of altruistic rights is in itself disagreeable. The more disagreeable it is the oftener will cases arise where the laws have to be called into operation (and their operation is by our supposition painful), or where retaliatory action is aroused, with waste of energy and disagreeable effects on either side. A society so restrained is held together by but weak bands, and is ill-fitted to support itself against external enemies. Internal co-operation for the benefit of the community cannot be active under such circumstances. The products of labour are insecure. Moreover whatever has to be done in the way of self-protection or of the safeguarding of property is so much withdrawn from the advancement of the general interests of the body social.

We have only to consider the condition of any European country, our own included, in the good old times which so many ignorant persons regret as a sort of golden age, to see how unsatisfactory must be the state of a nation in which only a stern code of laws, or the dread of retaliation, protects each against the undue egoism of his fellows. Internal wrongdoing and the necessity for constant struggle to resist such wrongdoing, made each nation unstable. Our good old England was invaded and conquered over and over again in consequence of instability so produced. From long before the invasions by Saxon hordes under pirate chieftains to long after the invasion by Normans under the bastard descendant of the pirate chief Rollo, England was made wretched and miserable by constant contests, having their origin invariably in that undue egoism which we now call rapine and plunder. None—not even the most powerful—were secure. The castles we find so picturesque and romantic, the battles which seem so glorious, the chivalry in which we see so much splendour, all tell us of a state of barbarism, of abject misery for the majority of magnificent discomfort for the powerful. In the unsafety of those days, however, resided the certainty that the undue egoism of "the good old times" would by a natural process of evolution be eliminated. It is not yet fully eliminated; probably centuries will elapse before it is even in great part got rid of; but it is manifestly much reduced. We still have laws to protect us against wrongdoing, but the worst wrong-doers—those who of yore were the principal component parts of the body politic—no longer exist in the same way as of old. A much larger proportion of the social body recognise regard for others as a duty; no inconsiderable proportion recognise it as a pleasure; and what

is of more importance still, men recognise the advantage of encouraging these changed tendencies.

These changes have come on so gradually that few consider how important they really are. It is not too much to say that a large proportion of the Englishmen of our day would find life not worth living if the old state of things were restored, if, for instance, life and property and reputation became as insecure now as in the days of the Plantagenets, the Tudors, or even the Stuarts.

And here it may be noticed that those who neglect the consideration that they form part of the social body and refrain from the taking due part in maintaining a healthy social state suffer from the defective arrangements which they permit to remain uncorrected. We see this in very marked degree in America though it can be recognised clearly,—far too clearly,—in our own country. There the best men keep out of politics for a reason which rightly understood should make all the best men take most anxious interest in politics. Because in America offices are too often filled by mere adventurers, because bribery and corruption are rife, and because fraudulent conduct is common among politicians, therefore should it be held the duty of every right-minded American to do his best to enforce the wholesome changes so obviously required,—as they might be enforced if so many of the best Americans were duly altruistic. But as a matter of fact the very circumstance which should arouse all the best in America to vigorous action is made the chief reason for withdrawing from public duties.

In our own country the same undue egoism shows itself in another and a scarcely less mischievous form. The individual members of the community find relief in the thought that social duties may be handed over to government. It seems easier to talk laws into existence for getting things done, than to do them. The laws are easily passed, but the doing of what is necessary passes in a great number of cases into the hands of men not nearly so much interested in the doing of it as those who passed the laws appointing them to the work,—nay often, by the very nature of the laws so passed, interested rather in delaying than in pushing on the work.

As Mr. Spencer well puts it, the man who thus shirks the duties which he owes to the community of which he forms part, who plumes himself on his wisdom in minding his own business, "is blind to the fact that his own business is made possible only by maintenance of a healthy social state, and that he loses all round by defective governmental arrangements. When there are many like-minded with himself—when as a consequence, offices come to be filled by political adventurers, and opinion is swayed by demagogues—when bribery vitiates the administration of the law and makes fraudulent State transactions habitual; heavy penalties fall on the community at large, and among others on those who have thus done everything for self and nothing for society. Their investments are insecure; recovery of their debts is difficult; and even their lives are less safe than they would otherwise have been. So that on such altruistic actions as are implied, firstly in being just, secondly in seeing justice done between others, and thirdly in upholding and improving the agencies by which justice is administered, depend, in large measure, the egoistic satisfactions of each."

Apart from dangers directly affecting life and property, those resulting from undue egoism in business relations show the necessity of just altruism for the welfare and happiness of the social body. Not only is it well for each to recognise the rights of others, but each is interested in securing due recognition of altruistic rights by his fellows. The evils resulting from business frauds affect the welfare of the community. To quote the illustrative cases cited by Mr. Spencer, "the larger the number of a shopkeeper's bills left unpaid by some customers, the higher must be the prices which other customers pay; the more manufacturers lose by defective raw materials or by carelessness of workmen, the more must they charge for their fabrics to buyers. The less trustworthy people are, the higher rises the rate of interest, the larger becomes the amount of capital hoarded, the greater are the impediments to industry; the further traders and people in general go beyond their means, and hypothecate the property of others in speculation, the more serious are those commercial panics which bring disasters on multitudes and injuriously affect all."

(*To be continued.*)

THE UNIVERSE OF SUNS.

By RICHARD A. PROCTOR.

(*Continued from page* 225.)

IN the papers which appeared in the years 1817 and 1818 Sir W. Herschel showed much of his former strength, but little of his former elasticity of mind. He clearly indicates the lines of inquiry by which certain results should be sought, and he travels with zeal and energy along those lines, but he no longer seems ready as of yore to explore the neighbouring country in order to recognise whether he has chosen the best paths, nor does he seem to recollect that he had already travelled along certain portions of these paths and had been led to recognise long since that they are not to be regarded as altogether trustworthy roads. We cannot wonder, considering that Herschel was now nearly eighty years old, that he should thus begin to show signs of failing elasticity, dimmed vision, and clouded memory. So far is it from being unfair to his memory to notice the change, that—in my judgment—it seems only just to him to ask that these two last papers written in extreme old age should not be allowed to outweigh the conclusions of his matured judgment when his wonderful powers of observation were still in full vigour, while age had not yet touched or impaired the singular clearness and versatility of his reasoning. After all he worked with apparently full power up to his seventy-fifth year, and it is only in the two closing papers of the marvellous series contributed by him to the records of the Royal Society that any diminution of his energies can be noticed.

We have seen that after long and careful study of the Milky Way, Sir William Herschel had become convinced that the stars forming that region of the stellar system are very differently arranged from those in our immediate neighbourhood which form the scattered stars of our constellations. He recognised also that the rich regions of the Milky Way, where rounded aggregations of stars are seen, are real cloud-like regions of star-strewn space drawn apart, so to speak, so as to be surrounded on all sides by relatively barren regions.

It does not inevitably follow from this, but manifestly must be regarded as highly probable, that in such richly crowded regions of space stars of many different orders of real size are gathered together. The stars seen within these rounded parts of the heavens are of many different orders of *apparent* size, but the brighter ones *may* be much nearer to us than the rounded cluster, and merely seen as it were by accident in the same field of view. But while of course this is possible, it is highly improbable, and in fact when attention is directed to the numerical arrangement of

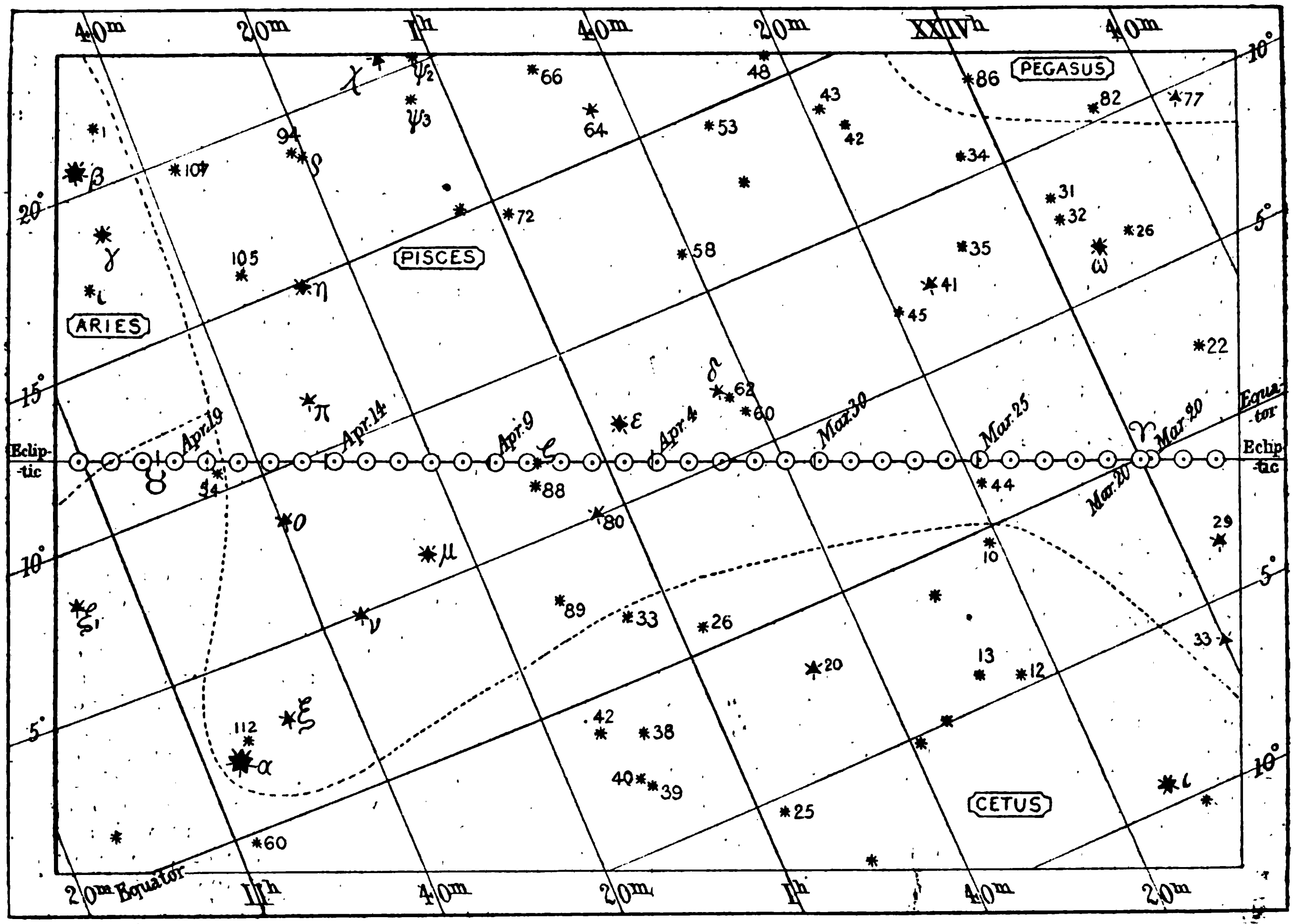
these brighter stars, it must be rejected as impossible. If we take a circular space say four degrees in diameter on the heavens, where we see a rounded cloudlike mass of milky light, and within this region find ten times as many stars of the first ten magnitudes as in any equal region around that rounded space, we are justified in regarding it as absolutely certain that the excess in the number of stars so seen within the apparent limits of the cluster is due to there being actually a greater number of those stars within the real limits of that rounded cluster. If on the skies we see a cloud which we know to be a cloud of insects—say locusts—and in the same field of view perceive a number of birds, of what kind we cannot tell, but all of them so situate as to appear either among or very close to the cloud of insects, we do not trouble ourselves to reason about the real situation of those birds; we *know* they are really among those insects. The mind rejects without discussing it the idea that either those birds are very large ones, much farther away than the insect cluster, or very small ones, much nearer than the insect cluster, but in either case so situated as by mere chance to seem to be among the insects. If we could feel any doubt on the subject all doubt would be removed if we noticed either that as the flight of insects travelled onwards the birds still remained apparently among them, or that there were several clouds of insects and that in each cloud there appeared to be a number of birds. We should be absolutely certain in either case that the birds were in the flight of insects, and we should thus be able to form an opinion as to the size of the birds, as to their relation to the insects, and so forth.

Now, though this may seem a very commonplace and simple illustration, such evidence is in reality as decisive as any amount of mathematical calculation, and especially any calculation based (as a great deal of the mathematical calculation which has most impressed mankind *has* been based) on mere opinion as to what may be. To an observer who had noticed the constant association of birds and insects just considered, and had thus been led to infer that they were small birds pursuing the insects for their food, no amount of learned talk about the ways and habits of some order of large birds, no amount of calculation as to the chance that such birds travelling in flights might seem to be immersed in the insect clouds, though not really so, would have or ought to have the least particle of effect. He would answer, and justly—I know that such and such small birds pursue and eat those insects, I see birds of the right apparent size among the insects, and though I cannot be sure that they are those very birds whose insect habits I know of, I am sure they are birds of about the same size and of the same ways, for there they are at their work: the chance may for aught I know be as the coefficient of $x^py^qz^r$ in the expansion of $(x+y+z)^n$ to the sum of all the coefficients in that expansion that those birds are wild ducks or geese or storks, really flying far above the insect clouds, and you may be quite right in your determination of the value of that chance; but your determination has no value whatever for me: your ducks and geese and storks are purely hypothetical, as are the habits you choose to assign to them to account for their forming into such groups as you suggest. But the birds I see are real and so are the insect clouds. The connection between the two sets of groups is certainly not accidental, and my explanation, which agrees with all the known facts, suffices for me, as it would for any one who bases his opinions on observed facts and reasoning thereon.

Sir W. Herschel, in the fine papers of 1811 and 1814, followed the simpler and more truly scientific way of dealing with his observations. In 1817 and 1818 he followed a plan which would have been most effective if it could have been based on actual observed facts, but in the absence of such facts was simply delusive. He started with an inquiry into the actual positions of the stars in space, and pointing out that in no single case (in his time) had a star's actual distance been determined, he adopted as a safe rule that—one with another—the stars of each succeeding magnitude beginning with the first are further from us than those of the magnitude immediately preceding.

The principle which seemed to Sir William Herschel so safe, is in reality altogether unsafe. It might be that all stars are — one with another — of the same order, all suns like our own, none very much larger and none very much smaller. But it might well be that they belong to different orders or classes, the stars of only *one* of these orders being akin to the sun. It might be (I am speaking always of what might have been, so far as Herschel knew) that in some regions of the stellar system one order of distribution prevailed, while in another the arrangement was entirely different. In either case,—and still more if both these peculiarities of stellar nature and distribution had to be considered,—the conclusions which Herschel might safely have drawn had the stars been all of one order and all similarly distributed, would not be true. As a mere matter of fact, it is now known that Sir William Herschel's fundamental conception was entirely erroneous: We know that there are several orders of suns, one order at least consisting of suns so much larger and mightier than he that they may be regarded as differing from him in kind rather than in degree. But the point to be noticed is that even at the time when Herschel advanced this view enough was known to suggest that it was incorrect, because inconsistent with analogy. The solar system, the next in order of scale below the system of stars, was known even then to consist of different orders of bodies, arranged in families. There was first the central ruling orb; then there were the giant planets Jupiter, Saturn, and Uranus, each with its family of dependent worlds. Then there were the four known asteroids, all travelling at nearly the same distance from the sun; and lastly there were the four terrestrial planets, Mercury, Venus, the Earth and Mars, as distinct in character from the asteroids on the one hand as from the giant planets on the other. In our day we know more respecting these chief divisions of the solar family, but our knowledge is of the same kind as Herschel's. To suppose in the face of such knowledge as he possessed that the next system in order of scale possesses no such variety of construction, but, on the contrary, is monotonously uniform, was not (as Sir W. Herschel seems in the closing part of his career as an inquirer to have imagined) to show extreme caution, but recklessly to run counter to all the available evidence. It may be said that now we *know* he was mistaken it is easy to point out why he ought to have thought differently; but as a matter of fact the evidence was clear and convincing; it had been noted ere as yet he had reached extreme old age by himself, and he had himself collected evidence which had forced him to recognise that the stars in one region of the stellar universe are very differently arranged from those in our neighbourhood.

Having overlooked in these later papers the circumstance that stars of certain orders are collected in certain regions of the stellar system must give very misleading evidence if they are regarded as of the same class and arranged in the same way as the stars forming our constellations, Sir W. Herschel was naturally led to erroneous conclusions in his new lines of inquiry. Any one who should start in his inquiry into our solar system, with no other evidence but the apparent positions and sizes of the planets of all orders,



The Day Sign for the Month.

would form an utterly erroneous idea as to the arrangement of these bodies. He would suppose Jupiter, Mars, Saturn, and Venus to be near, the asteriods to be far away, the whole system to be a flat disc in the midst of which our earth was set as one of a number of nearly equal and uniformly distributed bodies. He would be entirely mistaken, but not more mistaken than was Sir Wm. Herschel in the conclusion to which he was led in 1817 by a process of reasoning antecedently likely—one may say certain—to lead to an erroneous result.

Let us see what that reasoning was, and what was the result to which it led.

(*To be continued*).

THE THIRST CURE FOR CORPULENCE.

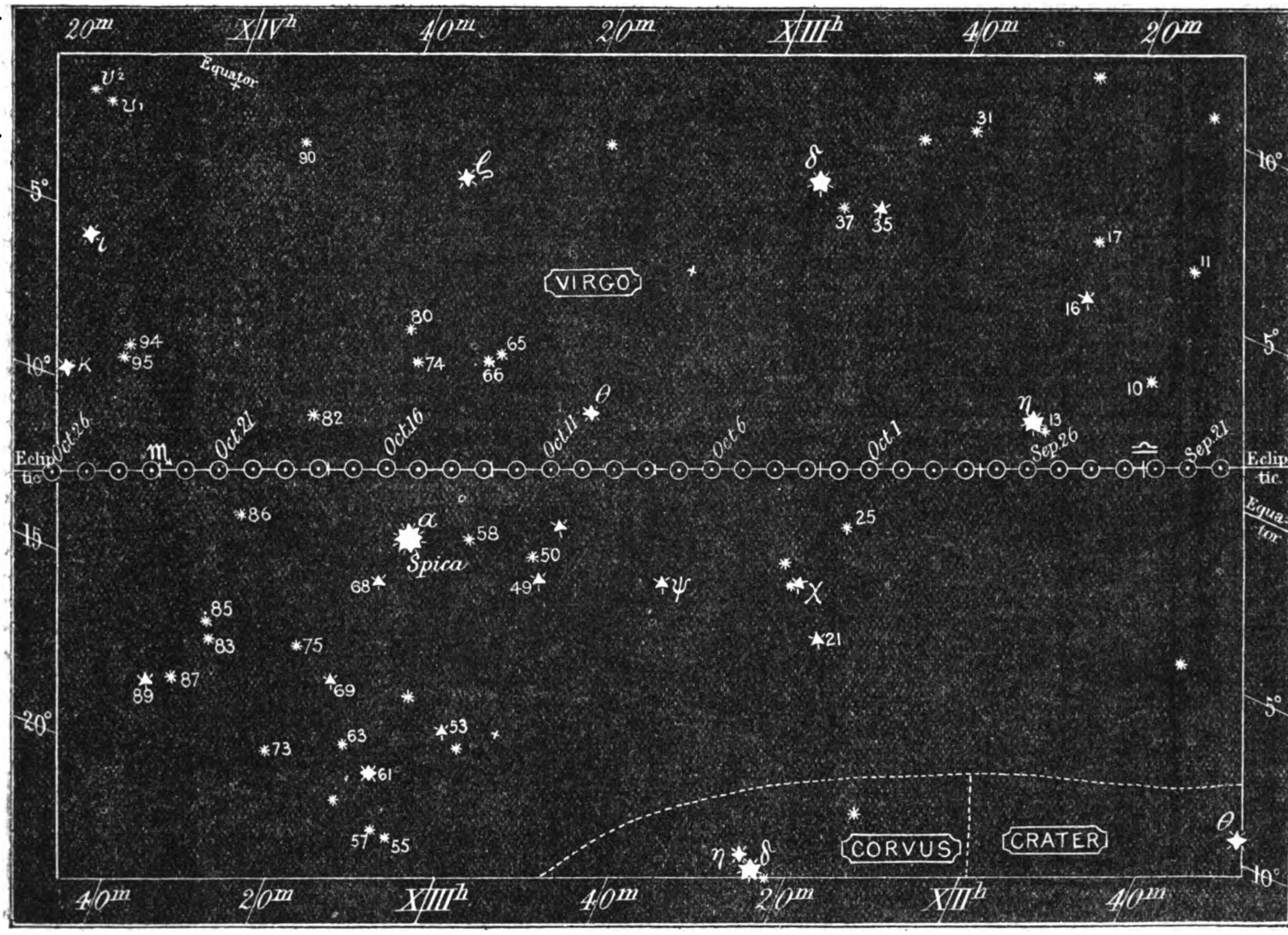
By Richard A. Proctor.

A CORRESPONDENT of the *Standard* has dwelt on the effectiveness of thirst in curing, or rather in preventing, corpulence. By taking but three half-pints of liquid daily he found his weight reduced a pound a week, and without any special restriction upon the amount of solid food. Moreover he found that he soon became accustomed to the reduction in the quantity of liquid food. A physician a few days later confirmed the experience of the former correspondent.

There can be no doubt that the method indicated is a sure and rapid cure for corpulence. But it is one which ought not to be adopted without medical sanction by persons of gouty diathesis, or probably by those having any constitutional tendency to excess either of acid or alkaline secretion. As to the gouty or lithic tendency I speak from experience. In the spring of 1868 I followed most successfully the thirst cure for corpulence. I was not, indeed, very corpulent at starting, though my weight —13 st. 4 lb.—was greatly in excess of that fairly due to my height (or shortness) and breadth. In the course of two months I reduced my weight to 11 st. 3 lb. But a constitutional tendency to a form of gouty trouble showed before that time most unpleasant readiness to increase and multiply tortures whereunto I strongly objected. Unfortunately, I did not for awhile recognise the connection between this result and the system by which I was so successfully reducing my weight. When I did, and resumed a more generous liquid treatment, I soon got rid of my lithic troubles.

In 1874, speaking with Dr. J. W. Draper on the subject of similar troubles experienced by him to a much greater degree, I mentioned my experience, showing what a valuable medicine water is. Although physicians are apt to regard lay suggestions with indifference, he noted my experience,—and when I met him in 1876 he told me he had enjoyed two years of freedom from the sufferings which had formerly been so severe.

It may be that by the use of appropriate medicine the thirst cure for corpulence may be made as safe as it is sure. For instance, a person of gouty tendency might take a



Night Sign for the Month.

certain small quantity daily of the bicarbonate of potass (or the bicarbonate of soda) in his pint and a half of water or whatever other beverage he took. But I venture to warn the obese not rashly to adopt the thirst remedy, lest haply, in their unwillingness to "bear those ills they have," they

"—— fly to others that they know not of."

ZODIACAL MAPS.

By Richard A. Proctor.

We give this week both the day sign and the night sign for the month, one showing the zodiacal sign now high in the heavens at midnight, the other showing the region of the zodiac athwart which the sun pursues his course at this part of the year.

ICHNEUMON FLIES.

By E. A. Butler.

(Continued from p. 245.)

Ichneumons do not confine their attacks to the Lepidoptera; some are parasitic upon beetles, some upon plant-lice, some upon flies, and some even upon other hymenopterous insects; they also extend the range of their enmities beyond the pale of the insect world, and, in some cases, live at the expense of spiders, whose eggs their grubs devour; still, the Lepidoptera are the group they principally affect. It must not be supposed that they are indiscriminate in their attacks; they will not take up with any caterpillar they come across, and very frequently each species of caterpillar has its own peculiar foe or foes, ever on the look out for it and ready to pounce upon it without a moment's warning. The elucidation of the life history of these creatures is not an easy task; the collectors of the Lepidoptera, in their efforts to rear their favourite insects from larvadom, are often baffled by the ichneumons, and in their disgust at getting a crop of uninteresting-looking "flies" instead of the handsome moths they had set their hearts upon, too often cast away the parasite as a despicable, worthless thing, and so lose the chance of solving a problem in nature. The determination of the species, moreover, is often an exceedingly difficult matter, so that it needs the combined efforts of a good Lepidopterist and a good Ichneumonologist before the matter can be settled—the former to determine what the caterpillar is, for, be it remembered, there is no chance of seeing the perfect moth, and the latter to decide on the ichneumon. Thus it happens that the hosts of many are still unknown.

Though so intensely carnivorous during their larval existence, these insects do not appear to have similar tastes in the adult state. They have, it is true, a good pair of jaws, but these they seem to have no great anxiety to use. If seized between finger and thumb, they do not attempt to bite, but wriggle their abdomen about in attempts, which are more often than not abortive, to wound with their ovipositor. Such food as they take seems to be confined to

the honey of flowers and other equally simple and harmless kinds of diet; hence many species are attracted to flowers, and the broad heads of the Umbelliferæ often form a pleasant foraging-ground for considerable numbers.

Many ichneumons are very active in their movements, and some, when running about over the leaves of plants in search of caterpillars, have a peculiar habit of rapidly vibrating their antennæ, as if trembling with excitement at the urgency of their business. Those with short ovipositors seek caterpillars that feed in exposed positions, where they can easily be reached, and such may, therefore, be found running over plants, &c.; but those with long ovipositors may be looked for on walls and trunks of trees, where they are on the look-out for caterpillars that shun publicity and lurk in obscure corners and in crevices of brickwork, or that burrow into the solid wood of the tree-trunks; hence the length of the ovipositor, which can be thrust down into burrows far too narrow for the ichneumons themselves to enter. In this way they can feel about till they find the object of their search, whose body is forthwith pierced and an egg inserted. Of course, those with long ovipositors, when intent only on their own delectation, and not engaged in oviposition, may be found on flowers with the other kinds.

There is often great disparity between the sexes; the males are sometimes so utterly unlike their partners as to have been referred not only to different species, but even to different genera. Thus one series have narrow-bodied, yellow-banded, long-winged, long-legged males and broader-bodied, red-banded, shorter winged, shorter-legged females. In some again, the males are winged, and the females absolutely apterous (Fig. 3), thus presenting a superficial resemblance to ants, from which, however, they may at once be distinguished by the fact of their antennæ not being elbowed, as those of ants are. White, yellow, and various shades of red, through deep browns to black, are almost the only colours with which they are adorned, and black, as a rule, forms a more or less important element in the coloration. The yellow is usually distributed in the form of bands on a black body, thus giving these slender creatures a supposed, but certainly very remote, resemblance to the heavy-bodied wasps. It is astonishing how frequently this style of ornamentation is met with among insects; besides the ichneumons, it occurs in bees, wasps, sawflies, flies, beetles, moths, and dragon-flies; but it is specially common amongst some sections of the Hymenoptera.

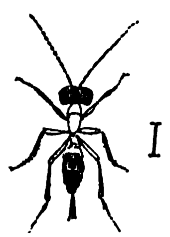


Fig. 3.—Pezomachus transfuga. Female.

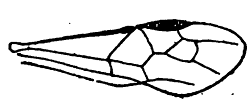


Fig. 4.—Forewing of Zele testaceator.

Some of the cocoons of ichneumons are marvels of elegance and neatness. They are of various hues—white, yellow, brown, black, and sometimes they are banded with different colours. They are almost always of an elongate oval shape. Sometimes the silk of which they are composed is so closely spun up together that the outside is almost as smooth as the inside, which is saying a great deal, and the whole has a paper-like consistency, the white ones having a lustre like satin; but, on the other hand, the outside sometimes has a certain amount of loose silk hanging about it like that of the silkworm, and occasionally whole clusters of cocoons are bound together in a common mass of silk. Some of these objects are probably familiar to most people, as groups of them are often found on walls towards the end of summer, enveloping the shrivelled carcasses of the caterpillars of the common white butterflies that work such havoc among cabbages.

There are two large families of Hymenoptera to which the name Ichneumon Fly is usually applied, though these do not by any means include the whole of the insects that have similar parasitic habits. These families are the Ichneumonidæ and the Braconidæ. They may be easily distinguished from one another—at least, those that are not apterous—by the neuration of the wings, which very constantly follows, with only slight variations, a distinct plan in each group. Fig. 4 represents the wing of a Braconid, and may be compared with the wing of an Ichneumonid shown at Fig. 1 in the former part of this paper. The species are unequally divided between these families, the Ichneumonidæ being by far the more numerous of the two. Many of the Braconidæ are very small, and it is to this family that the insect belongs which makes the clusters of tiny oval cocoons referred to above as forming a shroud for the mangled remains of the caterpillars of the cabbage butterflies. In escaping from their cocoons, these little creatures detach a small piece at one end which looks like a kind of lid, and generally remains hanging by some silk threads belonging to the outer surface.

TRICYCLES IN 1884.

By John Browning,

Chairman of the London Tricycle Club.

TRYING THE TANDEM.

It is necessary at the outset for me to say that I have purposely written trying *the* Tandem, and not trying *a* Tandem, or otherwise I should find myself accused within a week of not knowing that a Tandem had been invented previous to this particular machine. If this should seem ridiculous, I would ask attention to a paragraph of mine in another part of this number.

At the recent great Exhibition of Tricycles, at the Agricultural Hall, Mr. Cooper, of the renowned firm of Humber, Marriott, & Cooper, invited me to take a ride with him on the new "Humber Tandem," for the purpose of testing the machine.

I started with him at one o'clock in the day to ride forty miles. Rain had been falling heavily early in the morning, and the roads for the first few miles were covered with mud, and were heavy for several miles out.

The new Tandem is simply a "Humber" Tricycle, with a seat in front for a second rider to drive by means of central gearing—a pair of bicycle pedals. The tube which carries these pedals projects forwards, and carries a small safety-wheel, about 6 in. in diameter, under a pair of foot-rests. The safety-wheel is provided with a strong spiral spring, to break the shock if it touches the ground suddenly, and is arranged as a castor, so that if it touches when the machine is turning it instantly follows the line of the machine. I may at once state, however, that it never, throughout the day, touched the ground but once, and that was only on crossing a raised footpath across a very rough macadamised road, where the machine had to dip down and rise again; it touched here and spun round, but it rose again instantly, and did not in any way influence the action of the machine, or communicate any shock to me. Indeed, I should not have known it had touched the ground had I not seen it turning.

Throughout the ride I experienced none of the difficulty in keeping up a conversation with my fellow rider which I had anticipated in riding a tandem.

When passing over some villanous rough granite, running at a very rapid pace down hill, I said that my hat was getting loose. Thereupon Mr. Cooper bonneted me most effectually with a cheerful alacrity which gave me a high opinion of his presence of mind in any emergency.

As soon as we got well off from the rough granite of the metropolis, we ran easily, when unimpeded by traffic, at the rate of about ten miles an hour. As the machine was geared up to 54, the pedalling at this pace was not at all rapid, and unless the distance and time had been taken carefully, I should not have thought it possible that we were travelling so quickly.

From Brompton we ran to Kingston without stopping a pedal.

Just as we were getting nearly to the bottom of one of the hills after leaving Kingston, we saw two ladies walking down the middle of the road. Neither ringing a bell, whistling, nor shouting had any effect in attracting their attention to the fact that we were close upon them. When suddenly Mr. Cooper gave a cry close behind my ear to which the cry of a screech-owl would be music. Until I heard this cry I thought sounding the Otto horn the best possible method of waking people up who were inclined to go to sleep in the middle of the road, but it is not to be named in the same week with the sound of the human voice when properly pitched (?), as in the Cooper-screech. I cannot do more than attempt to describe this, but must content myself by saying that it combines the effect of a girl's shriek when she has discovered that her clothes are on fire and a dog's agonised yelp when his hind leg has been run over. I have actually seen it effective in waking a drunken carter, and should think it might wake the dead unless they were in double coffins.

At Kingston we noticed a racing bicycle outside a house. Having stopped and partaken of some sandwiches here, we proceeded on our way, improving the pace as we went on, and settling down to work together, but not attempting to force it. About three miles before we reached Ripley, we were overhauled by the owner of the racing bicycle we had left at Kingston. We had no idea he was near us until we saw him pass, for he travelled quite noiselessly. As he went by us we increased our pace, and as he had evidently, although a first-class rider, overdone himself in catching us, and was riding a machine too high for him, in trying to keep in front of us, he missed his pedal and came down heavily. He cut his hands rather severely, and damaged his face, though not seriously. We, of course, stopped and helped him up, waited till he had recovered himself, and then proceeded with him at a more leisurely pace on to Ripley.

All the way we had been riding against a tolerably strong south wind. After partaking of tea at Ripley, where our bicycling friend had his injuries attended to by a chemist, we returned to town, the bicyclist accompanying us. Two long rests, the attention he had had, a good tea, and half-an-hour's riding had refreshed him so much that, like a plucky Englishman, he challenged us to try our pace with him; but, both Mr. Cooper and myself, without consulting on the subject, made the same answer, that we did not come out to race, but only to test the machine and to try what pace it would go with easy riding.

The wind, which had been strongly against us while riding out, had fallen with the sun considerably, and shifted so as to be across our roads, so that we had little benefit from it in returning.

At Kingston our bicycling friend left us. Curious to relate, his machine, which only weighed 26 lb., and had a 53-in. wheel, did not receive the slightest injury in the fall.

In returning, we were catching up to a horse in a light cart. The sun was nearly setting behind us, casting long shadows on the road. Suddenly the horse reared, and then made as if he would bolt. "What are you about, you stupid brute?" said the driver; "there's nothing to shy at except your own shadow. God knows that's ugly enough; but *you* ought to have got used to it."

At Wimbledon Common we met two gentlemen who had just brought down one of the new Humber Tandems, and had not yet taken it to their home, and they expressed their great satisfaction with its easy running.

Time was taken carefully from place to place, and when we reached Humber's premises we found that we had ridden 40 miles in a trifle under 3¾ hours, by which it will be seen that we averaged nearly 11 miles per hour.

I was as cool when I got back as when I started. I had never been warm once throughout the day, let alone hot; and Mr. Cooper assured me he had never had so easy a ride to Ripley in his life. I never at any time did anything like as much work as I was capable of doing, except in riding up the steepest hills, for as soon as I attempted to work hard Mr. Cooper informed me it was unnecessary. I certainly did not ride anything like my best for more than five miles out of the forty.

The steering of the machine was marvellous. Mr. Cooper could evidently control it when going at a speed of 20 miles an hour down hills, so as to steer with perfect certainty within an inch; and while running at this great speed he let go both steering-handles, and placed both his hands on my shoulders, and kept them there for some little time, proving the tendency of the machine to run in a straight line without steering.

Several times I have run the risk of prophesying, in spite of the witty injuction to the contrary, with regard to the weight of machines, the price of machines, and the size of wheels, but I candidly admit that the last thing I should have prophesied would be that the "Humber" tricycle would make the best Tandem tricycle yet known, and I started with some prejudice against it. After my experience, however, I must say that I consider it, in good hands and with judicious management, the fastest, safest, and easiest machine yet made, combining, as it does, the comfort of a tricycle and the speed of a bicycle.

Under the skilful pilotage of Mr. Cooper the machine ran with less vibration than any other tricycle I have ridden considering the great pace we made. As we rode up the hills at the rate only of six or seven miles an hour at the utmost we must, over the best parts of the road where they were clear, have been travelling at something like the rate of fifteen miles an hour to have covered the whole distance in the time we did.

I did not enjoy this ride more but less for proceeding at such a high pace, though it was accomplished so easily. The day was exceedingly fine, and the views across Putney Heath and Wimbledon Common, the run by the riverside after passing through Kingston, and the lovely country from Esher past Claremont Park, and across Esher Common and Wisley Common made me long again and again to linger in passing through such exquisitely pretty scenery.

I place but little value upon the great speed of a machine as a question of speed, but it cannot be too clearly understood *that a machine which can be ridden at a great speed with but little exertion will go at a moderate pace with scarcely any exertion at all.*

The "Humber Tandem," can be confidently recommended to husbands who wish to take their better halves out

tricycling, yet desire to keep them in a proper state of subjection. As the lady cannot sit astride the backbone, she must take the front seat with her back to the gentleman, who has the sole control of the break and steering; and let him say what he will, she cannot, while the machine is travelling, resent it, for she cannot turn round on her seat with safety, and she is not likely to let go of either of the handles, and if she should in any way contrive to make herself unpleasant her partner has only to put his break on suddenly, without giving her any warning, and she would find herself landed on the ground in the twinkling of an eye, whether safely or not might be uncertain, and this uncertainty would doubtless induce her to avoid such a "little difficulty" again.

Two days after I had taken this ride Major Knox Holmes rode 101 miles with Mr. Cooper on the "Humber Tandem," in 12½ hours, including stoppages of 1 hour 45 minutes; riding time, 10 hours 45 minutes.

Starting from Croydon they rode to Brighton and back to Croydon, and then returned to Merstham, and went back to Croydon. Major Holmes, who is 78 years of age, was fresh and well at the finish. A wonderful machine and a still more wonderful man had come together.

THE EVOLUTION OF FLOWERS.

By Grant Allen.

VI.—TULIP AND FRITILLARY.

The simpler and earlier lilies, well represented by our English Gagea, have bunches of small flowers at the end of a tall stem; and familiar examples of this type are afforded us by the Star of Bethlehem and the common wild garlic. But the more advanced lilies, especially in northern climates, often bear a single larger flower only, the increased attractiveness of the big petals making up, apparently, for the diminution in the blossoms, according to the usual principle that every gain in effectiveness is accompanied by a corresponding loss in number. Of these higher lilies we may take as typical specimens the garden tulip, and the fritillary.

So far as regards structure, the tulip does not differ essentially from the little yellow Gagea. Its points of variation are all adaptive in the simplest degree, and have reference almost entirely to its increased attractions for the larger insects. The tulips, however, are altogether finer, heartier, and more successful plants than the little Gagea, and have consequently throughout a decidedly bolder and more succulent growth. They inhabit naturally richer soils, where they can spread themselves more freely to the sun and air. Hence their leaves are not usually narrow and grass-like, as is the case with Gagea, but broad and thick, or sometimes almost fleshy. Their bulbs, stems, and blossoms are also larger, and their appearance more generally prosperous. But the great distinction which marks them off from Gagea and its allies is the fact that their petals and sepals, instead of spreading out horizontally, so as to form a flat open flower, converge together into a bell, thus producing a sort of cup or goblet for the fertilising bees. Still, the sepals and petals remain distinct from one another, and do not actually unite into a single solid bell or tubular corolla, as in the garden hyacinth: that last advance in integration is one which we shall only meet on a somewhat higher level. In our garden tulips (varieties of *Tulipa gesneriana*), and in most other species, the flower stands upright on the top of the stalk, instead of turning downward, after the common fashion of tubular blossoms. This is a trick which it shares with a whole host of similar large handsome flowers, like the crocuses and colchicums: and everybody who has ever watched a bee at work among them, bustling about at the bottom of the deep enclosing well, must have observed how very effectually it conduces to the proper fertilisation of the stigma. Garden tulips have only one flower on each plant, but some few other species have occasionally two, a last relic of the large bunch produced by their lower relatives.

We have on a few Welsh mountain-tops some little colonies of a very interesting intermediate form—*Lloydia serotina*—which combines some features of the true tulips with some features of the simpler type represented by Gagea. This pretty little plant, a mountain and arctic lily widely spread over Europe, Asia, and America, is a tulip in all its technical characters, but bears a single small white flower with spreading and open petals, like those of wild garlic. It is, in short, a simple lily of the Gagea type, verging in the direction of a tulip, but still preserved for

Tulipa gesneriana (Garden Tulip).

us under its comparatively primitive shape in the extreme north or on the chilly mountain-tops; while its relatives in more favoured climates have developed under happier conditions, and by stress of more advanced insect selection, into the large and handsome Asiatic tulips.

Even more characteristic, in some ways, of the higher lily type, is the common English fritillary or snake's head (*Fritillaria meleagris*), which grows abundantly in swampy meadows about Oxford, and in many other parts in the southern and eastern counties. The fritillaries are a group of handsome bell-shaped blossoms, with their sepals and petals still quite distinct, but with a very large and well-marked nectary near the base of each, thus testifying at once to their attractiveness for the honey-sucking insects. Unlike the tulips, their flowers droop downward, so that the bee has to approach them from the under-side. Our English species has only one blossom to each stem, but some South European kinds have two, and the crown-imperial of our gardens has a whole cluster of them, thus rendering it exceptionally conspicuous and brilliant. The colour of these

flowers gives us an excellent lesson in the principles of floral colouration. Unlike the Gagea, which is primitive yellow, and the Lloydia, which is white with a yellow spot at the base (a great many other flowers have this same

Lloydia serotina.

intermediate arrangement) the English snake's-head is usually a dull lurid red, curiously marked inside with very remarkable chequered spots. Now, there is reason

Fritillaria meleagris.

to believe that the general progress of colouration in flowers runs from yellow, through white and pink, to red, purple, and finally blue. The snake's-head has thus got about half-way up the ladder; but, as often happens, every now and then it begins to tumble down again part of the distance, or even the whole of it. White fritillaries are intermixed with red ones in the Oxford meadows, and here and there one may even come across a yellow specimen. At the same time, it is interesting to note that in some species of fritillaries this relapse in colouration, which occurs only occasionally in our English snake's-head, has become normal or habitual, no doubt owing to some change in the particular insect visitor by whose aid they are usually fertilised. It is well known that different kinds of insects have very different tastes in the matter of colour, and they select accordingly those variations which best suit their own peculiar æsthetic ideas. But what is most important of all is the curious fact that most yellow fritillaries show unmistakable signs of having been descended from red chequered ancestors, just like our own snake's-head, for though yellow is their prevailing colour, they are loosely sprinkled over with reddish-brown marks, exactly similar to those of the English species. Moreover, in all cases that I have observed (for example, in the yellow *Fritillaria delphinensis* of the Riviera, and the *F. lutea* of the Caucasus) the nectaries and base of the petals are still red, as are also the edges in many instances. This clearly points back to an original red ancestor, and shows that the yellowness of the flowers is not like the primitive yellow of Gagea or of the buttercups, but an incomplete reversion from a higher stage. By far the greater number of fritillaries are purplish red, and the yellow ones always bear marks of having fallen from their high estate. Our own snake's-head is chiefly fertilised by bees, both hive-bees and bumble-bees, as well as by a few other insects. Its secretion of honey is exceptionally abundant.

It is curious to observe in this same connection that a wild yellow tulip (*Tulipa sylvestris*), not, perhaps, truly indigenous to Britain, has established itself in some of the eastern counties. The group, as a whole, is a southern one, and its prevailing colours are scarlet and pink, often with a dark purple or almost black base to the petals; but in this more northern European species there is the same sort of reversion to yellow as in the Caucasian fritillaries. As a rule, the yellow of such reverted flowers is paler and less golden than in the more primitive buttercup and potentilla types.

Royal Victoria Coffee Hall (Waterloo-road, S.E.).—On Tuesday, April 1, a lecture was given at the above hall, by E. B. Knobel, Esq., Sec. R.A.S., on "The Planets," to a large audience of working people. The lecturer divided the planets into groups, according to their physical characteristics. First, Mercury and Venus, where the absence of surface-markings prevents our knowing anything as to the length of their day or the successions of their seasons. Secondly, the Earth and Mars, which have nearly the same length of day and succession of seasons; while the likeness between them is increased by the presence of watery vapours, and probably of an ice-cap at the poles. Thirdly, Jupiter and Saturn, with their many satellites, and their ever-varying markings, which probably show that these planets are gaseous. Lastly, Uranus and Neptune, of which we know comparatively little, but which possess an interest all their own from the way in which the latter planet was discovered. The lecturer told, in some detail, how perturbations in the movements of Uranus led to the discovery of Neptune. It is gratifying to notice how the audiences increase as these lectures become better known.——During the next few weeks the following Penny Lectures will be delivered, on Tuesday evenings, at the Royal Victoria Coffee Hall, Waterloo-road:—April 22, Rev. P. H. Wicksteed, "Camping-out on the Thames." April 29, Sir Thomas Brassey, M.P., "A Visit in the *Sunbeam* to the West Indies." May 6, Dr. W. B. Carpenter, "Ice, and its Work in Earth-shaping." May 13, Mr. Vernon Boys, "Fire, Electricity, and Other Forms of Power." May 20, Prof. H. G. Seeley, "A Working Man's Dinner." May 27, Mr. J. Norman Lockyer, "The Recent Eruption of Krakatoa."

Reviews.

SOME BOOKS ON OUR TABLE.

Workshop Appliances. By C. P. B. Shelley. Sixth edition, revised and enlarged. (London: Longmans, Green, & Co. 1883.)—As an example of a handbook, indispensable alike to the professional and amateur mechanic, this work of Professor Shelley's leaves nothing whatever to be desired. Reading through its descriptions of tools, at once plain, perspicuous, and exhaustive, we find it hard to conceive that the implements of the handicraftsman could be rendered more intelligible, or their use more clearly explained, than they are in the work before us. The mechanical engineer, the turner, the fitter, the carpenter, and the joiner will each find here an illustrated account of every single tool he employs; both the theory of its construction and the mode of its use being made thoroughly clear. There is an excellent account, with numerous engravings, of Sir Joseph Whitworth's marvellous apparatus for measuring to the one-millionth part of an inch; while the 291 woodcuts which illustrate the book comprise at once objects as diverse as a bradawl and a planing-machine; a screw-cutting power lathe and a spokeshave. We have no difficulty in understanding why Mr. Shelley's work has reached a sixth edition. We are quite prepared to hear of its attaining a sixteenth.

Notes of Lessons on Moral Subjects. By F. W. Hackwood. (London: T. Nelson & Sons. 1883.)—In the year 1878 the Education Department issued a circular to her Majesty's Inspectors of Schools, insisting on the importance of affording moral training to children in elementary schools over and above the intellectual teaching which it is the primary object of such schools to furnish. It is to facilitate such instruction that Mr. Hackwood's book has been compiled. It consists of a series of lessons on such subjects as Honesty, Truthfulness, Candour, Honour, Benevolence, Courage, Integrity, and the like; partly consisting of question and answer, but mainly of oral exposition by the teacher, illustrated by anecdotes, poetry, &c. The chief fault that we find with this work is that it is much too highly pitched and "fine" for the class for which it is ostensibly intended. Sharp children who have passed the upper standards in large towns and cities may appreciate the sonorous periods and rounded sentences in which instruction is to be conveyed, but we tremble to think of its effect upon a schoolroom full of boys and girls in a purely agricultural district. As furnishing hints to the teacher, though, undoubtedly Mr. Hackwood's volume has its value.

Unclaimed Money. A handy book for Heirs at Law, Next of Kin, &c. By E. Preston. Seventh Thousand. (London: E. W. Allen.) Here is a book that might well furnish forth fifty of the three volume novels which give intellectual indigestion to Mr. Mudie's subscribers. Apart from the curious stories with which it is filled, however, it really does contain a considerable amount of information, valuable to those who are at a loss how to proceed for the recovery of money to which they are—or fancy themselves to be—entitled. We had no idea until we perused Mr. Preston's readable little book that there was so much unclaimed money (to use an expressive vulgarism) "kicking about" as there undoubtedly appears to be.

Vignettes from Invisible Life. By John Badcock, F.R.M.S. (London: Cassell & Company. 1883.)—Apparently modelled to a considerable extent upon the delightful "Marvels of Pond Life," by our own esteemed contributor, Mr. H. J. Slack, Mr. Badcock's book should form an instructive and acceptable present to the young microscopist; describing, as it does, prettily and popularly, the leading forms of Protozoa, with the Rotifera, Volvoces, Water-bears, Hydra, Sponges, and Diatomaceæ. The wood-cuts are, as a rule, really likenesses, and the beginner with the microscope may do worse than obtain "Vignettes from Invisible Life."

Manual of Taxidermy. By C. J. Maynard. (London: Trübner & Co. 1883.)—Everyone who has travelled abroad, to say nothing of those who have come across rare specimens of animated life at home, must have felt the desirability of possessing some knowledge of taxidermy, and of being able to preserve a bird, beast, fish, or reptile in a condition which should enable it to be subsequently studied with advantage. Many a superb beast-skin, many a beautiful bird, has been reduced to a species of scrubby rug, or an amorphous mass of feathers, for the want of some rudimentary acquaintance with the art of preserving it, on the part of its captor. We well remember our own maiden effort in this art, which took the form of an attempt to stuff a tern, with the result of the production of a species of feather sausage! Had we only had Mr. Maynard's capital little book at hand, we might at least have been spared so ignominious a failure as this. Beginning with birds, he teaches us how to trap, shoot, or procure them in other ways; how to skin them, stuff them, and mount them; and how to construct the stands on which they are placed. This section of the book is followed by one giving equally detailed instructions with regard to mammals; and it concludes with a chapter on mounting reptiles, batrachians, and fishes. It is a work which should be in the library of every practical naturalist.

Capital for Working Boys. By J. E. M'Conaughy. (London: Hodder & Stoughton, 1884.)—This is apparently an English republication of an American book, every local reference in it being drawn from persons and places in the United States. Its aim is a laudable one; but the hand of Mr. Chadband, or one of his congeners, is too apparent throughout, the "I say untoe you my brethren" element being painfully conspicuous. This is regrettable, in so far that the book does contain a good deal calculated to strengthen and encourage lads who have to make their way, unaided, in the world, but who may well be repelled by the hyper-sanctimonious tone in which they are here addressed. We protest altogether against the dicta of the author regarding the theatre on pp. 56 *et seq.* It contains just that extremely small modicum of truth which gives the most dangerous form to a lie. There is, at the very least, as much evil and harm to be got in many a chapel as there is in an indifferently-conducted theatre; while it is much to be desired that as sound moral training (to say nothing of purely intellectual gratification) were derivable from a majority of our pulpits as may be obtained in a high-class one. So, again, with reference to the question of Sunday (or as Mr. M'Conaughy calls it, "the Sabbath"), treated in our author's XXIInd Chapter. What may be the case in the United States we cannot presume to affirm; but that opening museums and picture-galleries in England on that day would lead to the gradual introduction of Sunday labour, it is simply childish to assert. The trade unions alone will take exceedingly good care that that does not happen. Possibly, too, our author would fail to see how unwittingly he is playing into the hands of Messrs. Aveling, Bradlaugh, Foote, and Ramsey by his XXVth Chapter. And we should like to have the candid opinion of our own contributor, "Five of Clubs," as to the denunciation of cards which appears on pp. 98, &c. *Malgré* the faults at which we have hinted, this would not make a bad Sunday-school prize.

Editorial Gossip.

IN the *Evening Journal* of Adelaide, for March 8, 1884, a writer takes me to task for what he regards as an error in my solution of the problem of the flight of a projectile, Vol. 4, pp. 10, 11. I there assert that the projectile throughout its flight describes equal areas around the earth's centre. He denies this, saying that the velocity remains unchanged while the distance from the centre increases, therefore the areas described must increase too. He seems to suppose that it was a mere fad of mine to apply Kepler's second law to a projectile. But in reality the law observed by Kepler to hold in the case of a planet is part of a more general law, established by Newton, in the *Principia*, book I., sect. II., prop. I., in which he establishes the theorem that *The areas which revolving bodies describe by radii drawn to an immovable centre of force do lie in the same immovable planes, and are proportional to the times in which they are described.* The mistake made by my critic lies in overlooking the change in the angle at which the projectile's path is inclined to the moving line from the earth's centre to the projectile. It would not be easy properly to follow out the changes of inclination: by using the general law I avoided the difficulty.

I MAY presently take up the discussion of the more general problem of the flight of a missile projected in any direction with any velocity and from a point in any latitude. But I shall only sketch the conditions of the problem, not being likely to have time to follow it out in all its details.

THE mistake involved in the reasoning of the Adelaide writer is the same which one meets with in regard to the way in which a sportsman aims at a bird passing athwart the line of fire. It is quite commonly supposed that if such a sportsman so turns that while he is aiming he keeps the barrel constantly directed towards the bird (with due correction for elevation) he will hit the bird when he fires. As a matter of fact the main part of the charge would in that case pass behind the bird, and unless the charge spread sufficiently the bird would be missed.

THE four following notes are from an article of mine in the *Newcastle Weekly Chronicle*:—

MR. WILLIAM LANT CARPENTER, in a lecture recently delivered to a large audience of working men, illustrated his remarks on the mischievous effects of tight-lacing by showing first a series of photographs of Parisian fashions, and then views of the Venus of Milo and Miss Mary Anderson as Galatea. Probably those aware of the tastes of American ladies with regard to figure may be surprised to find that an American should be selected to exemplify the beauty of a natural as compared with an artificially contracted waist. But Miss Anderson appears to be as sensible as she is beautiful; and assuredly she loses nothing by allowing herself the breathing-room which nature gives to women.

WE are told repeatedly that there must be something beautiful and attractive in the Parisian waists, seeing that so many women aim at their attainment, while masculine folk apparently admire the fair ones thus distorted. The argument might be used with equal effect in favour of any one of the multitudinous absurdities which fashion has framed. The costumes of a quarter of a century ago were manifestly absurd, and men and women alike look with astonishment at the spoon bonnet, the wide expanse of crinoline, and the other monstrosities of the fashions of 1859-1860. But in those days, while the fashions of half a century ago were similarly ridiculed, women nearly all wore the ridiculous costume which chanced to be in vogue. Nor did men turn from them in disdain on that account, however thoroughly some of them saw the absurdity of the costume. If nine-tenths of the women chose to wear rings through their nostrils, or to adopt the costume of the Andaman Isles, they would find little in the ways of the younger at least among the men-folk to suggest that these were tricks worthy only of savagery.

THE argument, in fine, drawn from common consent among women, and apparently general acceptance among men, has no validity whatsoever. It had as much application to crinolines and spoonbills as to tiebacks and panniers; or, in old times, to waists not half a foot from the neck; or, in times still older, to waists three feet long. But breathing is an older fashion, and, we may fairly trust, is likely to prove a longer lasting fashion than these absurdities. And seeing that what is worth doing at all is worth doing well, the fair sex may reasonably learn the lesson that a woman may look her best without depriving herself of half her breathing power as the tight-lacers do, or even of a third of that power, as it has been proved that even those do who lace but lightly.

IF the graceful lady who has been one of the chief ornaments of the English stage for some time past should have helped to teach this lesson, it will be a useful addition to the comforting doctrine she has helped to teach, that an actress may charm an audience by sheer force of grace and dignity, and obtain a greater influence by purity and innocence than has been attained even by the cleverest and most beautiful women who have trusted in bold manners and brazen bearing.

AT the request of our esteemed contributor, Miss Amelia B. Edwards, we copy the enclosed from the *Academy*:—

TO THE COMPASSIONATE.—Help is implored for a lady who is dangerously ill and absolutely destitute; daughter of a deceased Colonel in the U.S. Army and correspondent of the leading English newspapers.—Subscriptions received by Miss AMELIA B. EDWARDS, The Larches, Westbury-on-Trym, Bristol.

LAST week one of my kindly critics (only one would have condescended to make such a statement) informed his readers that I did not know the difference between a central-geared front-steering machine with a double-driving arrangement or balance gear, and a rear-steerer with a hay-fork frame without a double-driving arrangement. In other words, I was accused of praising and censuring the Sparkbrook National in the same paragraph. I need scarcely say that I should not have praised that machine so highly had I not considered it one of the safest machines made. The facts of the case are as follows:—A few weeks since a subscriber to KNOWLEDGE asked my opinion of the Sparkbrook National tricycle, adding that an agent had told him it could be ridden up a flight of stairs. I replied "that the central-geared Sparkbrook National was a good machine, but that I should not value a machine any more because it could be driven up a flight of stairs, because it might be useless, or even dangerous for any other purpose." Having set myself right to this extent, if my readers will excuse me for doing so, I will promise them not to pay any regard to such matters for the future, but to treat them with as much respect as they deserve, but no more.—JOHN BROWNING.

THE FACE OF THE SKY.

From April 25 to May 9.

By F.R.A.S.

THE usual watch should be daily kept upon the sun for spots, &c. Map IV. of "The Stars in their Seasons" gives the present aspect of the night sky. Mercury is an evening star during the whole of the next fortnight, attaining his greatest elongation east of the sun (20° 21′) at 2 o'clock this afternoon. Towards 9 p.m. he may be seen with the naked eye over the W.N.W. part of the horizon. He is travelling through Taurus south of the Pleiades ("The Stars in their Seasons," Map I.). Venus is also an evening star, and attains her greatest eastern elongation (45° 27′) at 6 o'clock in the evening on May 2. It is getting on towards midnight ere she sets close to the N.W. point of the horizon. She is a most brilliant and conspicuous object. She is travelling from Taurus into Gemini (same Map). Mars looks simply like a big red star. His position may be found in the Zodiacal Map on p. 70. Jupiter must be looked for as soon as ever it is dark enough, as he is approaching the west. He is, as shown by the Zodiacal Map on p. 40, in Cancer. During the next fourteen days the following phenomena of his Satellites will be visible. To-night (the 25th) the egress of Satellite I. will occur at 8h. 52m., as will that of the shadow it casts at 10h. 9m. p.m. On the 28th Satellite III. will reappear from occultation at 10h. 49m.; only, however, to suffer subsequent eclipse at 12h. 23m. 8s. The transit of Satellite II. will begin at 10 o'clock on the night of the 30th. The observation of the ingress of its shadow 28 minutes after midnight is problematical. On May 1 Satellite I. will be occulted at 11h. 12m. p.m., and the transit of Satellite IV. commence 12 minutes after midnight. This last phenomenon should be watched if possible, as the satellite will probably cross Jupiter's face as a dark spot, like its own shadow. On the 2nd the transit of Satellite I. begins at 8h. 29m. p.m.; and that of its shadow at 9h. 44m. Satellite II. will reappear from eclipse at 10h. 9m. 13s.; Satellite I. pass off Jupiter's face at 10h. 49m.; and its shadow follow it at 4 minutes after midnight. On the 3rd Satellite I. will reappear from eclipse at 9h. 13m. 53s. p.m. Satellite III. will be occulted at 11h. 23m. on the night of the 5th. On May 9th the egress of the shadow of this same satellite (III.) will happen at 9h. 59m. Satellite I. will begin its transit at 10h. 26m., and be followed by its shadow at 11h. 38m. p.m. Saturn is even worse placed than Jupiter, and is rapidly leaving us for the season. He is in Taurus, and will be nearly due North of Aldebaran when these notes begin. Uranus continues to be fairly well placed for the observer. He is in Virgo, where his path may be traced on the Zodiacal Map on p. 165. Neptune is lost in the glare of sunlight. The moon is 29·3 days old at noon to-day, 0·9 day old to-morrow, and obviously 13·9 days old at the same hour on May 9. Three Stars will be occulted by the Moon during the next fortnight. The first is ω Leonis, a 6th magnitude one, which, on the evening of May 2, will disappear at the Moon's dark limb at 7h. 43m. at an angle of 37° from her vertex, to reappear at her bright limb at 8h. 36m. p.m. at a vertical angle of 318°. Then on the 8th, λ Virginis of the 4½th magnitude will disappear at the dark limb of the moon at 9h. 12m., at an angle of 79° from her vertex, reappearing at her bright limb at 10h. 24m. p.m., at an angle from her vertex of 213°. Lastly, on May 9, the 6th magnitude star ν^1 Libræ will be occulted at 9h. 16m. at a vertical angle of 350°. The disappearance will really take place at the dark limb, but the moon will be so nearly full that it will look as though the star vanished behind a bright one. The reappearance at the bright limb happens at a point 291° from the moon's vertex, at 9h. 57m. p.m. The moon is in Aries until three o'clock to-morrow afternoon, when she enters Taurus; and it takes her until midnight on the 28th to cross the last-named constellation. She then passes into the extreme northern part of Orion. Her passage through this occupies eleven hours, after the lapse of which she crosses into Gemini. She is travelling through Gemini until two o'clock in the morning of May 1, at which hour she enters Cancer. She quits Cancer for Leo at 4 p.m. on the 2nd, and four-and twenty hours later descends into Sextans; whence she re-emerges into Leo at 11 a.m. on the 4th. She enters Virgo at 1 p.m. on the 5th. She takes until 11 o'clock on the night of the 8th to pass through this great constellation, and at that hour crosses the boundary into Libra. She is still travelling through Libra when our notes terminate.

According to a paper by Dr. N. H. Stone, in *Nature*, the resistance of the human body with a current of 3·6 volts, as found by experiments with a few patients, may be roughly given as from 1,200 to 1,500 ohms from foot to foot or from foot to hand, contact being made by means of brine baths.

"Let Knowledge grow from more to more."—Alfred Tennyson.

Only a small proportion of Letters received can possibly be inserted. Correspondents must not be offended, therefore, should their letters not appear.

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THE BACH INCIDENT AND MR. OWEN.

[1199]—In your paper of April 4 appears one of a series of articles. The one in question refers to the Bach incident, published by my father, Robert Dale Owen, in his "Debatable Land between this World and the Next." Will you be good enough to give me space in your paper to meet, if possible, a few of the objections which you present?

You kindly say "that the converts should not be ridiculed," hence I feel prompted to reply to so fair an opponent—a fairness with which spiritualists do not always meet.

I was with my father a portion of the time while he was gathering the material for his books, and I can therefore testify that no one could use more indefatigable care in collecting facts. He sometimes spent days in verifying the most insignificant details, travelling from place to place, and sparing neither time nor trouble. I have never known a lawyer so conscientiously discriminating in collecting evidence as was my father. If human testimony may be trusted in a court of law, then surely it should be received when thus carefully gathered in the interests of a higher tribunal.

In many instances, lawyers, jury, and witnesses may be bribed; but in my father's case, and in that of many others, there was no temptation to prevaricate. Spiritualism is not so popular that a man would in any way perjure himself to be ranked among its converts. I can well remember when my father decided to publish the vast evidence collected through years of investigation, continued first through months in his own drawing-room, and afterwards in a more extended field. He had an established reputation as a statesman in America; he was Minister to Naples when he first accepted the fact of spirit communion, and he thought that through the avowal of his belief he would lose the high position which had been won through years of public duty.

Even at this present time it requires some courage to acknowledge oneself a spiritualist, but twenty-five years ago it required much more.

One great difficulty encountered by my father was to induce people to communicate their experience at all; they feared to incur the world's contempt, and in many cases obtained it for their avowals.

Under such circumstances witnesses, as a rule, do not manufacture evidence. I do not claim that there have not been some hysterical and morbidly imaginative people whose testimony is not trustworthy, for there are many such both within and without the ranks of spiritualists; but if human evidence taken *en masse* can ever be trusted, it is when such evidence is given at a personal sacrifice to the witnesses. No one likes to be laughed at, and spiritualists share this trial with the rest of humanity.

I quite agree with you, however, that no one should believe "such stories" until he has investigated for himself, but neither should he disbelieve them until he has so investigated.

There are a sufficient number of sound-minded men and women in the ranks of spiritualism at the present time to warrant respect, and to demand from other sound-minded men and women who have not investigated its phenomena an unprejudiced neutrality.

If you will give a little further thought to the subject, you may be inclined to reconsider the sentence in which you say, "It ought to be shown that the well-being of the human race is to some important degree concerned in this matter . . . . before the philosophers can be expected to devote much of their time to the inquiry

suggested." Does it require any argument to show that the question of a future life is a subject of sufficient interest to engage the attention of even the most profound philosopher? And can there be any way of proving this all-important truth except as we can do so by communicating with those who have passed through the death of the body and yet live?

It is often the painful duty of spiritualists to answer appeals from death-robbed unbelievers, whose philosophy is unable to give them comfort when suffering a heavy loss. A negation may suffice as a neutral background to positive well-being and happiness, but a negation has never filled a vital human want. Even within a few weeks I have been grieved for sceptical friends, who, in happier moments, believed their theories offered a safe refuge in time of need. One comes sorely to dread unbelief when one sees, as I have, the suffering of bereaved unbelievers.

Concerning your objection as to the triviality of the messages, I have found through a life-long experience that many of the thoughts suggested are not trivial. Believing that the soul passes into the life beyond in the same state that it passed out of this existence, we do not wonder that some of the communications are unimportant; we unfortunately find in this world many men and women who are not profound either in thought or feeling; and these shallow streams are as noisily obtrusive on that side as on this? Is it not a wise dispensation that this is the law? Growth cannot be an attribute of the infallible; were we, therefore, to have free intercommunion with angels, whose judgment in all things was perfect, and we were, therefore, not called upon to use our own reason and discrimination, would not such communion lift us by artificial means to an exalted position which we had in no wise won by that individual effort, that experimental progression, which only can give us full-rounded moral muscle? Man is not prepared as yet for plenary inspiration.

Further, if we are to reject a belief in intercommunion between this world and the next because those who communicate are not perfect, then we must also reject humanity for the same reason. But we can scarcely affirm that there are no human beings because some of them are foolish or wicked, neither would it be wise to isolate our lives because our fellow creatures are not quite satisfactory.

To a philosopher such imperfection should not be a stumbling-block, but rather a test of genuineness, for philosophers should realise more fully than less careful students, how slow is the progression of any growth, how gradual and consecutive are the up-building processes even in the physical world. The law makes no leap.

Were spiritualists to claim that a murderer becomes an angel in the twinkling of an eye, then the philosophers might object to investigating a theory so contrary to their own experience, but we make no such claim; we know that we are all interlinked in a great progressive scheme wherein there are innumerable shades of differing growths, we feel that we must have an infinite patience with lower spirits, even as higher spirits have an infinite patience with us.

If you will give your attention to the study of the laws which bind the worlds of matter and spirit, you will find, I think, a wide and subtly interesting field of research, and I venture to affirm that you will discover it to be a subject, not only worthy of philosophers, but one requiring the most conscientious search of the greatest hearts, the most continued thought of the wisest brains in the world. ROSAMOND DALE OWEN.

[I am myself unable to understand why any one who possesses evidence apparently substantiating spiritual or ghostly theories should hesitate to produce it. Holding it back implies doubt as to its validity. That my correspondent's father was not among those who sought to deceive, all who knew him know. But that he was not deceived is by no means so well attested. I do not wish spiritualistic notions to be discussed in these columns, for the simple reason that so much deception has been attempted and detected. But well attested facts are always admissible, as they have a scientific interest:—first, because there must be *some* explanation, and the search for an explanation of observed facts is good practice; secondly, because by noting the facts on which erroneous opinions have been based we learn how to gauge the average intellect.—R. P.]

APPARITIONS.

[1200]—Mr. Rudd's suggestion that apparitions (*vivid dreams*) may be due to one hemisphere of the brain being in the state of sleep and dreaming, while the other performs functions for the man consciously awake, has, as he admits possible, been made before.

Many may have entertained the same idea. Mr. Sergeant Cox, in "The Mechanism of Man," Vol. 2, 1879, where he devotes sixty pages to the subject of sleep and dreams, adopts almost identically the same, namely that one hemisphere is awake, the other asleep See pages 106, 128, and 129 of his book.

In the first edition of his work, published in 1874, he expresses the same opinion. EYE WITNESS.

VISIONS.

[1201]—In my letter published in KNOWLEDGE (1147, of March 14) I related singular visual experiences which I had had, extending over a period of fourteen days. These continued for at least a week afterwards, but have since entirely ceased, owing, as I imagine, to their cause being removed; and as this may have some interest for men of science and opticians, I venture to ask for space to relate a suggestion as to their origin which has since occurred to me.

During the three weeks in which I was subject to seeing curious scintilations of light in the hours of darkness I was engaged each evening till bedtime in searching out and comparing certain passages in a badly-printed book of small type, and though I am not in the habit of wearing glasses, I found it convenient to use occasionally a magnifying glass of strong power in this work. The light of a lamp passing through this frequently caused bright rays of light to rest upon the book; these did not at the time cause me any inconvenience; but it has struck me that they may have become, as it were, photographed on the retina of the eye, and have been reproduced as I there described; and I suppose, therefore, that the scintillations I spoke of were not due to some mere subjective cause, "or of internal origination" within the optic ganglia, as Mr. Wilson, in his letter 1174 of KNOWLEDGE, April 4, appears to think was probably the case. COSMOPOLITAN.

COINCIDENCES.

[1202]—Here is a trivial example of a coincidence which may not be absolutely without interest as an illustration. In the *Standard* for the 7th inst. I read this paragraph:—"The name of the midshipman whose gallant conduct, in connection with the death of Lieutenant Montressor, was mentioned by our Souakim Correspondent, was Tyndale-Biscoe." I never, to my knowledge, met with the uncommon double-barrelled name of this plucky young officer before; but, taking up the paper on the succeeding morning (the 8th), I find another Mr. Tyndale-Biscoe figuring as the coxswain of the winning boat in the University Boat Race.

[Here is another odd little coincidence—I write a letter to my agent Mr. J. Stuart, and being interrupted while addressing it, write only his name and the words "Royal Conc." The letter is posted in Kew. One would say it was bound to come back to me through the dead letter office. Not at all. My wife chances to call the same evening on a lady of the same name as my agent. The letter is brought in while she is there, is recognised by her as in my handwriting, and is forwarded unopened by the same post which would have taken it had it been rightly addressed.

"No, sir, I am *not* in the habit of wrongly or imperfectly addressing letters. I never made such a mistake as this before to my knowledge."—R. P.]

[1203]—The following curious case of coincidence occurred many years ago, when my family resided near Keswick, in the Lake District, I being a boy at the time. I was walking one day with a friend along the road to Newlands, when our conversation turned on flowers. I remarked to him how curious it was that in the whole neighbourhood—say within a radius of at least eight or ten miles—there were no wild cowslips. Without being a bit of a botanist, I was always fond of gathering wild flowers, and there were few showy ones that I did not know. I therefore considered my never having, during many years' residence in the place, observed a cowslip out of a garden, fair evidence that the plant did not exist in a wild state. Not more than a minute elapsed from the time I spoke, when, happening to glance down on the road side, I saw a cowslip. Now, I have been in the same neighbourhood a great deal since that day, but I have never *observed* another such flower. How it came to be growing where it was (near a side-road which leads to Nicol Ending boat-landing) it would be fruitless to inquire; it may have been planted by a bird, or a child from Portinscale, a neighbouring village (where there were gardens), may have planted it. No doubt the cause of its presence was most simple. But still there remains the curious coincidence of my speaking of a rare object and seeing it not more than a minute later.

I. R. CAMPBELL.

ORTHOPIC.

[1204]—Many thanks for courteous correction of errata (1190, April 11, 1884, p. 252).

In reply to question as to difference between the terminations "scopic" and "opic," I may be allowed to say that, in scientific

nomenclature, all instruments made to assist vision (except, I think, spectacles, and in those we have peri-scopic) are indicated by names ending in "scope" (from σκοπέω, I view), as micro-scope, tele-scope, stereo-scope, spectro-scope, pseudo-scope, &c. The corresponding adjectival forms of which, applicable to their respective phenomena, are micro-scopic, tele-scopic, &c.

But vision with naked eye is indicated by opia, from ὤψ, an eye; hence, short-sightedness is called my-opia; far-sightedness (or rather aged-sightedness), presby-opia; double-seeing, bi-opia; correct seeing, orth-opia (from ορθος, correct); and false-seeing, pseud-opia (from ψεῦδος, falsehood); and *their* corresponding adjectival forms, applicable to unaided vision, are my-opic, presby-opic, bi-opic, orth-opic, and pseud-opic. That "orthopic" and "pseudopic" do not involve any physical "impossibility" is manifest from "Cosmopolitan's" original letter (1147).

The whole subject is very fully discussed in a charming book, "Vision, Pseudopia," by Edward H. Clarke, M.D., edited by Oliver Wendell Holmes, M.D., published by Houghton, Osgood, & Co., Boston, U.S. WILLIAM WILSON, M.A., LL.D.

[The words "orthopic," "pseudopic," &c., seem to me open to serious objection. "Myopic" and "presbyopic" are all right, because the word compounded with "op" really characterises the "eye,"—"closed-eye" and "aged eye" we understand, but false-eye and right-eye have no proper relation to false seeing and right seeing. A myopic is one who sees with half-closed eyes as a short-sighted person does, a presbyopic one who sees with aged eyes; but a pseudopic is not one who sees with false eyes. So at least it seems to me.—R. P.]

THE "NATIONAL" TRICYCLES.

[1205]—As Mr. Browning's errors of last week seem likely to damage a very trustworthy firm, will you permit me to correct them?

The "Sparkbrook National" is constructed by the Sparkbrook Manufacturing Company, Sparkbrook Works, Coventry. It is a central, or side-geared front-steerer, of first-class construction, and its makers do *not* claim that it may be driven up a flight of stairs.

The stair-climbing machine is a "Royal National," made by the National Cycle Company, also of Coventry. It is a direct-action machine, and *cannot* be central-geared. Mr. B. has confounded the two makes. GALLUS.

P.S. The accident alluded to by Mr. B. was that which resulted in the death of Mr. A. A. Broad. He fell off a "Royal National" direct-action stair-climbing machine down-hill, and the jury described the brake as ineffective.

A TRICYCLE PROBLEM.

[1206]—"D. M." asks, in letter 1189, why a tricycle wheel leaves a double rut in dust. I have watched the production of the phenomenon referred to long before the invention of tricycles. This little ridge of dust in the middle of the wheel-track is piled by the air rushing from each side into the wake of the wheel. It is the upward motion of the hinder part of the tire which gives rise to the effect. M. H. C.

IMPRESSION OF TRICYCLE TYRES.

[1207]—I have frequently observed the impressions left by the wheels of a tricycle, as referred to by "D. M." in last week's KNOWLEDGE. I think the peculiar form of the impression is simply due to the alternate expansion and contraction of the rubber tyres. When the tyre is revolving on a dusty road, that part in actual contact with the road (bearing the greatest weight) is flattened, and therefore leaves a depression rather wider than the tyre. The ridge in the centre is caused by the contraction of the tyre immediately after it has passed its point of contact with the road. This contraction of the rubber (from either side of the depression) draws the dust to the centre of the tyre, and thus forms a ridge in the centre of the depression, so that, as long as the wheel revolved, there would be continual expansion and contraction, causing trough and ridge.

The absence of the ridge from a trough made by a tyre in mud might be due to one of two causes: either the mud sticks to the tyre and obliterates the evidence of contraction, or, the mud may be too tenacious to be drawn to the centre of the trough and "ridged" by the contracting power.

I have made no experiments to test the accuracy of my opinion, although I have held the same for some years.

On first noticing it I attributed it in some way or another to the air under the tyre, at the point of contact, being expelled, and rushing in again from behind to fill the vacuum, carrying a certain amount of dust with it, which, when deposited, would form a ridge.

Interesting experiments of this kind can be tried, and "dendritic" markings be artificially produced. If two pieces of smooth slate be wetted and rubbed together until they become sticky, and then suddenly drawn asunder, the mud produced by the attrition will assume the form of tree, fern, sea-weed, or something "dendritic."

I have often observed the same effect produced by a "dry process" when walking along a dusty pavement on a still day, for when the sole of the boot is lifted from the pavement in walking, dendritic markings will be formed in the dust impressed by the foot. The finer the dust the more perfect are the markings. That ancient fossil, the *Oldhamia radiata* of the Cambrian rocks (supposed to represent a kind of sea-weed) is believed by *some* geologists to be nothing more than a "marking," caused, probably, by one of the processes aboved-named, and thought to be so because a weight suddenly lifted from a piece of tissue-paper will produce markings not unlike *Oldhamia*. C. CARUS-WILSON, F.G.S., F.R.G.S.

"TWINKLE, TWINKLE LITTLE STAR."

[1208]—*Quandoque bonus dormitat Homerus.* "F.R.A.S," in his valuable essay on "Scintillation," infers that the famous "Twinkle, Twinkle Little Star," &c., was written by Dr. Watts. Not so; it was the Sisters Taylor (Jane and Ann) who, in their "Original Poems for Infant Minds" (published fifty-five years after the death of the pious doctor), first gave to the world this graceful little poem. The parodied lines commence "Scintillate, scintillate," and so forth. J. W. HOWELL.

NOTES OF SPECTROSCOPIC OBSERVATIONS OF COMET "PONS," JAN. 27 TO FEB. 2, 1884.

[1209]—The spectroscope used was a small direct-vision compound prism, by Browning, the telescope being a refractor of 3-in. aperture. After some difficulty in getting the object focussed upon the slit of the spectroscope, there flashed out three bright bands. They appeared somewhat pyramidal in form, the base being on the south side of the telescopic image. The relative spaces between them appeared about as 2 to 3. I could not distinguish any difference, or even any trace, of colour; they appeared rather to resemble a phosphorescent glow. I had not the means at the time of determining their relative positions in the spectrum, having left the reflecting prism at home. On subsequent evenings, however, I took every precaution for determining this point.

On the evening of January 29th, I succeeded in getting the spectrum of a gas-flame (common coal-gas) turned down to a minute point of blue flame, in juxtaposition with the spectrum of the comet, when, to my surprise and gratification, I found that the three comet lines coincided perfectly with the three conspicuous lines shown in the gas spectrum, the principal difference between the two spectra being that the gas showed a faint continuous spectrum, showing all the colours, whilst that of the comet had perfectly dark spaces between the lines, and, so far as I could discern, no colour. I cannot say, however, that I might not have obtained a continuous spectrum from the comet had I been able to grasp more of its light. Such seemed to be suggested by the fact that the lines were broadened out towards the violet end, gradually fading away on that side, but were pretty sharp and decided on the side next the red.

By a contrivance of my own, designed specially for double star measurement, I was enabled to project a dark-field "ghost" scale into the field of the spectroscope, and thereby got the relative positions of the lines from the sodium line, D. This line I obtained by sprinkling a little salt in the gas-flame. The mean of several observations gave the following results, the readings of my scale being reduced to that of Roscoe's frontispiece for comparison. The line D being at 50, the comet lines stood at 59·3, 72·2, and 99·5. Roscoe's carbon lines stand at 60, 76, and 100; also another group at 123 to 128. This latter, however, I did not detect. I think this agreement very close, and what little difference there is may well be accounted for by a difference of material of the prisms and the difficulty of measuring. I may mention that I took the precaution of scaling the principal solar lines during the preceding afternoon, for the purpose of comparison.

It was evidently chiefly, if not entirely, the *nucleus* that gave the lines; as, on the briefest stoppage of the driving-clock, the lines instantly disappeared. A. B. BIGGS.

THE GOODWIN SANDS.

[1210]—Our Kentish saying is that "Tenterden steeple was the cause of Goodwin Sands." The legend is that long ago the Goodwin Sands were a tract of valuable land lying rather below the high-water level, the sea being kept out by embankments, as it is at this time on many parts of our coast. The money which ought to have been spent annually in keeping the "sea-walls" in proper repair was laid out, it is said, in building Tenterden steeple. The

natural result followed. The sea-wall became weakened, and at last, during a violent storm, the sea burst in, and could never again be excluded. So a tract of valuable land was converted into a dreary waste of sand only exposed at low tide, and which has been the grave of hundreds of brave sailors.—Yours truly,

A Man of Kent.

LETTERS RECEIVED.

William Secundus. Do not know of any receipt for skeletonising birds. Our Query Column long since closed.—W. Glassford Walker. (1) Know of no sure remedy for smoking chimneys. (2) No reactionary effects follow discontinuance of exercise. (3) Moisture gathers on glass if the air in its neighbourhood is saturated for a temperature higher than that of the glass. Thus, suppose the air inside the glass shade is at a temperature of 60°, and the quantity of moisture such that the air would be saturated at a temperature of 50°, then if the air outside is at a temperature of, say, 40°, the layers next the glass on the inside must give up a part of their moisture in the form of water-drops. As a matter of fact, the air inside a fern-shade is always very nearly saturated.—W. Miller. Thanks; but those maps are not coming out *weekly*.—A. B. Know of no work on mental arithmetic.—Folio. I fear the time has not yet come for a complete popular edition of my works in twelve shilling parts. Alas! my publishers are obliged to ask fifteen shillings for my Library Atlas alone, and I am guilty of forty works in all.—H. Darnsell. I have forgotten what I paid for my object-glass and eye-pieces. Any optician would answer your question better than I.—H. Goldurgh. I never answer by post; moreover, I do not know.—A Beginner. Your calculation would be quite correct were the atmospheric absorption of heat less important than it is. As matters actually are the heat received from the sun at mid-day in summer is much more than 3·7 times as great as that received at mid-day in winter. —P. F. Kerr. Will think over the constellation lore question.—Richard Price. *Exact* speed of light has not yet been determined. About 187,000 miles per second.—W. W. Fawcett. Whether the earth is creeping in towards the sun?—In the words of the eloquent Bishop Peter of Rum-ti-foo:

I do not say it ain't,
But *Time!* my Christian friend.

The year has not diminished ten seconds in length during the last 2,000 years, that is not by 1-3,156,000th part of its length. Since the cube of the distance varies as the square of the period, the distance varies as the period to power two-thirds; and therefore the distance has not changed in 2,000 years by two-thirds of the 1-3,156,000th part, or by 1-4,734,000th of about 92,700,000 miles—say in round numbers 20 miles, or one mile in 100 years. Since at this rate 100 millions of years would only reduce the distance by 1 million miles, or less than a third of the actual range of distance due to the eccentricity of the earth's orbit, it did not seem to me necessary to alarm my audience by any reference to the earth's future absorption by the sun. It won't happen in our time.—A Lady Mathematician. Your letters leave me in doubt whether I should address you as babe or as fossil. Allow me to raise my hat to you and pass on.—T. H. No, certainly. A. McD. makes simply a statement which relates to a matter of fact. I will assuredly not insert the simply hateful and untrue doctrine which you find obvious and pleasing. Why on earth should a man be incapable of thinking and acting aright because what you think so certainly true seems to him indifferent or doubtful or worse? As I happen to know that your view is entirely incorrect, I have a *raison de plus* for not inserting your communication.—Senex. I scarcely think the storm had carried away *your* electricity. With the progress of the storm the electrical condition of the air changed and the phenomena you had noticed ceased; but probably your own condition was much the same all through.—E. T. The outer satellite appears to have a darker surface than the others: but they all look dark when on the brightest parts of the disc of Jupiter.—Jas. Gillespie. Tycho Brahe's old objection, long since met and disproved.—J. R. Sutton. Your letter is an angry one and therefore naturally unwise. It certainly never occurred to me that your imperfect communication was intended as an article! You yourself point out that "it was incomplete"; and in another place describe it very justly as "a half-reasoned paper." Yet you claim that it should have appeared as an article! and, if I understand aright your remark that "time is too valuable to" you for you to "afford to waste it in useless letter-writing," you consider that you should have been paid for your "incomplete, half-reasoned paper"! Still only half-reasoning, or perhaps in your anger not reasoning at all, you deliberately charge me with dishonourable conduct in allowing it to appear (which *I* thought a favour to you) among the letters. You remark further—or rather you begin your letter with the remark—that the communication was sent me "in mistake"; "it should never have been sent at all." To that portion of your extraordinary communication I beg to express my perfect assent. Note, however, that it was *your* mistake, not mine.—C. R. Mr. Hampden, like the original earth-flattener Parallax, is very clever in that sort of argument. Taking a tangent line to the earth's surface, you get for a distance of 65 miles a height above the surface of $(65)^2 \times 8$ in. (8 inches being the depression for one mile), that is, some 3,063 feet. But the real problem is not thus dealt with. Two considerations are conveniently overlooked,—first, the line of sight is slightly curved by atmospheric refraction (the setting sun illustrates this, which also follows inevitably from known optical laws), so that the actual departure of a tangent line of sight from the earth's spherical surface is about 6 in. for a mile; secondly, the height of the observer's eye above the sea-level makes a very great difference in the estimated depression. I do not know at what height the photographic camera was set when the mountains in Man were photographed from Blackpool. I suppose it was at about the height you mention in the case of your own observations—or 300 ft. above the sea-level. [At the top of a tall house in Blackpool, such a distance would easily be obtained at low tide.] Then we have—Distance of point of contact of line of sight with sea-surface, taking 6 in. or half-a-foot for dip from optical tangent line in one mile, is $\sqrt{600}$ miles, or close on 25 miles. For the remaining 40 miles we get a depression of $(40)^2 \times 6$ in. or 800 ft. In certain conditions of the air,—conditions generally present when the air is clear enough to show Man from Blackpool, the refractive effect of the air is even greater, and probably 5 in. instead of 6 in. might be allowed for one mile. But Man rises much more than 800 ft. above the sea-level, especially at the time of low water. The Mourne Mts. could be well seen in clear weather from an elevation of 300 ft. near Dublin, 70 miles away.—T. J. S. I have not seen that narrative. Should be glad to quote it, if you have it by you.—J. Malet. The geological evidence is not needed. Prof. G. Darwin's mathematical demonstration suffices to prove, beyond all doubt, that the real globe of the sun is very much smaller than the globe we see.—Raphael K. May. I agree with you it is a great pity that two meanings have been given to the words billion, trillion, &c. The meaning given in this country is undoubtedly the correct one, as it is the only one which gives any significance to the bi, tri, quadri, &c. A billion is (a million)^2, a trillion is (a million)^3, a quadrillion is (a million)^4; and so forth. But in America and on the Continent, a billion = 1,000 million, a trillion = 1,000,000 million, and so forth.—F. A. M. If gravity took appreciable time at its work, say it acted only as quickly as light travels, the pull of the sun on the earth would not be directed towards the sun's centre. There would be always a force tending to draw the earth onwards tangentially. This in the thousands of years over which astronomy extends its survey would have produced a measurable change even in the position of the earth's orbit.—Subscriber. He is considered very skilful and to know more than most men on the subject.—M. Still your ring reflector would not do. Consider for instance the case of rays proceeding from a point not in the axis (produced) of the cylinder. A telescope which only focussed along its optical axis would be of rather small use.—R. A. H. Thanks, article delayed—but shall appear. The subject is important. — G. Lapthorne. The path of Jupiter, marked in among known stars, unintelligible! My dear sir, what was there to explain?—R. Hodgson. I have not by me the letter or address of "Cosmopolitan," and it would not be consistent with rules to forward them; but I have no doubt he will be ready to give any information you may require. I think your communication of April 4 can hardly have escaped his attention. The matter relating to eight gateways of knowledge might, if somewhat abridged, appear as a letter; but there is not sufficiently general interest in the subject to justify its insertion as an article.—F. M. Thanks for explanation of tricycle problem, which agrees with others received (one or two published).—Stephen Burrell. A reformed alphabet in which all could agree would be worth considering; but remembering how Mr. Pitman destroyed all chances of the phonetic system being generally considered by introducing changes, we prefer to wait until a definite plan is adopted by the whole body of reformers. —J. Harrison. Thanks. Your suggested limits to alterations in constellation maps agree with my own views closely.—E. R. Cox. Thanks for reminding me.—[F. Thompson. No wonder you are angry to see the very secretary of the Zetetic Society converted from his mistake. There was never any idea of discussing Parallax's arguments: they are too utterly worthless.—Sub.-Ed.]—Pascal. But has any one found any difficulty in determining the superfices of rectangles?—C. T. B. Such queries not suitable for these columns.—C. R. The subject is infinitely difficult. Unfortunately our space is finite. But I agree with you that it is as difficult to form an idea of the infinitely small as of the infinitely great.—Orpheus. The story seems to have been discredited 130 years ago, and is scarcely likely to get credit now.

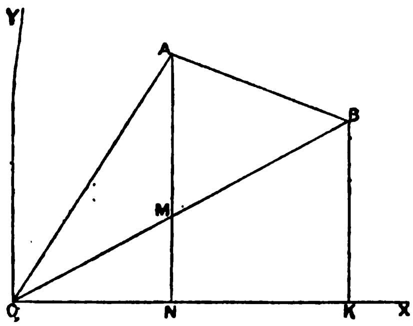
Our Mathematical Column.

EASY LESSONS IN CO-ORDINATE GEOMETRY.

By Richard A. Proctor.

(Continued from page 254.)

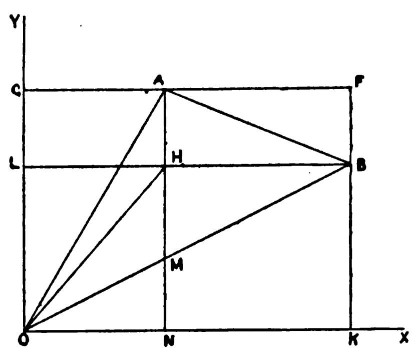
14. *One angle of a triangle is at the origin; it is required to determine its area in terms of the co-ordinates of the other angnlar points.*



Let A B O be a triangle; $x_1\ y_1$ the co-ordinates of A; x_2, y_2, those of B. Suppose x_1 less than x_2. Draw B K, A N, perpendicular to O X, A N cutting O B in M; then

$$\triangle ABO = \triangle OAM + \triangle AMB$$
$$= \tfrac{1}{2} AM.ON + \tfrac{1}{2} AM.NK$$
$$= \tfrac{1}{2} AM\ (ON + NK)$$
$$= \tfrac{1}{2}\ (AN - NM)\ OK.$$
$$= \frac{1}{2}\left(y_1 - \frac{x_1}{x_2}y_2\right)x_2, \text{ since } NM = \frac{ON}{OK}.BK.$$
$$= \tfrac{1}{2}(y_1x_2 - y_2x_1)$$

It will be worth while to consider for a moment what this result means. This may be regarded as the first problem which has come before us for treatment by the methods of co-ordinate geometry, and it illustrates well—though simply—the value of these methods. Consider the result just obtained: it not only solves our problem it



presents a theorem. Draw the parallels G A F, K B F, A H N, L H B: then our result tells us that

$$\triangle ABO = \tfrac{1}{2}\ (\text{rect. } GK - \text{rect. } LN)$$

a result worth noticing, though of course the geometrical proof is sufficiently simple. We join H O, and see at once that the three triangles into which A B O is divided by the lines A H, H O, H B, are equal to half the rectangles G H, H K, H F. But the analytical demonstration of the area of the triangle gives us this result at once, and as something thrown in.

The expression for the area is quantitatively correct whatever the values of x_1, x_2, y_1, and y_2; but it is negative if y_1x_2 is less than x_1y_2, or if

$$\frac{y_1}{y_2} < \frac{x_1}{x_2}$$

Now since M N : B K :: O N : O K

$$\text{or } \frac{MN}{y_2} = \frac{x_1}{x_2}$$

we see that our expression for the area is positive or negative according as y_1 is greater or less than M N.

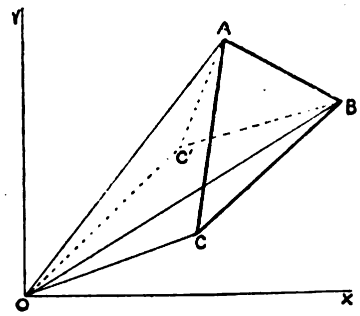
The reader will find it a useful exercise to take the points A, B in other positions, within or without the angle Y O X, interpreting the meaning of the result geometrically for each case thus arising.

15. *To determine the area of a triangle* A B C, *the co-ordinates of* A *being* x_1, y_1; *of* B x_2, y_2; *of* C x_3, y_3.

Join O A, O B, O C, and let the values of x_1, y_1, &c. be such that the lines O A, O B, O C, fall as shown,—O A farthest from O X, O B next, O C nearest. Then

$$\triangle ABC = \triangle AOB + \triangle BOC - \triangle AOC$$
$$= \frac{1}{2}\left\{(y_1x_2 - x_1y_2) + (y_2x_3 - x_2y_3) + (y_3x_1 - x_3y_1)\right\}$$

It will be seen that we have taken for BOC its proper positive expression, while for AOC we have used the negative expression (y_3x_1 being manifestly less than x_3y_1), and therefore altered the negative sign outside to the positive.



The expression thus obtained will always represent quantitatively the area of the triangle, but it will be positive or negative, according as

$$y_1x_2 + y_2x_3 + y_3x_1 > = \text{or} < x_1y_2 + x_2y_3 + x_3y_1.$$

We need not trouble ourselves to consider the effect of this condition, as every case that can arrive yields its own interpretation. the reader will do well however to consider several cases, as that will give useful exercise to him. Thus suppose C′ the position of the third angle, then

$$\triangle ABC = \triangle AOB - \triangle BOC' - \triangle AOC'$$

but the same expression results, for $\triangle$BOC is now expressed not by $y_2x_3 - x_2y_3$ but by $y_3x_2 - y_2x_3$.

16. *The expression for the area may be written also in either of the following symmetrical forms:—*

$$\frac{1}{2}\left\{y_1(x_2 - x_3) + y_2(x_3 - x_1) + y_3(x_1 - x_2)\right\}$$
$$\frac{1}{2}\left\{x_1(y_3 - y_2) + x_2(y_1 - y_3) + x_3(y_2 - y_1)\right\}$$
$$\frac{1}{2}\left\{x_1y_3 + x_2y_1 + x_3y_2 - x_1y_2 - x_2y_3 - x_3y_1\right\}$$

The result of 15 may be obtained somewhat differently as follows:—If the origin of co-ordinates be removed to C, the axes remaining parallel to their original directions, the new co-ordinates of A and B are by 8 (p. 213) (or obviously)

$(x_1 - x_3)$, $(y_1 - y_3)$, and $(x_2 - x_3)$, $(y_2 - y_3)$, respectivel

Hence by Art.

$$\triangle ABC = \frac{1}{2}\left\{(x_2 - x_3)\ (y_1 - y_3) - (x_1 - x_3)\ y_2 - y_3)\right\}$$

which reduces immediately to one of the forms just obtained.

Or the result of 15 may be obtained independently in the manner manner of 14. We should obtain by means of 12 (p. 253)

$$\triangle ABC = \frac{1}{2}\left\{y_1 - \frac{(x_1 - x_3)\ y_2 + (x_2 - x_1)\ y_3}{(x_1 - x_3) + (x_2 - x_1)}\right\}x_2 - x_3)$$
$$= \frac{1}{2}\left\{y_1(x_2 - x_3) + y_2(x_3 - x_1) + y_3(x_1 - x_2)\right\}$$

a form already obtained.

Some particular cases may be noted. Thus if $x_2 = 0$ and $x_3 = 0$, that is, if one side of the triangle lies upon the axis of y, then the area

$$= \frac{x_1}{2}(y_3 - y_2)$$

Similarly if $y_2 = 0$, and $y_3 = 0$, the area

$$= \frac{y_1}{2}(x_2 - x_3)$$

Lastly if $x_3 = 0$, and $y_2 = 0$, or one angle lies on the axis of y and another on the axis of x, the area

$$= \frac{1}{2}\left\{x_1y_3 - x_2y_3 + y_1x_2\right\}$$

which may be written in either of the forms

$$\frac{1}{2}\left\{x_1y_3 + x_2(y_1 - y_3)\right\}, \text{ or } \frac{1}{2}\left\{x_2y_1 + y_3(x_1 - x_2)\right\}$$

(To be continued.)

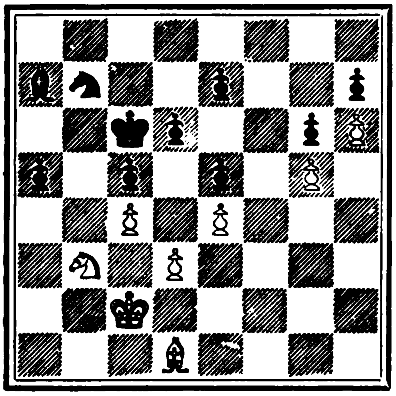
Our Chess Column.

END GAME STUDY.

By Mephisto.

Bishop and Knight.

Black.



White.

White to play and win.

We offer a copy of Bird's "Masterpieces" for the best analytical solution of this Ending.

EASY NOTES ON THE CHESS OPENINGS.

By Richard A. Proctor.

Besides the mistakes already mentioned there is another which young players very often make,—that of starting an attack too hastily. This always leads, if properly met, to an inferior position, for the over-hasty advance has to be followed by a hasty retreat, and the moves made both in the advance and retreat are so many moves wasted.

Returning to our opening, we will now suppose that after the moves 1. P to K4 / P to K4 2. Kt to KB3 / P to Q3 3. P to Q4 / P takes P White plays 4. Kt takes P instead of 4. Q takes P. The position is now

Black.

White.

Position after 4. Kt takes P.

I am rather heterodox in regard to this move, which is usually considered as at least as good as 4. Q takes P. Neither seems to me really good; in fact the continuation 3. B to QB4 seems to me sounder than 3. P to Q4. But for reasons akin to those which cause the move 4. Kt takes P in the Scotch Gambit (which only differs from the above in Black's second move being 2. Kt to QB3) to be regarded as unsatisfactory, I hold the same move to be objectionable against the Philidor defence. Why Black's choice of a reply should be held to lie between 4. P to Q4 and 4. Kt to KB3 I cannot divine; for it seems to me that with the White Knight removed to a position whence he can be easily dislodged, Black has a choice of other moves for the development of his game. I have found 4. Q to KR5 effective at this stage, though it may not be altogether sound. White may more safely be allowed to try 5. Kt to QKt5 than in a familiar form of the Scotch Gambit, for Black can then play 5. Q takes P (ch), and if 6. B to K2 Black can reply 6. Q to QB3. The best reply to 4. Q to KR5 seems to be 5. Kt to QB3; if Black then plays 5. Kt to KB3 he will have to be careful lest his Queen be unpleasantly hampered. However, the books countenance no such response as 4. Q to KR5 in the position illustrated above. The lines of play approved of are given in the following columns:—

4.	P to Q4		Kt to KB3	
5.	P takes P	P to K5	Kt to QB3	
	Q takes P	P to QB4	B to K2	
6.	Q to K2 (ch)	B to QKt5 (ch)	B to Q3 (b)	
	B to K2	B to Q2	Castles	
7.	Kt to QKt5	P to K6	Castles	P to KB4
	Kt to QR3	B takes B	P to QB4	B to KKt5
8.	QKt to QB3	P takes P (ch)	KKt to K2	Kt to KB3
	Q to Q sq.	K takes P	Q to Kt3	P to QB3
9.	B to KB4	Kt takes B	K to R sq.	P to KR3
	Kt to KB3	Q to K sq. (ch)	Kt to QB3	White has the better game.
10.	QR to Q sq.	Q to K2	P to KB4	
	B to Q2	Q takes Q (ch)	White has the better game.	
11.	Q to K5	K takes Q		
	Castles	Kt to QR3		
	Equal game (a)	Black has the better game.		

(a) White can now win the QB Pawn, but after the exchanges R to K sq. followed by B to QKt5 equalises the game.

(b) Or 6. P to KB4 / Castles 7. B to K2 / P to QB4 8. Kt to KB3 / Kt to QB3 Castles with the better game.

But the recapture of the Pawn by White at move 4, though thus shown to be safe, seems to me less sound Chess than the developing move 4. B to QB4, as in the best form of the Scotch Gambit. This leads to the following lines of play:—

Black.

White.

Position after 4. B to QB4

4.	B to QB4		
	Kt to KB3	B to K2	Kt to QB3
5.	Kt to KKt5	P to QB3 (c)	Kt takes P
	B to K3	P to Q6 (d)	Kt takes Kt
6.	B takes B	Q to QKt3	Q takes Kt
	P takes B	B to K3	Kt to K2
7.	Kt takes P	B takes B	Castles
	Q to K2	P takes B	Kt to QB3
8.	Kt takes QP	Q takes KtP	B to QKt5
	Q takes P (ch)	Kt to Q2	B to Q2
9.	Q to K2	Q to QKt5	B takes Kt
	Q takes Q (ch)		B takes B
			P to KB4
	Equal game.	White has the better game.	White has the better game.

(c) 5. Q takes P gives the same position as the fifth move in the second line following the position shown at p. 255, and White gets the better position.

(d) This was the move played by Barnes in a game against Paul Morphy. The object of the move is obvious. White cannot take the Pawn at Q's 6th without impairing his position. Had Black either played P takes P or left the Pawn to be taken by White's QBP, White's game would have been obviously superior to Black's, nor can Black safely defend the Pawn by 5. P to QB4, for the QBP is wanted for the defence of Black's entrenchments on the Queen's side.

Our Whist Column.

By Five of Clubs.

The Hands.

B { *Clubs*—A, Kn, 10, 5, 4. Diamonds—10, 5, 3. Hearts—Q, 9, 5, 4. Spades—3. }

Y { *Clubs*—K, 2. Diamonds—Kn, 8, 6, 4. Hearts—K, Kn, 10, 7, 2. Spades—K, 9. }

A B, 1. B

Y — Z

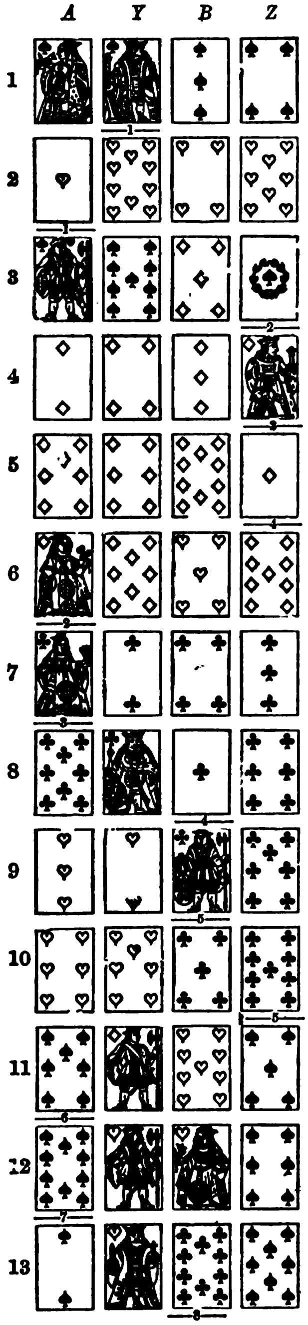
Y Z, 4. *Trump.* C. 7.

A Leads.

Z { 3, 6, 7, 9—*Clubs*. 9, K, A—Diamonds. 8—Hearts. 4, 5, 6, 8, A.—Spades. }

A { *Clubs*—Q, 8. Diamonds—Q, 7, 2. Hearts—A, 6, 3. Spades—Q, Kn, 10 7, 2. }

A Y *B* Z



NOTES AND INFERENCES.

1\. *A* rightly leads Queen from Queen, Knave, Ten, to five.

2\. *B* holding Heart Nine knows that *Z* has no more Hearts. He knows also that *Y*'s lead is from King, Knave, Ten, and others, so that after this round *Y* holds the major tenace in Hearts.

3\. *B* rightly refrains from ruffing, though he knows *Z* holds the Ace. Not only to save his own strength in trumps but to get his partner's suit cleared, he lets the trick go to *Y-Z*. All the table knows that the remaining Spades lie between *A* and *Z*. *B* begins a late signal. His hand would not have justified an early one, but a late one is less imperative.

4\. *Z* should have led through *A*'s strength in Spades. *B* has indicated length in trumps, so that forcing him would have been good policy for *Z*, who holds four himself.

6\. *B* knows that Diamond Knave lies with *Y*; for since the Ten has been already played, the Knave would have been in sequence with *Z*'s 9, and would have been the right card for *Z* to have led had he held it. Clearly *A* does not hold it, or he would have played it as in sequence with the Queen and lower.

7\. *A* responds to his partner's signal with his best trump.

9\. *B* now knows that *Z* holds Club Nine and three Spades; that *A* holds a small Heart and three Spades, headed by the Ten; and that *Y* holds the long Diamond and major tenace in Hearts. *A-B* wanting three tricks to win.

10\. *B* does not capture *Z*'s Club Nine with his Ten, for then he could make only two tricks, but leading the small trump lets *Z* win the trick, who must lead a Spade through *A*'s major tenace.

11, 12. *A* makes two tricks in Spades

13\. And *B* one with his long trump. *A B* win the game.

Note.—The above game illustrates the importance of inferences made as the cards fall. *B* knows that he can lose nothing by letting *Z* take the 10th trick and *may* win, *Z* being obliged to lead through *A*'s strength.

Erratum.—In the game at p. 235, the trump card should have been Diamond Two instead of Diamond Queen.

Accident from Electric Lighting.—From New York it is reported that on the 14th inst. the *Rebecca Everingham*, a river steamer which was laden with cotton, and had forty-one persons on board, passengers and crew, had been totally destroyed by fire near the town of Florence, in the State of Georgia. The disaster is attributed to a fragment of the carbon of the electric light falling upon the cotton. Eleven deaths are known to have taken place, and it is feared that that number will be raised to twenty. The entire living freight of the vessel would have been sacrificed but for the splendid heroism of the pilot, who ordered his son to swim to the shore with a rope, and at the same time continued to steer the vessel towards the shore with the flames literally surging around him. By these means about half the number on board were saved.

The *Riachuelo*, a new ironclad built by Mr. Samuda for the Brazilian Government, has been fitted with compound engines by Messrs. Humphreys and Tennant. The engines are claimed to be the best ever made, and have indicated 4,537 horse-power for three hours' consecutive working, during which the consumption was 1·38 lb. per horse-power per hour. The vessel has also been fitted by Messrs. Siemens Bros. & Co. with an electric-lighting installation, comprising 270 20-candle-power Swan lamps; two yard-arm lights, each consisting of eight 40-candle-power Swan lamps, and two 25,000-candle-power arc lamps for search purposes. The current is supplied by three Siemens' dynamos, each driven direct by a Brotherhood three-cylinder engine.

Contents of No. 129.

SPECIAL NOTICES.

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The following arrangements are complete: the numbers in brackets referring to above list.

BIRMINGHAM (Town Hall), April 25, 28; May 2 (3, 5, 6).
LEAMINGTON (Royal Music Hall), Afternoon, April 26 (6).
COVENTRY, April 30, May 1 (1, 2).
MALVERN, May 3, 17 (Afternoon) (2, 3).
LLANELLY, May 5 (1).
SWANSEA, May 6, 7 (1, 2).
WORCESTER, May 8, 9, 15, 16.
BANBURY, May 14 (Afternoon and Evening).
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CAMBRIDGE, May 21, 22, 23.

Note.—*All communications respecting Lectures should be addressed to Mr. John Stuart, Royal Concert Hall, St. Leonards.*

KNOWLEDGE
AN ILLUSTRATED
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LONDON: FRIDAY, MAY 2, 1884.

CONTENTS OF No. 131.

THE EARTHQUAKE.

By RICHARD A. PROCTOR.

THE earthquake of April 22 belonged to the eastern series of British, or rather of English, earthquakes. There are two regions in which the most remarkable English earthquakes have occurred, one in the west, apparently having Herefordshire for its centre (or perhaps related more directly to the Severn Valley), the other affecting chiefly the eastern counties, though perhaps the eastern earthquakes may in turn be sub-divisible into those affecting the Thames Valley and those affecting the region drained by the Ouse. In KNOWLEDGE for Oct. 20, 1882, Nov. 3, Nov. 17, and Dec. 1, a somewhat full account of the most remarkable British earthquakes was given; and I refer readers to those numbers for full details.

It is noteworthy that the older earthquakes are described,—it may be, carelessly—as affecting the whole of England. Thus Wendover states that in 974 all England was shaken by an earthquake, while a chronicler writes of the earthquake of 1081 that its heavy bellowing was heard throughout the length and breadth of the land. We have no such earthquakes now. So in 1089 there was a "mickle earthquake over all England."

But the earthquake of 1110 was a western one, being felt most near Shrewsbury. That of 1165 was as markedly eastern: it was felt in Ely, Norfolk, and Suffolk, throwing men off their feet and ringing church-bells. So was the earthquake of 1185, in which Lincoln Cathedral was partially destroyed.

In the earthquake of 1247 the banks of the Thames were much shaken, many buildings being flung to the ground. But in 1248 the western parts of England were disturbed in their turn, the shocks experienced in Monmouthshire, Gloucestershire, and Somersetshire proving very destructive. So also the earthquake of 1275 was felt chiefly in the west.

The great earthquake of 1382 resembled in character that of April 22, but was more violent. Several churches in the south-east of England were thrown down.

The earthquake in which Lisbon was destroyed shook the whole of Great Britain, but can hardly be called a British earthquake.

For a long time after the earthquake of 1382 the only remarkable disturbances of the earth's surface in Great Britain were felt in Scotland. The neighbourhood of Comrie, in Perthshire, seems to have been a special region of disturbance,—in fact, scarcely a year passes without some signs of disturbance in or near Comrie.

The earthquake which took place in the autumn of 1863 was a western one, the region of most violent action being in the neighbourhood of Ross and Abergavenny. The Golden Valley in Herefordshire, and the beds of the Wye and its tributaries, were violently shaken.

The earthquake of 1868, which was a very slight affair, was also a western one.

The recent earthquake was a more serious matter. The shock was of different degrees of violence in different places, even in the central region of disturbance; but where the effects were strongest they approached in violence those disturbances in which buildings of considerable size have been cast down. It might be useful to collect the evidence of all who felt the shock distinctly, and by mapping down the spots where the disturbance was greatest, to determine the relation (if any is recognisable) between the structure and contour of the land, and the movements of greater or less energy in the earth's surface.

THE CHEMISTRY OF COOKERY.

XXXIII.

By W. MATTIEU WILLIAMS.

THE practical importance of the fermentation described in my last is strikingly shown by the fact that, in the course of sponge rising, dough rising, and baking, a loaf becomes about four times as large as the original mixture of flour, water, &c., of which it was made; or, otherwise stated, an ordinary loaf is made up of one part of solid bread to more than three parts of air bubbles or pores. French rolls, and some other kinds of fancy bread are still more gaseous.

So far I have only named the flour, water, salt, and yeast. These, with a little sugar or milk added according to taste and custom, are the ingredients of home-made bread, but "baker's bread" is commonly, though not necessarily, somewhat more complex. There is the material technically known as "fruit," and another which bears the equivocal name of "stuff," or "rocky." The fruit are potatoes. The quantity of these prescribed in Knight's "Guide to Trade" is one peck to the sack of flour. This proportion is so small (about 3 per cent. by weight) that, if not exceeded, it cannot be regarded as a fraudulent adulteration, for the additional cost involved in the boiling, skinning, and general preparing of the small addition exceeds the saving in the price of raw material. The fruit, therefore, is not added merely because it is cheaper than flour, as many people suppose.

The instructions concerning its use given in the work above named clearly indicate that the potato flour is used to assist fermentation. These instructions prescribe that the peck of potatoes shall be boiled in their skins, mashed in the "seasoning tub," then mixed with two or three quarts of water, the same quantity of patent yeast, and three or four pounds of flour. The mixture is left to stand for six or twelve hours, when it will have become what is called a *ferment*. After straining through a sieve, to separate the skins of the fruit, it is mixed with the sack of flour, water, &c.

It is evident from this that it would not pay to add such a quantity in such a manner as a mere adulterant. The

baker uses it for improving the bread, from his point of view.

The *stuff* or *rocky* consists, according to Tomlinson, of 1 part of alum to 3 parts of common salt. The same authority tells us that the bakers buy this at 2d. per packet, containing 1 lb. in each, and that they believe it to be ground alum. They buy it thus for immediate use, being subject to a heavy fine if they keep alum on the premises. The quantity of the mixture ordinarily used is 8 oz. to each sack of flour weighing 280 lb., so that the proportion of alum is but 2 oz. to 280 lb. As one sack of flour is (with water) made into 80 loaves weighing 4 lb. each, the quantity of alum in 1 lb. of bread amounts to $\frac{1}{160}$ of an oz.

The *rationale* of the action of this small quantity of alum is still a chemical puzzle. That it has an appreciable effect in improving the *appearance* of the bread is unquestionable, and it may actually improve the quality of bread made from inferior flour.

One of the baker's technical tests of quality is the manner in which the loaves of a batch separate from each other. That they should break evenly and present a somewhat silky rather than a lumpy fracture, is a matter of trade estimation. When the fracture is rough and lumpy, one loaf pulling away some of the just belongings of its neighbour, the feelings of the orthodox baker are much wounded. The alum is said to prevent this impropriety, while an excess of salt aggravates it.

It appears to be a fact that this small quantity of alum whitens the bread. In this, as in so many other cases of adulteration, there are two guilty parties—the buyer who demands impossible or unnatural appearances, and the manufacturer or vendor who supplies the foolish demand. The judging of bread by its whiteness is a mistake which has led to much mischief, against which the recent agitation for "whole meal" is, I think, an extreme reaction.

If the husk, which is demanded by the whole-meal agitators, were as digestible as the inner flour, they would unquestionably be right, but it is easy to show that it is not, and that in some cases the passage of the undigested particles may produce mischievous irritation in the intestinal canal. My own opinion on this subject (it still remains in the region of opinion rather than of science) is that a middle course is the right one, viz., that bread should be made of moderately dressed or "seconds" flour rather than over-dressed "firsts," or undressed "thirds," *i.e.*, unsifted whole-meal flour.

Such seconds flour does not fairly produce white bread, and consumers are unwise in demanding whiteness. In my household we make our own bread, but occasionally, when the demand exceeds ordinary supply, a loaf or two is bought from the baker. I find that, with corresponding or identical flour, the baker's bread is whiter than the home made, and correspondingly inferior. I may say, colourless in flavour, it lacks the characteristic of wheaten sweetness. There are, however, exceptions to this, as certain bakers are now doing a great business in supplying what they call "home-made" or "farm-house" bread. It is darker in colour than ordinary bread, but is sold nevertheless at a higher price, and I find that it has the flavour of the bread made in my own kitchen. When their customers become more intelligent, all the bakers will doubtless cease to incur the expense of buying packets of "stuff" or "rocky," or any other bleaching abomination.

Liebig asserts that in certain cases the use of lime-water improves the quality of bread. Tomlinson says that, "in the time of bad harvests, when the wheat is damaged, the flour may be considerably improved, without any injurious result whatever, by the addition of from 20 to 40 grains of carbonate of magnesia to every pound of flour." It is also stated that chalk has been used for the same purpose. These would all act in nearly the same manner by neutralising any acid that might already exist or be generated in the course of fermentation.

When gluten is kept in a moist state it slowly loses its soft, elastic, and insoluble condition; if kept in water for a few days, it gradually runs down into a turbid, slimy solution, which does not form dough when mixed with starch. The gluten of imperfectly ripened wheat, or of flour or wheat that has been badly kept in the midst of humid surroundings, appears to have fallen partially into this condition, the gluten being an actively hygroscopic substance.

Liebig's experiments show that flour in which the gluten has undergome this partial change may have its original qualities restored by mixing 100 parts of flour with 26 or 27 parts of saturated lime-water and a sufficiency of ordinary water to work it into dough. I suspect that the action of the alum is of a similar kind, though this does not satisfactorily account for the bleaching.

The action of sulphate of copper, which has been used in Belgium and other places for improving the appearance and sponginess of loaves, is still more mysterious than that of alum. Kuhlmann found that a single grain in a 4 lb. loaf produced a marked alteration in the appearance of the bread. Fortunately this adulteration, if perpetrated to a mischievous extent, may be easily detected by acidulating the crumb, and then moistening with a solution of ferrocyanide of potassium. The brown colour thus produced betrays the presence of copper. The detection of alum is difficult.

I should add that the ancient method of effecting the fermentation of bread, and which I understand is still employed to some extent in France, differs somewhat from the ordinary modern practice described in my last. When flour made into dough is kept for some time moderately warm, it undergoes spontaneous fermentation, formerly described as "panary fermentation," and supposed to be of a different nature from the fermentation which produces yeast.

Dough in this condition is called *leaven*, and when kneaded with fresh flour and water its fermentation is communicated to the whole lump; hence the ancient metaphors. In practice the leaven was obtained by setting aside some of the dough of a previous batch, and adding this when its fermentation reached its maximum activity. One reason why the modern method has superseded this appears to be that the leaven is liable to proceed onward beyond the first stage of fermentation, or that producing alcohol, and run into the acetous, or vinegar-forming fermentation, producing sour bread. Another reason may be that the potato mixture above described, which is but another kind of leaven, is more effectual and convenient.

Dr. Dauglish's method (patented in 1856, 1857, and 1858) is based on the fact that water under pressure absorbs and holds in solution a large quantity of carbonic acid gas, which escapes when the pressure is diminished, as in uncorking soda-water, &c. Dr. Dauglish places the flour in a strong, air-tight iron vessel, then forces water saturated with carbonic acid under high pressure into this; kneading knives mix the dough by their rotation. When the mixture is completed a trap at the lower part of the globular iron vessel is opened. The pressure of the confined carbonic acid above forces the dough through this in a cylindrical jet or flat ribbon as required, and this squirted cylinder or ribbon is fashioned by suitable cutters, &c., into loaves. The compressed gas expands, and the loaves are smartly baked before the expansive energy of the gas is exhausted.

The difference between new and stale bread is familiar enough, but the nature of the difference is by no means so commonly understood. It is generally supposed to be a simple result of mere drying. That this is not a true explanation may be easily proved by repeating the experiments of Boussingault, who placed a very stale loaf (six days old) in an oven for an hour, during which time it was, of course, being further dried; but, nevertheless, it came out as a new loaf. He found that during the six days, while becoming stale, it only lost 1 per cent. of its weight by drying, and that during the one hour in the oven it lost $3\frac{1}{2}$ per cent. in becoming new, and apparently more moist. By using an air-tight case instead of an ordinary oven, he repeated the experiment several times in succession on the same piece of bread, making it alternately stale and new, each time.

For this experiment the oven should be but moderately heated—130 deg. to 150 deg. is sufficient. I am fond of hot rolls for breakfast, and frequently have them *à la Boussingault*, by treating stale bread crusts in this manner. My wife tells me that when the crusts have been long neglected, and are thin, the Boussingault hot rolls are improved by dipping the crust in water before putting it into the oven. This is not necessary in experimenting with a whole loaf or a thick piece of stale bread.

The crumb of bread, whether new or stale, contains about 45 per cent. of water. Miller says "the difference in properties between the two depends simply upon difference in molecular arrangement."

This "molecular arrangement" is the customary modern method of explaining a multitude of similar physical and chemical problems, or, as I would rather say, of evading them under the cover of a conventional phrase.

I am making a few experiments which promise to afford an explanation of the changes above described, without invoking the aid of any invisible atoms or molecules, or anything else beyond the reach of our simple senses, and will communicate the results in my next paper.

LIFE IN MARS.

By R. A. Proctor.

All that we have learned about Mars leads to the conclusion that it is well fitted to be the abode of life. We can trace, indeed, the progress of such changes as we may conceive that the inhabitants of Venus or of Mercury must recognise in the case of our own earth. The progress of summer and winter in the northern and southern halves of the planet, the effects due to the progress of the Martial day, from sunrise to sunset—nay, even hourly changes, corresponding to those which take place in our own skies, as clouds gather over our continents, or fall in rain, or are dissipated by solar heat: such signs as these that Mars is a world like ours can be recognised most clearly by all who care to study the planet with a telescope of adequate power.

As regards the atmosphere of Mars, by the way, the earliest telescopic observers fell into a somewhat strange mistake. For, noticing that stars seemed to disappear from view at some considerable distance from the planet, they assigned to the Martial atmosphere a depth of many hundreds of miles—I care not to say how many. More careful observation, however, showed that the phenomenon upon which so much stress had been laid was merely optical. Sir J. South and other observers, carefully studying the planet with telescopes of modern construction, have been able to prove abundantly that the atmosphere of Mars has no such abnormal extension as Cassini and others of the earlier telescopists had imagined.

The early observations made on the polar snows of Mars were more trustworthy. Maraldi found that at each of two points nearly opposite to each other on the globe of the planet, a white spot could be recognised, whose light, indeed, was so brilliant as to far outshine that emitted by the remainder of the disc. The idea that these white spots correspond in any way to the polar snows on our own earth does not seem to have occurred to Maraldi. Yet he made observations which were well calculated to suggest the idea, for he noticed that one of the spots had at a certain time diminished greatly in size. Instead, however, of ascribing this change to the progress of the Martial seasons, he was led to the strange conclusion that the white spot was undergoing a progress of continuous decrease, and he even announced the date when, as he supposed, it would finally disappear.

No such disappearance took place, however. When Sir W. Herschel began his series of observations upon Mars, more than half a century later, the spots were still there. The energy of our great astronomer did not suffer these striking features to remain long unexamined. Searching, as was his wont, after terrestrial analogies—or, at least, analogies depending on known facts—he was quickly led to associate the white spots with our arctic regions. It would follow, of course, that in the summer months of either Martial hemisphere, the snow-cap would be reduced in size, while in the winter it would attain its greatest dimensions. Sir W. Herschel found this to be the case, and he was able to show that the changes which Maraldi had interpreted as suggesting the eventual disappearance of one of the bright spots, were due to the progress of the Martial summer. Precisely as in our summer months, those who voyage across the Atlantic may sail in far higher latitudes than they could safely venture to traverse in winter, so in Mars the polar ice and snow is limited within a far narrower region in summer than in winter.

But after all (it may be urged), to suppose that these two bright spots are formed in reality of ice and snow is rather venturesome. Might we not imagine that some other material than water is concerned in the observed changes? What reason have we for inferring that the same elements that we are familiar with exist out yonder in space?

The answer to these questions,—or, rather, the answers, for we have to do with a whole series of facts, dovetailing in the most satisfactory manner into each other,—will be found full of interest.

We all know that Mars shines with a ruddy light. He is, indeed, far the ruddiest star in the heavens: Aldebaran and Antares are pale beside him. Now, in the telescope the surface of Mars does not appear wholly red. We have seen that at two opposite points his orb exhibits white spots. But, besides these regions, there are others which are not red. Dark spaces are seen, sometimes strangely complicated in figure, which present a well-marked tinge of greenish blue. Here, then, we have a feature which we should certainly expect to find if the polar spots are really snow-caps; for the existence of water in quantities sufficient to account for snow regions covering many thousand square miles of the surface of Mars would undoubtedly lead us to infer the existence of oceans, and these oceans might be expected to resemble our own oceans in their general tint. According to this view, the dark greenish-blue markings on Mars would come to be regarded as the Martial seas.

If this be the case, then I may note in passing that the seas of Mars cover a much smaller proportion of his surface than those of our own earth, the extent of our

seas being to that of our continents about the proportion of 11 to 4: in Mars the land and sea surfaces would seem to be nearly equal in extent. The seas in Mars are also very singularly shaped. They run into long inlets and straits; many are bottle- or flask-shaped—that is, we see a somewhat rounded inland sea connected with what must be called the main ocean by a narrow inlet; and further it would seem as though oceanic communication must be far more complete in Mars (notwithstanding the relative smallness of his ocean surface) than on our own earth. One could travel by sea between all parts of Mars, with very few exceptions, the long inlets and the flask-shaped seas breaking up his land surface much more completely than the actual extent of water would lead us to infer. It may be supposed that on the other hand land communication is far more complete in the case of Mars than in that of our own earth. This is, indeed, the case, insomuch that such Martialists as object to sea travelling (and we can scarcely suppose sea-sickness to be a phenomenon peculiar to our own earth) may very readily avoid it, and yet not be debarred from visiting any portion of their miniature world, save one or two extensive islands. Even these are separated by such narrow seas from the neighbouring continents, that we may regard it as fairly within the power of the Martial Brunels and Stephensons to bridge over the intervening straits, and so to enable the advocates of land-voyaging to visit those portions of their planet. This view is encouraged by the consideration that all engineering operations must be much more readily effected in Mars than on our own earth. The force of gravity is so small at the surface of Mars that a mass which on the earth weighs a pound, would weigh on Mars about six and a quarter ounces, so that in every way the work of the engineer, and of his ally the spadesman, would be lightened. A being shaped as men are, but fourteen feet high, would be as active as a man six feet high, and many times more powerful. On such a scale, then, might the Martial navvies be framed. But that is not all. The soil in which they would work would weigh very much less, mass for mass, than that in which our terrestrial spadesmen labour. So that, between the far greater powers of Martial beings, and the far greater lightness of the materials they would have to deal with in constructing roads, canals, bridges, or the like, we may very reasonably conclude that the progress of such labours would be very much more rapid, and their scale very much more important than in the case of our own earth.

But let us return to our oceans, remembering that at present we have not proved that the dark greenish-blue regions we have called oceans really consist of water.

It might seem hopeless to inquire whether this is the case. Unless the astronomer could visit Mars and sail upon the Martial seas, he could never learn—so at a first view one might fairly judge—whether the dark markings he chooses to call oceans are really so or not.

But he possesses an instrument which can answer even such a question as this. The spectroscope, the ally of the telescope—of small use in astronomical work without the latter, but able to tell us much which the most powerful telescope could never reveal—has been called in to solve this special problem. It cannot, indeed, directly answer our question. It cannot so analyse the light from the greenish markings as to tell us the nature of the material which emits or reflects to us that peculiarly tinted light. But the astronomer and physicist is capable of reasoning as to certain effects which must necessarily follow if the Planet of War have oceans and polar snow-caps, and which could not possibly appear if the markings we call oceans were not really so, nor the white spots at the Martial poles really snow-caps. Extensive seas in one part of the planet, and extensive snow regions in another, would imply, in a manner there could be no mistaking, that the vapour of water is raised in large quantities from the Martial oceans to be transferred by Martial winds to polar regions, there to fall in snow-showers. It is this aqueous vapour in the Martial atmosphere that the spectroscope can inform us about. Our spectroscopists know quite well what the vapour of water is capable of showing in the rainbow-tinted streak which is called the spectrum. When white light is caused to shine through a sufficient quantity of the vapour of water, the rainbow-tinted streak forming the spectrum of white light is seen to be crossed by certain dark lines, whose position and arrangement there is no mistaking. Now the light we get from Mars is reflected sunlight, but it is sunlight which has been subjected to more than reflection, since it has passed twice through the depths of the Martial atmosphere, first while passing to his surface, and secondly while leaving that surface on its voyage towards ourselves. If that double passage have carried it through the vapour of water, the spectroscope will certainly tell us of the fact.

Let us see how this problem was dealt with by our most skilful spectroscopist, Dr. Huggins, justly called the Herschel of the spectroscope. The following account is an epitome of his own narrative:—"On Feb. 14, 1868, he examined Mars with a spectroscope attached to his powerful eight-inch refractor. The rainbow-coloured streak was crossed, near the orange part, by groups of lines agreeing in position with those seen in the solar spectrum when the sun is low down and so shines through the vapour-laden lower strata of our atmosphere. To determine whether these lines belonged to the light from Mars or were caused by our own atmosphere, Dr. Huggins turned his spectroscope towards the moon, which was at the time nearer to the horizon than Mars, so that the lines belonging to our own atmosphere would be stronger in the moon's spectrum than in that of the planet. But the groups of lines referred to were not visible in the lunar spectrum. It remained clear, therefore, that they belonged to the atmosphere of Mars, and not to our own."

This observation removes all reasonable doubt as to the real character as well of the dark greenish-blue markings as of the white polar caps. We see that Mars certainly possesses seas resembling our own, and as certainly that he has his arctic regions, waxing and waning, as our own do, with the progress of the seasons. But in fact Dr. Huggins's observation proves much more than this. The aqueous vapour raised from the Martial seas can find its way to the Martial poles only along a certain course—that is, by traversing a Martial atmosphere. Mars certainly has an atmosphere, therefore, though whether the constitution of that atmosphere exactly resembles that of our own air is not so certainly known. On this point the spectroscope has given no positive information, yet it allows us to draw this negative inference—that, inasmuch as no new lines are seen in the spectrum of the planet, it would seem likely that no gases other than those existing in our own atmosphere are present in the atmosphere of Mars.

(To be continued.)

IN the House of Commons on Tuesday week Mr. Fawcett stated that the number of telegraph messages sent last year was 32,732,000, being an increase over the previous year of 640,000, which again was an increase over the preceding year of 746,000.

COPYRIGHT IN LECTURES.

By H. Greenwood, M.A., Barrister-at-Law.

LECTURERS will hail with satisfaction the recent decision of Mr. Justice Kay, to the effect that they, and not their audiences, have the right of publishing their own lectures. A full and fair newspaper report of a lecture is an undoubted advantage to the lecturer; but the unauthorised publication and sale by a stranger of the entire lecture is quite another matter. It inflicts a treble injury upon the lecturer,—first, by diverting the profits which justly belong to him into the pockets of another; secondly, by enabling persons who might form future audiences to read the lecture instead of hearing it; and, thirdly, by rendering it impossible for him to publish his lecture for his own benefit at any future time. To a mind unversed in legal subtleties it might appear a self-evident proposition that a man has an exclusive right to the creations of his own brain and the results of his own study and research; but in the present state of the law of copyright it is not safe to take too much for granted.

That an author who has *published* and duly registered under the Copyright Acts his book or his pamphlet acquires the exclusive English copyright in it for a specified time is provided by law; and it is clear that the author of an unpublished manuscript may lock it up in his own desk, and thus keep it from publication during his pleasure. And although he may lend his manuscript to any number of friends for private perusal, he has the right to say that nobody shall publish it without his permission. But an impression seems to have been prevalent in some quarters that the delivery of a lecture is a sort of dedication to the public.

It is true that a venerable Lecture Copyright Act, passed half a century ago, provides for the recovery of certain penalties in case of the unauthorised publication of a lecture. But, in order to take advantage of this Act, it is necessary to give previous formal notice to two neighbouring justices of the peace of the intended delivery of the lecture; and as this is seldom, if ever, done, the Act is practically a dead letter, and the rights of the lecturer are governed by the law as it existed before the passing of the Act.

Where no such notice is given, it has often, though erroneously, been assumed that any person, capable of doing so, who hears a lecture, may not only take down in shorthand every word which falls from the lecturer's lips, but may either publish the notes so taken, or make any other use of them which he thinks fit.

It is probably in consequence of such misapprehension of the law that the well-known phonographic publisher, Mr. Pitman, has for some years been in the habit of taking notes of popular lectures, and publishing them in a monthly serial called the *Phonographic Lecturer*. Although this publication is in phonographic characters, it has a large circulation among readers and learners of shorthand; but the legality of this practice does not appear to have been questioned until the present year. The *Phonographic Lecturer* for February, 1884, contained a lecture entitled "The Dog as the Friend of Man," which had been delivered by Mr. Arthur Nicols, F.R.G.S., F.R.S., at the Working Men's College, in October, 1882. The publication was wholly unauthorised by him, and he brought an action to restrain Mr. Pitman from continuing the publication of the lecture, and to compel him to deliver up all copies of it in his possession. It appeared that admission to the lecture was gained by tickets, issued gratuitously by the Committee of the College; and it was contended on behalf of the defendant that, inasmuch as his ticket contained no intimation that the lecture was copyright, he was quite at liberty to publish it. Mr. Justice Kay, however, held that there was an implied contract between the lecturer and his audience to the effect that no person who heard the lecture should publish it for his own profit, and to the consequent detriment of the lecturer; and he granted the relief asked for, with costs. In so doing he followed a decision of Lord Eldon who, in 1823, granted an injunction to restrain the publication in the *Lancet* of lectures delivered by Dr. Abernethy to his pupils. It is worthy of observation that the question does not seem to have been raised in an English Court since that time, probably because Lord Eldon's decision has, to some extent, been misunderstood. Although he ultimately granted the injunction, he at first refused to do so, because Dr. Abernethy's lecture had not been reduced into writing, and he doubted whether ideas and sentiments orally delivered constituted a sufficiently tangible description of property to claim the protection of the Court. But his final judgment was based on the lecturer's right of property in his composition, as well as on the breach of an implied contract between the doctor and his pupils. In the recent case the lecture *had* been reduced into writing, but the judgment of Mr. Justice Kay was based on the same grounds as that of Lord Eldon, and will, therefore, also apply to cases where lectures are orally delivered without having been previously written.

No damages were given to the lecturer, because, although the publication in phonographic characters was just as much an infringement of his rights, as a publication in Greek, Latin, or Sanscrit would be, it was not a publication calculated to inflict so serious an injury upon him as a publication in ordinary typographical characters would have been.

The practical result of this decision will be, not to render illegal a fair newspaper report of any lecture, but to assure all lecturers that the law implies a contract between them and their audiences which will enable them to retain for their own benefit the sole right of publishing in a complete form, the lectures which are, in many cases, the result of years of observation, thought, study, and experience.

OPTICAL RECREATIONS.

By a Fellow of the Royal Astronomical Society.

"SEGNIUS irritant," wrote Horace to the Pisos 1,900 years ago, "Segnius irritant animos demissa per aurem, quam quæ sunt oculis subjecta fidelibus." Things which we merely hear with the ear affect the mind less than those which have been subjected to the more trustworthy sight. With reference to no department of human knowledge are these words truer than they are in connection with the study of physical science. The student may, and often does, succeed in mastering the principles of some physical phenomenon after hours and, perhaps, days of reading; but, still, his conception of it is not of a vivid character, and he fails to realise it, in any legitimate sense, thoroughly. Let him, however, make it the subject of experiment, let him witness it with his own eyes, and the whole character of his knowledge of it is at once changed. The reader may forget: the observer never does.

The study of the properties of, and the phenomena exhibited by, light, and of its effects upon our own organs of vision is an extremely interesting one, and may be carried on by the aid of the most inexpensive apparatus. Especially do such properties and phenomena admit of illustration by curious and amusing toys, a description of some of which

will form a leading feature in this series of papers. In them we purpose to give a theoretical and descriptive account of the phenomena of light and vision, and to explain the construction of the very simple apparatus by the aid of which the student may see and observe such phenomena for himself. In every case we shall give practical instructions for the manufacture of the apparatus described; so that the reader possessed of the slightest mechanical aptitude may be able to make it with his own hands. If, occasionally, some of the instruments we treat of may appear at first sight of a trivial and childish character, we would, in the outset, plead that it by no means follows that the lessons to be derived from them will possess less scientific value. The immortal Newton himself excited ridicule by blowing soap-bubbles, but his optical discoveries made by their aid are now matter of history, and have largely contributed towards our knowledge of the nature of coloured light.

To begin with, then; the fact that we see at all shows that there must be a communication of some sort between the eye and the object viewed. Plato and Empedocles, among other of the ancient philosophers, regarded sight as a species of touch, and imagined that something went out from the eye to the object seen! How any one got this strange idea into his head it would be hard to guess. Perhaps the originator of it had been viewing a lamp with his eyes nearly closed, and took the rays seemingly connecting his eyes and the lamp for some kind of feelers proceeding from his own visual organs. But we give this notion, which occurs to us as we write, for what it is worth. The fact, however, that we cannot see in the dark is sufficient to show that vision is not a tactile sense in the way these old philosophers conceived it to be, and that something else besides the eye is concerned in it. For every one knows that unless an object is what we call "luminous," either inherently so, like the sun, the fixed stars, a lamp, or the electric arc; or from reflected light, like the moon and planets, the walls of our rooms in the daytime, or by lamp-light, or the page on which the reader's eyes are now fixed, it is invisible. Moreover, it is further essential that nothing opaque shall intervene in a straight line between the eye and the object viewed. The forefinger, if held sufficiently near to either eye (the other being closed), will blot out all view of this page whatsoever. Calling, then, the medium between the object seen and the eye light, we may say that it is only perceived when it enters the eye in straight lines. Whatever its nature may be (a question to be immediately discussed), one thing is quite certain, and that is that *light is itself invisible!* Let us begin our experimental investigations by showing this.

A tube should be made (by rolling pasted brown paper round a wooden cylinder) and lined with the dead black paper procurable at most stationers' shops. The bottom of this is closed, but one end is left open to be looked into. Two large holes, which are shown at H H' in figure 1, one on each side, make a wide opening to admit strong light across the tube only. A smaller hole, *h*, is also bored,

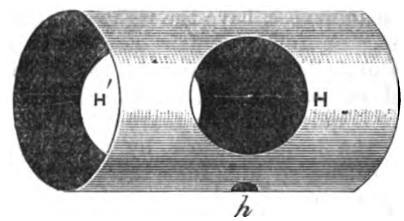


Fig. 1.

for the purpose to be immediately explained. Now, let the sunlight entering, say, through a hole in the shutter of a darkened room, shine through the side apertures of our tube, going in at one and coming out at the other, and let us look into the tube. Doing so, we shall see—nothing. Everything is perfectly dark. If now, though, through the little hole, *h*, we introduce the tip of a tooth-brush handle, or of the finger, or a bit of white paper into the course of the rays, this instantly reflects a portion of such rays of the crossing beam of light, and so reveals their existence. We have described this little piece of apparatus, because it exhibits the fact it is intended to illustrate in a vivid form—a moment's consideration, though, will show that were light, as light, not invisible, the whole of our night-sky, outside of the conical shadow of the earth, must be a blaze of light, as, of course, the sun's rays are traversing it everywhere. But here, the observant beginner may remark:—"If what you say is true, how do you account for those fan-like rays seemingly emanating from the sun, which country people call 'the sun drawing water'; why is it that you can often track the beam of light from a policeman's bull's-eye lantern, like a comet's tail; and how was it that the last time I was in a room with the shutters shut and the sun shining through a chink in them, I could trace the light right across from the shutter to where it fell on to the floor?" The answer to which very pertinent questions is, that in the first two cases particles of vapour, and in the third case of dust, played the part of our tooth-brush handle, or bit of paper, and reflected to our eyes some of the crossing light. A pretty illustration that light travels in straight lines may be obtained thus.

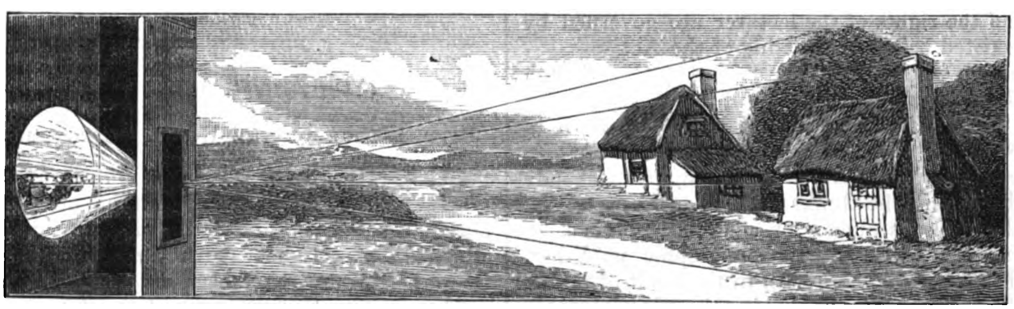


Fig. 2.

Darken a room, looking out upon a street or landscape, totally, and bore a little gimlet-hole in the shutter. Now hold a white screen in front of the hole, shifting it backwards and forwards until the image painted upon it is at its sharpest; then will the whole exterior view be seen depicted

on the screen in its natural colours, but turned upside down, on account of the rays of light crossing in the opening. The course of the rays is shown in Fig. 2. So far, then, we have learned two things concerning the means of communication between our eyes and the objects we see: the first, that it is itself invisible; and the next, that it is propagated in straight lines. Let us see what else we can discover as to its nature. Every means of physical communication (save perhaps gravitation) takes time. If we wish to get from Westminster Bridge to Kew Gardens, we shall scarcely effect it by two hours' walking. The Underground Railway, however, will take us there in about five-and-thirty minutes; while the flight of a Whitworth projectile would be measured by seconds. Still, whether the interval be short or long, an interval there is, as we have said, in every means of physical communication. Now, does this exist in the case of light? Our first impulse would be to say, No. The velocity with which light travels is so enormous, that, save by apparatus of the utmost possible refinement and delicacy, it is absolutely immeasurable upon the earth. Galileo was the first to attempt to ascertain the rate at which it travels. In effect his method was this: he placed two observers a mile or two apart at night, each having a lantern, with a little contrivance by the aid of which the light could be instantaneously covered and caused to disappear. Then Observer No. 1 covered his lantern, and as soon as Observer No. 2 saw the distant light disappear he covered his also. Now that the first observer should see the light of the second disappear it will be noted that the light must have travelled from the first station to the second and back again. Suppose, for the sake of illustration, that the stations were two miles apart, and that the light travelled two miles in a second, then Observer No. 2 would continue to see the light of Observer No. 1 for a second after it had actually been extinguished; and if he then instantly shut off his light, Observer No. 1 would continue to see that for a second after it had gone out; so that, apparently, to him light No. 2 would not disappear for two seconds after he had extinguished his own. But this ingenious device of the immortal Pisan broke down utterly; as we know now, because so far from light travelling two miles in a second, it traverses that space in ·000011 second; or, practically, in just one hundred thousandth part of the time!

It was through the eclipses of Jupiter's first satellite (p. 126) that our first inkling of the velocity of light was obtained. In 1675, Römer, a Danish astronomer living in Paris, set himself to improve the tables of these phenomena. He found that this satellite went round the planet in 42h. 28m. 36s., so that if it were eclipsed at a given instant, its (say) forty-seventh succeeding eclipse would be accurately predicted by adding 42h. 28m. 36s. × 47 to the observed time. His first observations were made when the earth was at her nearest point to Jupiter—when she was between him and the sun, or, technically speaking, Jupiter was in opposition. Six months afterwards, when the earth was on the opposite side of the sun, or Jupiter was approaching conjunction, Römer found, to his surprise, that the eclipses happened (as he supposed) 22m. too late! By a flash of inspiration, it occurred to him that this must arise from the light taking this time to cross the diameter of the orbit of the earth; so that light must travel from the sun to the earth (over the radius of that orbit) in about 11m. There was considerable error in this determination, owing to Römer's imperfect means of observation, but the principle was correct enough; 8m. 19s. represent this last time with greater accuracy. The theory of the ingenious Dane was subsequently corroborated by the discovery of Aberration in 1728 by Bradley, who showed that, owing to the velocity of the earth's motion in her orbit bearing a measurable proportion to that of light—being, in fact, 1-10,089th the latter—every star apparently describes a tiny annual orbit at right angles to that of the earth. Bradley, though, as well as Römer, over-estimated the velocity of light, from imagining the earth to be farther off from the sun than we now know it to be.

(*To be continued.*)

ELECTRO-PLATING.

IV.

BY W. SLINGO.

SEVERAL forms of battery are employed by working electroplaters, some of which can lay claim to but few if any good qualities. The forms most generally used are the Daniell, Smee, or a modification of the simple cell containing copper and zinc plates immersed in an acid solution. The first great desideratum in electro deposition is a current of constant strength derived from a source of low resistance. Now, of all the cells ever introduced, there never has been, and it is open to doubt if there ever will be, a battery steadier or more reliable than the Daniell. The relative resistance of a Daniell cell is high, but this is a difficulty which may always be overcome by increasing the dimensions of the plates. It is urged by those opposed to this battery that it is a troublesome one, and calls for a great deal of attention. But it is a remarkable fact that in other industries, where battery cells are used by the thousand, the Daniell cell requires less attention than almost any other form of battery. The chief point to be observed in the maintenance of the battery is to keep it clean, internally as well as externally. A damp cloth should be passed over the top of the porous pot, &c., to remove any crystals that may have been deposited, and the zinc plate should be freed from any copper which may have been deposited upon it. Occasionally a portion of the zinc solution should be removed and the cell filled up with water. The copper division should always contain a supply of undissolved sulphate. With a battery of two cells a mean attention of ten minutes every other week should keep it in excellent order, and it is not too much to say that the current yielded ought not to vary in strength to the extent of 1 per cent. The amateur will find two cells of gallon capacity answer his purpose for depositing copper. The zinc may take the form of a cylinder (of quarter inch metal) or of a rod, being placed in the former case outside the porous pot, and in the latter case inside the pot. If the amount of zinc surface in the one instance be equal to the amount of copper surface in the other, and the copper surface of the first equal to the zinc surface of the other, equal currents will be produced, because the resistances are identical. An easily-cast zinc rod may therefore be made to yield a current equal in strength to that resulting from a large cylinder. The weight of zinc consumed for equal currents must, obviously, be equal. The rod would consequently call for more frequent renewal than the cylinder. I am in the habit of using zinc cylinders made from quarter-inch plates weighing 10 lb. each. No advantage is gained by using an outer vessel made entirely of copper as the negative plate. Smaller cells may be used, but the quantity of copper deposited will be proportionately less. When the battery is not in use, it will be found to be advantageous to remove the zinc from the solution. A small piece of zinc placed at the bottom of the zinc division also helps to keep the solution clear by precipitating the metal from any of

the copper solution that passes through the porous partition. When the battery is not expected to be called upon for any lengthened period, the porous pot should likewise be removed and placed in a vessel containing water.

A very useful and economical form of battery is a modification of the Smee, in which odd scraps of zinc are placed at the bottom of a glazed earthenware jar, and pouring over them a few ounces of mercury (sufficient at least to connect all the zinc scraps). This forms the positive plate, the bared end of a piece of gutta-percha-covered wire dipping into the mercury to provide a means of connection. The negative plate consists of a thin sheet of platinised silver. Acidulated water (one of sulphuric acid to twelve of water) is poured in until the silver plate is covered. Such a cell offers but little resistance, and is reliable. As the zinc is dissolved, fresh scraps should be placed in the mercury.

Sufficient having been said anent the battery and the required current, it behoves us to direct our attention to the electrolytic cell, or the vessel in which the deposition is intended to be produced. The various depositions of copper may be divided into three classes: the separation of pure copper from the crude metal; the development of a copper surface on baser metals; and the production of copies of an innumerable variety of substances. The separation of pure metal is an important process to the practical electrician, as the copper may be obtained from the impure smelted metal in a state of almost absolute purity. As the demand for electrical wires of the highest conductivity increases, there is little doubt but that this industry will be largely developed. The process consists in immersing the slab of crude metal in a solution of sulphate of copper, and connecting it to the positive pole of a battery or, in this particular case, of a dynamo machine specially wound so as to yield a current of great strength (in amperes) but of feeble electromotive force. If the containing vessel be of metal, or if it is covered internally with some conducting material such as plumbago, it may be connected to the negative pole of the generator of the current. The flow of electricity results in the slab being dissolved and the copper precipitated either on the sides of the vessel or on whatever else is used as the negative electrode. It is an interesting fact that, however many impurities may be present in the slab, very few are likely to be deposited with the copper. The most likely is cadmium, and that is rarely present. The impurities, naturally, vary with different examples of crude metal. According to Dr. Gore, "silver is precipitated at the anode or positive electrode by the traces of hydrochloric acid present in the common sulphuric acid employed. Gold falls as metal at the anode, lead as sulphate; carbon and metallic sulphides, also selenium and silica fall at the anode. Zinc, iron, tin, cadmium, cobalt, nickel, and antimony are more or less dissolved, but, being less readily deposited than copper, remain in solution. Arsenic falls as an arsenide." By a similar process copper is frequently separated from the rough ore.

"Electro-plating" is a term generally applied to the deposition of silver upon baser metals and other substances which require a silver coating; but it is equally applicable to the like deposition of any other metal. The process consists in placing the substance to be coated in the electrolytic cell, and connecting it to the negative pole of the battery. When depositing copper a bluestone solution is employed, and the anode or positive electrode consists of a stout plate of good copper. The passage of a current causes the decomposition of the solution and the deposition of the metal. The acid portion of the decomposed sulphate of copper dissolves copper from the plate forming the anode equal in quantity to that deposited, so that the amount of sulphate in solution remains constant. Although but few metals are deposited in the presence of copper, it is essential in plating processes to use copper-plates and salts of the purest obtainable qualities, otherwise the solution will soon become charged with a large percentage of impurities.

The third form of electro deposition, viz., electrotyping, or the production of metallic copies of various substances, consists in placing a mould or cast of the object to be copied in an electrolytic cell or bath, the surface of the mould being well covered with some such substance as plumbago, and connected with the negative pole of a battery. The other conditions of the cell are similar to those of the plating process. The preparation of iron and other metallic surfaces, and the manufacture of moulds, &c., involve processes fraught with the greatest interest, and will next claim our attention.

HOW TO SELECT A LIFE ASSURANCE OFFICE.

II.

In making comparisons between the affairs of various life assurance offices there are two or three facts that should be borne in mind.

1. Is the rate of interest on the assets too great or too small? If it be above the average rate of interest, it would show that the investments are risky; and, as there are fewer purchasers for risky investments, and such investments often decline in price and require time in order to be realised without loss, that therefore they are probably not readily realisable. In such case, there may be delay, or possibly failure in paying the death claims in full. If the rate be below the average, it would seem that the funds are not invested to the greatest advantage, and that the bonus returns would accordingly be small.

2. With regard to the items of commission and expenses of management, it is a matter of great judgment and requiring large experience. What is the boundary-line between wasteful expenditure and ill-advised parsimony? As either extravagance or parsimony will prejudice the assured, the former reducing the bonus, and the latter affecting the size and prosperity of the office, it is important to ascertain what is that boundary line, and assuredly it will be best known by those who have had experience, namely, the directors of the best offices. Their opinion will be evinced by the average amount they spend in these items, which amount has been already ascertained.

3. It has been said that, under certain circumstances, excessive premiums do not at all prejudicially affect the assured in mutual offices, and only to a certain extent the assured in offices having proprietors. The subject may be carried further. The amount of each premium being small, it will generally be paid out of income, and not out of capital, and if it were not so applied it would most probably be expended on unnecessary purchases, or be lying idle in a bank. But an assurance office aggregates the several premiums they receive, and reserving as much ready money as experience proves necessary to meet current liabilities, invest the residue at compound interest. The excessive premium being thus returned with the interest to the assured as "bonus," he gains an obvious benefit.

4. It is found that an office begins to feel the strain of first assurers dying and first death-claims occurring when it has been established about thirty years. It is therefore well to select an office that has successfully tided over this important era, and has not then been found wanting.

5. No doubt, on principle, the assured in an established, well-managed office paying adequate premiums, do not require the security of a proprietors' fund to be certain that the office will meet its liabilities. The amount the proprietors take out of the profits above the amount of the current rate of interest on their capital is, therefore, loss to the assured.

6. With regard to the size of an office. Fifty men of the same age might form amongst themselves an assurance society; but the working of such a society would not be as economical as that of a large society. First, the expenses that would be necessary for working a society of fifty members would be sufficient, or nearly sufficient, for working another of 100 members. Consequently each member of the smaller society would have to pay twice as much towards working expenses as the member of the larger society; and the premiums of the smaller society would have to be larger in proportion. Secondly, the large society would be justified in giving a remuneration sufficient to attract persons of acquirements and cultivated intelligence superior to the common average, and who have made the management of assurance societies their specialty, to fill the appointments of a skilled character; and these persons, from their superior education and greater experience, would be able to see probabilities of advantages, to guard against blunders, and generally to conduct the business of the society to the best advantage. But a small society would be reluctant, on the ground of expense, to employ highly-paid officials. Thirdly, if the number of members, or the area of their residence, or the variety of their occupations, be limited, there is a greater liability to more than average mortality, by reason of the number of members not being sufficient to allow the number of deaths at old ages to counteract the number at young ages; or of there being an epidemic, and consequent exceptional mortality, in the area; or of all the occupations of the members being unhealthy. The small society may therefore either require to ask higher rates of premium, or fail to meet its liabilities. On the other hand, in a large society, the members would be numerous, and scattered over a large area. An epidemic at one place, or the fact of some members being engaged in one of the more unhealthy occupations, would be compensated for by unusual healthiness in another place, or by other members being more healthily engaged. Besides which, the funds of a large society would be able to tide over any exceptional strain, while those of a smaller society would not.

7. Public opinion, as evidenced by the popularity of an office, is surely worth attention, as showing the justice and liberality with which the office is accustomed to treat the assured.

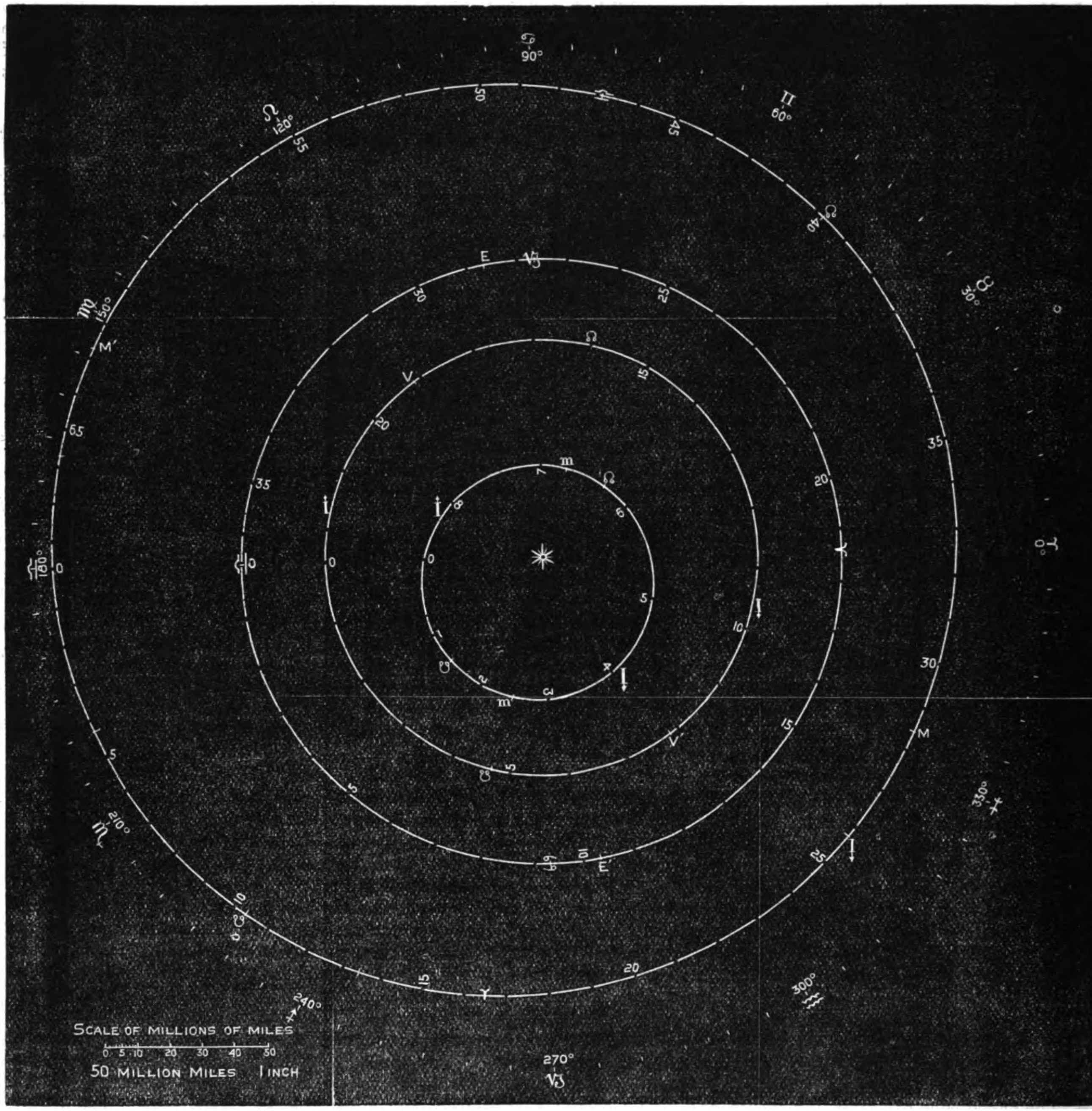
8. The expediency of employing agents, and giving commissions, more especially the latter, is a matter on which opinions differ. Some will say that in these days of keen competition, the practice is necessary, and the money so spent an economical outlay—the giving of commissions being a fair way of remunerating those who use their influence to bring business. Others will tell you that, even if agents be necessary, the granting of commissions is bribery and corruption.

In conclusion, a young clerk, or man in receipt of a salary which will probably increase, would be glad if he might pay a smaller premium for the first few years. On the other hand, a newly-married woman would be glad to pay more at first, so that when her sons and daughters grow up, and become more expensive, she may have the more money at disposal with which to help to educate them, and start them in life. Some of the offices are willing to accommodate their customers in these and in many other minor matters; but these matters apply only to individual cases, and the mention of them would take up too much of our space. Assistance must, if necessary, be obtained from those who are acquainted with the course of business of the various offices, and have the requirements of the particular case before them.

SINCE 1879 the mileage of Ohio railways has increased 107 per cent., the number of locomotives 145 per cent., and the number of cars 456 per cent. The number of cars to each locomotive in 1869 was 15; now the number is 34. Improvements in permanent way by the laying of steel rails, substantial ballast, and superior motive power have increased the carrying capacity of Ohio railways 126 per cent. during the last fifteen years.

AN ELECTRIC TRAMWAY INCIDENT.—The following appeared recently in the *Irish Times* (Dublin):—"It is not generally known that the country people along the line of the electric railway make strange uses of the insulated rails, which is the medium of the electricity on this tramway, in connection with one of which an extraordinary and very remarkable occurrence is reported. People have no objection to touch the rail and receive a smart shock, which is, however, harmless, at least so far. On Thursday evening a ploughman, returning from work, stood upon this rail in order to mount his horse. The rail is elevated on insulators 18 in. above the level of the tramway. As soon as the man placed his hands on the back of the animal it received a shock, which at once brought it down, and falling against the rail it died instantly. The remarkable part is, that the current of electricity which proved fatal to the brute must have passed through the body of the man and proved harmless to him."

ONE of the finest examples of lighthouse construction the world possesses, says the *Electrical World*, is that at the entrance to Port Jackson, Australia, called Macquarie Light. It is a first order, 16-sided, dioptric, holophotal revolving white light, of the system of Fresnel, showing a flash of eight seconds in every minute, and having a range of 25 miles seaward. The gas and oil burners for use during clear weather have, with flames of $1\frac{1}{2}$ in. diameter, an intensity of about 200 candles instead of 80 candles with the same diameter, as with the old type of burner originally intended. When these flames are at the focus there is a consumption of about 40 cubic feet per hour of 16-candle gas, and of good paraffin about 1 pint per hour, and it is estimated that the mean intensity of the flashes from the apparatus is about 40,000 candles, or about five times the intensity of the flashes of the old Macquarie Light. With the full power of the electric light (used in conjunction with the gas in hazy weather) at the focus, the mean intensity of the flashes in the direction of the sea horizon is not less than five or six million candles. By a single arrangement the change from gas light to electric light at the focus or the reverse, can be effected in ten seconds, and the flames of the oil lamp can be substituted for the gas or electric light in nearly the same space of time. The Macquarie Light is intended only to illumine half the horizon. It is therefore possible to make use of the landward rays by means of a dioptric mirror. This is probably the first instance of the use of a dioptric mirror for an electric light. Arrangements are made to burn either gas or paraffin oil, or to exhibit the electric light at full power or half power. When the electric light is in use, there is always a second lamp in readiness for action.



PATHS OF MARS (M), EARTH (E), VENUS (V), AND MERCURY (m).

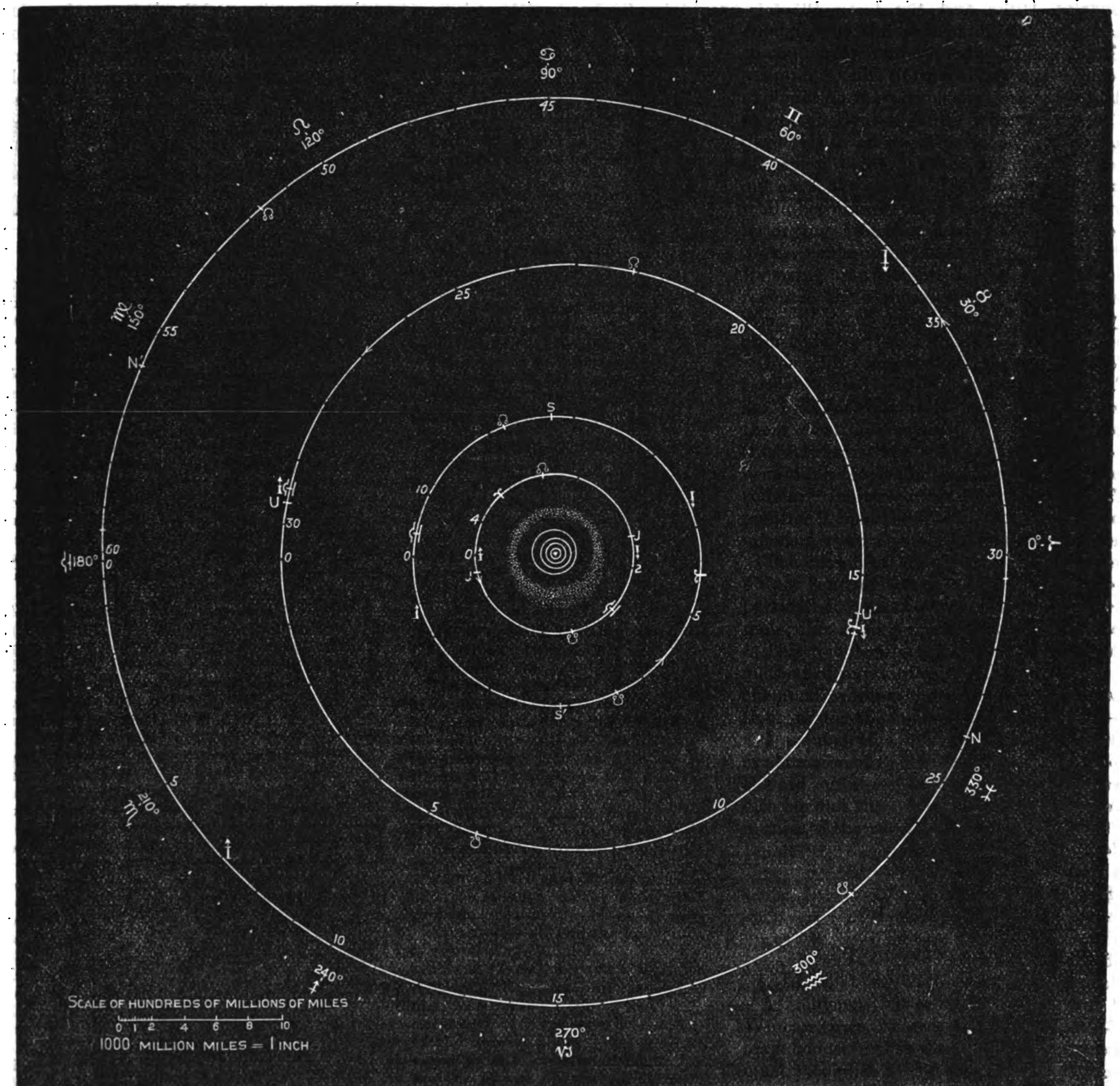
Fig. 1.—The central part of the solar system, showing the region where the terrestrial planets travel. The scale of Fig. 1 is twenty times that of Fig. 2. The numbers round the orbits indicate the number of ten-day periods occupied by each planet in passing to the position so numbered from the position numbered 0 on its orbit.

PLANETARY MOVEMENTS.

SEVERAL correspondents have expressed a wish that we should show from time to time (some suggest once a month) the position of the planets in their respective paths around the earth. Perhaps the accompanying diagrams, which *together* serve to show the real proportions of the solar system, may indicate the difficulty of the suggested task. The pictures explain themselves. The *perihelion* of each planet is where the planet's initial letter (N for Neptune, U for Uranus, and so on, M for Mars, and m for Mercury) is put unaccented; *aphelion* where the initial letter is accented. Rising nodes are marked ☊, descending nodes ☋. The greatest distances attained above and below the plane of the earth's path are shown by a mark like the letter I with a little arrow above or below, respectively. The place corresponding to the spring of the northern hemisphere of each planet whose axial position is known is marked ♈, the place corresponding to autumn is marked ♎.

The puzzle is how *usefully* to show the monthly positions of the various planets.

[Readers are requested to obliterate the marks ♑ and ♋ on the earth's path in Fig. 1.]



PATHS OF NEPTUNE (N), URANUS (U), SATURN (S), AND JUPITER (J).

Fig. 2.—The solar system, showing (truly to scale) the paths of the eight primary planets, and the position of the zone of asteroids. The scale of Fig. 2 is one-twentieth that of Fig. 1. The numbers round the orbits indicate the number of 100-day periods occupied by each planet in passing to the position so numbered from the position numbered 0 on its orbit.

Reviews.

SOME BOOKS ON OUR TABLE.

Lives Worth Living. I. Sir John Herschel. By the Rev. Timothy Harley, F.R.A.S. (London: Alexander & Shepheard, 1884.)—Here, again, is a little tract containing much that is interesting and readable, but disfigured by the "goody" element, which is dragged in, in season and out of season. The perusal of such a work as this always suggests to us the reasonableness of the negro in the legend, who protested against being compelled to endure the "preachee, preachee," in addition to the "floggee." The reader, though, may if he chooses, skip the bits of sermons scattered through Mr. Harley's biography of the great astronomer; he will find the rest pleasing and instructive enough.

Sir Lyon Playfair Taken to Pieces; Likewise Sir Charles Dilke, Bart. (London: E. W. Allen, 1884.)—Lest anyone hastily reading the above title should be frightened into the temporary belief that two eminent statesmen have been dissected and their remains got rid of, let us hasten to reassure them by saying at once that the farrago whose title heads this notice is the production of one of those amiable fanatics the anti-vaccinators. If assertion were proof, or abuse and declamation argument, verily Sir

Charles Dilke and Sir Lyon Playfair would be "in a parlous state." Inasmuch, however, as ninety-nine per cent. of the readers of Mr. White's book will at once decline to accept them as such, we may reasonably regard the two objects of its attack as still entire and unscathed. The senseless insult to an eminent American physician, on p. 49; the utterly unsupported assertion as to the domestic surroundings of Mary, the Queen of William III., on p. 110; and the vulgarly abusive letter on p. 127 *et seq.* will suffice to disgust many who will perhaps fail to perceive the fallacies underlying the mass of Mr. White's statistics. It may—or may not—be desirable that vaccination should be compulsory, but it is absolutely certain that the repeal of the existing law will never be brought about by such stuff as this.

Facts Around Us.—Simple Readings in Inorganic Science, with Experiments. By C. Lloyd Morgan, F.G.S. (London: Edward Stanford. 1884.)—This little book contains a series of lessons in Physics, adapted to the capacity of the young; and is apparently intended to be used as a text-book by the teacher, who is to illustrate the various essays by the simple experiments described in them. There are forty of these lecturettes in the work before us; an appendix of chemical problems, with answers; and a really good index. We have said that Mr. Morgan's book seems to pre-suppose its use by a teacher; but it appears to us equally adapted for self-instruction. Certainly, any one who will thoroughly master its contents will have received an efficient grounding in the rudiments of natural and chemical philosophy.

Physiography. Advanced Course. By Andrew Findlater, M.A., LL.D. (London: W. & R. Chambers. 1883.)—We have one solitary fault to find with Dr. Findlater's work, and that is its title. This is one of those pieces of bastard classicism (akin to "chromosphere") for which we are indebted to certain gentlemen at Brompton, and was well described at the time of its introduction as Physi(cal ge)ography eviscerated. The present work deals, though, mainly with Celestial Physics and Chemistry, subjects which are treated at once succinctly and efficiently; a mass of astronomical information being at once pleasantly and perspicuously conveyed. We note that while the author himself, of course, speaks in pp. 25 *et seq.* of the Precession of the Equinoxes," in a copy of the "Syllabus of the Science Department, South Kensington," on p. 152, the "Procession of the Equinoxes" appears. Does the Brompton Examiner in "Physiography" regard the annual motion of the intersection of the equator and the ecliptic as a species of celestial Lord Mayor's Show? "Aperture," too, by the way, on the ninth line of p. 3, should be "aperture²."

The Art of Soap-Making. By Alexander Watt. (London: Crosby, Lockwood, & Co. 1884.)—If the time-worn proverb be true that "Cleanliness is next to godliness," then should the English nation (as assuredly the very "tubbingest" in the world) be on the high road to righteousness, and Mr. Watts's capital book command a circle of readers co-extensive with the British public. Speaking seriously, however, this is really an excellent example of a technical manual, entering, as it does thoroughly and exhaustively, both into the theory and practice of soap-manufacture. Not only will Mr. Watts's book, we imagine, be read by every one engaged in the soap trade, whether as producer or distributor; but its pages may be consulted, not without profit, by all who wish to get a little behind the scenes as to the composition of those "toilet soaps," &c., whose virtues are extolled in advertisements of such remarkable ingenuity. Nor will certain trade "dodges" for cheapening the production of soap be without interest to the consumer; while the general reader may here learn how glycerine is recovered from the spent "leys" used in soap production. Ample directions are given for analysing and testing soaps, and the materials used in their manufacture; in fact, nothing is omitted which can be of use to the soap-maker. The book is well and honestly done, and deserves the considerable circulation with which it will doubtless meet.

Electrical Congress.—The adjourned congress to determine and adopt the length of a column of mercury of one square millimètre section, which represents the true ohm, met in Paris on Monday, the 28th ult. M. Mascart has made some measurements which agree very closely with Lord Rayleigh's, the latter being 106·24 and the former 106·33 centimètres. England is represented by Sir William Thomson, Professor Hughes, Professor Fleeming Jenkin, Captain Abney, Mr. Graves, and Mr. Preece.

At a recent meeting of the Berlin Physical Society, Prof. Landolt produced a cylinder of solid carbonic acid he had prepared about an hour before the sitting, and described the mode of its formation. From a Natterer compressing vessel a stream of liquid carbonic acid was made to penetrate into a conical cloth bag. The bag speedily got filled with a loose snow of carbonic acid, which was then, by means of a stamper, hammered together in a cylindrical vessel into a solid cylinder. Compact carbonic acid cylinders of this kind could be touched gently with the hand, and possessed the hardness of chalk, which, too, they resembled in appearance, and on account of their brittleness did not readily admit of being cut with a knife. The specific gravity of solid hammered carbonic acid was found to be 1·2.—*Engineer.*

A Singular Suicide.—On Saturday, April 5, an incident, perhaps unprecedented, took place at Stalybridge. Mr. Barnes, of the firm of Garside & Barnes, owned a fine retriever dog. A few days before, it exhibited symptoms of considerable suffering, and had difficulty in drawing its breath. Early on Saturday morning it made several attempts to get to the canal, but was fetched back to the house by Mrs. Barnes. Eventually, however, it succeeded in effecting its object, and plunged into the water. Several persons who witnessed the incident declare that the dog, although, like all its kind, able to swim with ease, kept its head persistently under water until it was suffocated. Attempts were made to get it out of the canal, but before these were carried into effect the animal was dead. The case is regarded as one of deliberate suicide. The opinion was that the animal had swallowed a piece of meat into which pins had been inserted, and that these had remained in the throat. Some attempt was made to discover if this theory was correct, but the *post mortem* examination was not made by skilled hands, and the search was unsuccessful.—From the *Ashton and Stalybridge Reporter* for April 12, 1884.

Herr S. von Wroblewski and K. Olszewski say the result which M.M. Cailletet and Raoul Pictet obtained in the liquefaction of the gases, lead to the hope that the time is not far distant when it will be possible to examine liquid oxygen in a glass tube as easily as we can at present look at a tube filled with carbonic acid in the liquid state. The one condition which must of necessity be arrived at is a sufficiently low temperature. In a paper published by Cailletet, he directs attention to fluid ethylene as a means of reaching an exceedingly low temperature. This gas, in a fluid state, boils under the atmospheric pressure at 103 deg., as measured by a thermometer of carbon disulphide. S. von Wroblewski has constructed an apparatus for higher pressures, in which considerably large quantities of gas can be subjected to a pressure of 200 atmospheres, and with this apparatus it is proposed to study the temperatures at the moment of expansion. The experiments soon led them to the discovery of a temperature at which carbon disulphide and alcohol became solid, and oxygen was rendered completely liquid with the greatest ease. This temperature is reached by letting liquid ethylene boil in a vacuum. The critical temperature of oxygen is lower than that at which the liquefied ethylene boils under the pressure of one atmosphere. From a number of determinations it was found that with a temperature of 131·6 deg. oxygen begins to liquefy at a pressure of 26·5 atmospheres; at 133·4 deg. it began to liquefy at a pressure of 24·8 atmospheres; and at 135·8 deg. it began to liquefy at 22·5 atmospheres. Liquid oxygen is colourless and as transparent as fluid carbonic acid; it is very mobile and exhibits a very beautiful meniscus. Carbon disulphide freezes at about 116 deg., and again becomes liquid at about 110 deg. Absolute alcohol at about 129 deg. is thick and viscous like oil, and at about 130·5 deg. solidifies to a firm mass.—*Engineer.*

Editorial Gossip.

We have received from Mr. Thos. Foster a singular paper discussing the quaintly scientific suggestion of Dr. S. V. Clevenger, (formerly Professor of Comparative Anatomy in the Chicago University,) that man rose to the upright position ere yet his body was thoroughly adapted for the change. If Dr. Clevenger's theory runs on all fours man ought to have gone on all fours a little longer. Or, more hopefully, we may say that according to the doctrine of evolution, man has not yet reached his best, for for where there is room for improvement, (and Dr. Clevenger has shown that there is) evolution is bound to work out that improvement, in the fulness of time.

The *Cornhill Magazine* for May contains a very interesting instalment of Mr. Payn's "Literary Reminiscences." His experiences as editor of *Chambers's Journal* are exceedingly amusing. I think I could cap some of them with strange coincidences which have come to my own Knowledge. His trouble with impatient contributors whose accepted articles do not appear at once, are particularly amusing. He had an instructive experience of the sort himself, when a beginner (and very young), an accepted article turning into a rejected one, in response to two too earnest appeals for early insertion.

When he was first associated with *Chambers's Journal*, Leitch Ritchie was the editor. Nearly always, writers who sent articles with expressions of admiration for the editor's writings, spelled his name incorrectly! We have no such way of testing our own admirers. But we *are* sometimes favoured with communications from admirers of the great Herschell, with two l's, and with manifest unconsciousness that the Herschelian star is a noble binary—for those who know it.

We get borrowed articles sent us as original ones time and again, but have hitherto not been caught napping that way. Stolen stories must be harder to detect. But probably Mr. Payn's experience in having a story sent him, under a new name, which had appeared in the very magazine he was publishing, twenty years before, is unique.

As an odd coincidence, Mr. Payn's reference to the "Vestiges of Creation" coming at the very time when the authorship has been acknowledged by the last survivor of those originally acquainted with the truth in the matter, is deserving of mention. "I have no doubt," Mr. Payn writes, "that he wrote the famous 'Vestiges,' though possibly in collaboration." Before these words appeared in the *Cornhill* the new edition of the "Vestiges" had appeared with its most interesting and amusing preface, in which the whole-and-sole authorship of Robert Chambers is acknowledged.

In that preface mention is made of a MS. proving incontestably that Robert Chambers *could* not have been the author. That MS. was offered to us when Knowledge was but a few weeks old. We declined it on the plea that whether Robert Chambers could or could not have written the book he certainly *had* written it. The author of the MS. proved "incontestably" that Sir Charles Lyell had written the "Vestiges." But the *Wanganui Herald* took us seriously to task for not knowing that Professor Sedgwick was the author, his attacks on the work being "only pretend," as the little folks say.

It is singular that so able a man as Robert Chambers should have had for brother and (apparently) as fellow-worker a man so shallow as William Chambers, under whose control (after Robert's death) *Chambers's Journal* lost caste seriously. His papers, as Mr. Payn truly says, were written in a bald style and full of platitudes. Yet Robert Chambers, with a family of eleven children (his "eleven reasons" as he once said for not acknowledging the authorship of the "Vestiges") "was dwarfed in the public eye beside his brother," who "being childless and of great wealth was enabled to perform certain public acts, which cast Robert, weighted with his large family, comparatively into the shade." Such is the unfair influence of mere wealth in this country.

I have received, or rather the publishers of Knowledge have received, a further communication from the solicitor of Dr. Birley, as the earth-flattening "Parallax" now elects to be called. (I might be wronging "Parallax" by speaking of this name, rather than Tryon, Goulden, or Rowbotham, as his real name, or by giving an entirely different name as more genuine than any of the four; so for the present let the name "Parallax" be employed.) Mr. Rumney is surprised that after his former letter any further communication from Mr. Wolfson should have been inserted here. He overlooks the circumstance that his former letter stated that proceedings had been commenced against Mr. Wolfson, and that on the strength of this assurance, only, I decided to insert no letters from Mr. Wolfson till such proceedings had been concluded. As it turned out, Mr. Rumney was mistaken on that point.

I have now conveyed to "Parallax's" solicitor my opinion that if "Parallax" is unwilling to avail himself of the facilities I have offered for contradicting *here* any misstatements here made,—over his very own name, of course—the best arrangement, in the interests of the public, is that the whole matter should be thoroughly ventilated by proceedings against myself, or (which would virtually be the same thing) against the publishers of Knowledge. If any statements made here are incorrect they cannot be too publicly set right, and I shall be right glad to help in any way towards that end. On the other hand if—whether through mistaken faith in nostrums or in special forms of regimen, or through ignorance of physical and physical laws—"Parallax" has (in his "Patriarchal Longevity" or otherwise) promised more than can be safely attempted, it is well that this should be as publicly as possible set right.

As to the Flat-Earth nonsense, that is not and never has been worth the trouble of crushing. "Parallax" (as presumably, were his real name known, a medical man), will understand the opinion I have formed on that point, when I remark that his Zetetic Astronomy cannot even be described as still-born: it was never more than an abortion. But those who are foolish enough to be deceived by such reasoning (save the mark) as is found there, though none the worse for that, might be very much the worse if they were misled by false hopes of patriarchal longevity, apparently associated with the advent of one who has not rejected but openly quotes the definition of himself as "a greater than Newton." Hence have I held it my duty to touch on both sides of "Parallax's" public character, his claim to profound knowledge in matters of life and death, as well as his claim to super-Newtonian knowledge respecting astronomy. He is the author of "Earth Life" and "Patriarchal Longevity," as well as of the "Zetetic Astronomy"; and mistakes about matters medicinal are

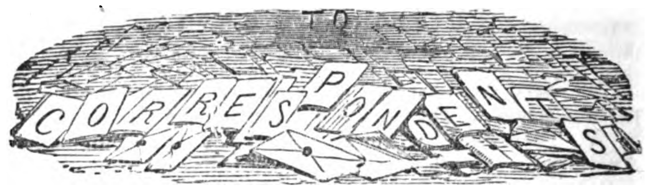
apt to be very much more serious than erroneous ideas as to the shape of the earth.

However, we shall soon know more about "Parallax" if he carries out his announced intention,—and perhaps whether he carries it out or not. If he insists on asserting that he has been wronged, and will not state where and how he has been wronged, I suppose I must endeavour to do him justice without such help. *Fiat justitia ruat cœlum.*

In the meantime, while resolute to discharge what I hold to be a duty, and a duty specially falling within the province of Knowledge (for true knowledge is as much advanced by the rejection of what is erroneous as by the assertion of what is sound) I have no wish, and it is no part of my business, to attack any man's private character. Nothing of the sort was intended, and I find nothing of the sort in Mr. Wolfson's remarks. He applied the term "quack"—with the complimentary adjective "accomplished"—to "Parallax"; but apart from the circumstance that the term relates to "Parallax's" public, not to his private proceedings, it is in itself an epithet of a very mild character. Hundreds of worthy men are quacks. Every man who publicly claims more for his remedies,—social, political, medical, or otherwise,—than they are really worth (and many even of the worthiest do so) is a quack. Nay if any man claims more for a remedy of any sort than another *deems* it worth, he is a quack in the eyes of this other. If the term "quack" signified one of those men who fraudulently assume the medical character, *and under that guise attempt cures or operations of a dangerous, or unlawful nature*, then doubtless it would be a very serious matter to apply it to any person, even though it might be known and could be proved that the implied accusation were well founded. But the term means nothing of the sort. It is certainly applicable without offence to one who has vended medicine for prolonging life beyond its natural term, and has associated the sale of such medicine with public promises of patriarchal longevity.

To assert of such a man what Mr. Wolfson asserted of "Parallax,"—that his real object is to put money in his pocket and that he is very careful of money so made,—is only objectionable in being unnecessary. The real object of every profession and of every trade is to gain money; it is a very proper object (meaning the support of life in self and family), and every wise man is careful of money so made. Only "a *fool* and his money are quickly parted."

An Interesting Discovery.—The Rev. C. W. Markham, of Saxby Rectory, Barton-on-Humber, writes to the *Banner*:—"Sir,—I beg to send you a brief description of a singular and interesting discovery in this neighbourhood. Some brickyard labourers in excavating clay on some level land in the valley of Aucholme, near Brigg, in Lincolnshire, have recently discovered, about 7 ft. below the surface, a very ancient wooden way. It consists of massive beams of oak laid tranversely, and fastened into the soil below—which is the glacial drift—by oak pins. This structure, which it is thought extends nearly a mile across the valley (which in bygone ages was no doubt generally covered with water from the Humber), is thought by some archæologists to be a relic of the Neolithic Man. Its age must be enormous—almost beyond calculation—as on the top of the wooden way is about 6 ft. of solid clay, and above the clay about 1 ft. of peat; and this peat was there at the time of the Romans, we know, as the Roman roads in the neighbourhood are laid upon it. This most interesting discovery can now be seen, but it is to be covered up very shortly, unless some steps should be taken by any learned society to preserve it; it is, I believe, on land belonging to the Earl of Yarborough.



"Let Knowledge grow from more to more."—Alfred Tennyson.

Only a small proportion of Letters received can possibly be inserted. Correspondents must not be offended, therefore, should their letters not appear.

All Editorial communications should be addressed to the Editor of Knowledge; *all Business communications to the* Publishers, *at the Office, 74, Great Queen-street, W.C.* If this is not attended to delays arise for which the Editor is not responsible.

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The Editor is not responsible for the opinions of correspondents.

No communications are answered by post, even though stamped and directed envelope be enclosed.

THE EARTHQUAKE.

[1211]—I beg to record that at 9.18 this morning my iron bedstead was visibly shaken for about two seconds of time. The wardrobe, doors, and windows were also visibly affected, while a servant then in the room said she felt as if she were moved. The same thing was experienced by others in the upper part of the house, but downstairs nothing was noticed. I expect to-morrow either to hear of some great explosion or the occurrence of an earthquake.

Hill House, Long Melford, O. R. Bree, M.D.
Tuesday, April 22.

SIR J. HERSCHEL AND THE COMET OF 1862.

[1212]—Surely you are mistaken in regarding my father's remarks on the comet of 1862 as being based *on his own observations* "telescopically." I feel pretty certain that if he saw that comet in a telescope (of more than 2 in.) *at all*, it can only have been *casually* (*i.e.* in a neighbour's), as he had none in position at any time after leaving Slough in 1840. If I knew to which of his writings you refer, I should, of course, be better able to say if he is alluding to observations by himself, or as reported by observers in general. J. Herschel.

[Lieut.-Col. Herschel's letter reaches me when away from my books. The passage referred to occurs in Sir J. Herschel's "Familiar Lectures on Science." Sixteen or seventeen years have passed since I read the passage, but I certainly am under a strong impression that he speaks of his own observations throughout. In part, he certainly does, as he refers to his "octogenarian eyes." But I am prepared to find that my memory, usually trustworthy, has played me a trick in this case.—R. P.]

THE OCCULTATION OF VENUS, &c.

[1213]—I see in Knowledge, in the article on "Occultation in a Three-inch Telescope," by "F.R.A.S," in the second column, on page 268, he refers to the occultation of Venus which took place on February 29 last, and says that no intimation or prediction of it whatever was given in the "Nautical Almanack". As this statement is scarcely accurate you will, perhaps, in justice to the "Almanack," allow me to correct it. The occultation certainly is not mentioned among the "Occultations visible at Greenwich," on page 435 of the "Almanack," and why it is not I am at a loss to understand; but if "F.R.A.S." will turn to page 408, under the heading of "Elements of Occultations," he will there find it duly mentioned. I may add that this is not the only instance of occultations which are actually visible at Greenwich, being entirely omitted from the list of those visible at Greenwich in the "Almanack," for I have personally witnessed several occultations of stars which are duly referred to in the "Elements of Occultations," and yet not mentioned at all under the heading of "Occultations Visible at Greenwich." Why they are omitted from the latter list I cannot comprehend.

While writing, I may mention that I was somewhat disappointed on opening my Knowledge to-day, for as the planet Mercury is just now coming into a very favourable position for observation I fully expected "F.R.A.S." would have given us, this week, his long-promised paper on "Mercury in a 3-in. Telescope." May we hope to have it next week? I may add that I found the planet with my telescope last evening (April 17), and also saw him with naked eyes afterwards.

I noticed that in last week's KNOWLEDGE you invited suggestions from correspondents as to the "Constellation Maps," which you propose to publish, so I may as well give you mine. I think the black ground should only be inside the circle, and that the maps should contain no marking at all beyond the stars themselves (without letters or names), the constellation figures, and the constellation outlines. I notice that you favoured the exclusion of the outlines, but I should prefer them to be shown. EXCELSIOR.

COINCIDENCES.

[1214]—I have for many years made a practice of jotting down coincidences, and I believe I could cap almost anything in that way that might be offered. I will mention one only.

On my last return to India, the season being unfavourable, my wife followed at an interval of some months, stopping on the way at Colombo and at Madras, and eventually at Calcutta; while I was up country at Dehra. This occasioned considerable uncertainty in correspondence, which, owing to her bad health, caused me great anxiety; and on one occasion, this failure of letters, with other causes, led me to doubt the local post-office. It is these other causes that I have now to relate. Soon after returning, I wished to communicate with friends in different parts of the country; and in particular, I wrote to two ladies, one at a hill station, and the other at a station in the plains, whom I may designate as Mrs. A. and Mrs. B. respectively. My letters were despatched at some considerable interval. I was thus writing to three ladies in widely different parts about the same time, and it so fell out that at the period of my greatest distress at non-receipt of letters from my wife I was also so perplexed at receiving no answers from the other ladies as to cause me to make minute inquiries to ascertain whether my own letters had been duly posted. However, to cut the matter short, I at length got my expected letters—*all three by the same post*, of which there were two deliveries daily. This might of itself be regarded as a curious coincidence enough, but it was heightened by the contents of the letters—that is, of two of them; for Mrs. B. mentioned casually that an acquaintance was breaking up his establishment prior to going home, selling horses, &c., while Mrs. A. wrote *inter alia* that she was hourly expecting to hear of having succeeded in purchasing a horse advertised for sale; and although the writers were utter strangers, it was clear (as I afterwards proved, to the no small surprise of the latter) that the two references dovetailed, and supplemented each other. Whatever may be thought of the primary coincidence, I think it will be allowed that the secondary one was worthy of note. It is only unfortunate that it cannot be told in a fewer number of lines. It naturally appeared even more striking to myself, under the peculiar circumstances, than it can do to any casual reader barely realising the chief features.

I think, by the way, as to coincidence *versus* the supernatural, that it is worth considering that no true coincidence ever strikes any one as in any sense or way supernatural. This does not prevent other combinations being regarded *by some* as supernatural, and *by others* as mere coincidence.

J. HERSCHEL, Lieut.-Col. R.E.

[1215]—Here is a double coincidence which has just happened to me. I am not sufficiently a mathematician to estimate the chances against the double events, but they must be very considerable.

I and my boys are bicyclists. One Saturday afternoon last autumn, I and my eldest son rode, for the first time, to Shenley, in Middlesex. We had tea at the Black Lion, and as we were finishing the Hornsey B. C. arrived at the same house. After tea we rode to the top of Ridge Hill, where dismounting, we were greeted by a friend, Mr. H——, a "riding acquaintance," and quite unconnected with the Hornsey B. C.

Last Saturday, while on the road, we made up our minds to visit Shenley for the second time. We tea'd at the Black Lion, during which repast the Hornsey B. C. arrived. I remarked on the coincidence, and said, "It will be funny if we should see Mr. H—— at the top of Ridge Hill." We rode there, and, as we turned sharp round from the by-road to the summit of the hill, there stood Mr. H——, ready to welcome us!

I may add, that between whiles we had neither seen nor had any communication either with the Club or with Mr. H——.

EDWARD MARKS.

[I note with interest the experience of others as to coincidences—so odd, did we overlook the immense number of *events* among which they turn up, that the idea *will* suggest itself that there is some special meaning in them.—R. P.]

COOKING CHEESE.

[1216]—Observing in this week's KNOWLEDGE that some one has tried Mr. Williams's suggestion with regard to cheese cookery and failed, give me leave to state that we also have tried it and with complete success. The dish was very palatable, nourishing, and wholesome. May I ask, did Mr. Carter prepare the dish himself, or leave it to his cook? Also, was the baking left to the cook, as much depends on this. I ought, perhaps, to add that owing to difficulty in procuring bi-carbonate of potash in our village, we omitted this item; otherwise, we followed implicitly the directions given by Mr. Williams, and with highly satisfactory results.

ETHEL E. ELLIS.

[1217]—Can only suppose your correspondent, J. P. Carter, was not sufficiently careful in following the instructions of Mr. Williams in his preparation of dish of cheese with bicarbonate of potash, nutmeg, &c., &c.; otherwise he would have been as successful as I was, and enjoyed a tasty, digestible, and inexpensive dish. It would be unfair to Mr. Williams, I think, to let a complaint appear and not put a word in on the other side. R. T. PALMER.

London, April 22, 1884.

THE POTATO IN IRELAND.

[1218]—I have waited until to-day, April 12, to see if any of your readers would bring Mr. W. M. Williams to task for a statement made by him, in his article on the Chemistry of Cookery in your paper of March 28, on the use of the potato by the Irish people. In your issue of April 11, one who signs himself "A Resident Irishman" refers to Mr. Williams's views in same article concerning the absence of fruit-trees and the cause thereof, and points out that by the existing laws farmers are fairly well protected, and that it is not the fault of the laws that fruit-trees are not more cultivated in this country. I shall not refer to this portion of Mr. Williams's paper further than to mention that the county of Armagh is a notable exception in this respect to many other parts of Ireland. In this county, fruit-trees are largely planted, particularly apple-trees—it is quite a common sight to see from one to ten acres under this fruit alone. The rent of the entire farm is frequently paid from their sale. They are usually sold to dealers who come from England and Scotland early in the summer, when the fruit is barely formed, and who purchase the entire orchard, taking all the after risks of the crop. Owing to the absence of sunshine during the past few years, it has not been a profitable business to any of the parties concerned.

The portion of Mr. Williams's paper I consider objectionable reads as follows:—"My own observations in Ireland have fully convinced me of the wisdom of William Cobbett's denunciation of the potato as a staple article of food. The bulk that has to be eaten, and is eaten in order to sustain life, converts the potato-feeder into a mere assimilating machine during a large part of the day, and renders him unfit for any kind of vigorous mental or bodily exertion. If I were the autocratic Czar of Ireland, my first step towards the regeneration of the Irish people would be the introduction, acclimatisation, and dissemination of the Colorado beetle, in order to produce a complete and permanent potato famine." It is to this latter portion of Mr. Williams's views I particularly wish to refer.

The regeneration of the Irish people has been a very prominent theme for some time, from a certain autocratic Czar of Ireland down to the Irish-American Dynamiter, who proposes to effect the regeneration of the country by applying explosives to public buildings in England. Verily the regenerators are numerous and their modes various; but the Colorado beetle is the latest suggestion of all. Coming as it does from a scientific man in the columns of KNOWLEDGE, it should not be allowed to pass unchallenged. I do not know whether Mr. Williams is a young or an old man, but he says he explored the country rather exhaustively. His statement to be true must be carried back to a time previous to the year 1846.

I have been intimately acquainted with the social condition of the Irish people in the northern half of Ireland for more than twenty-five years, and have known a considerable portion of Ulster for a much longer period, and can state from my own knowledge that the people now, and for a long time past, do not live on the potato in the way indicated by Mr. Williams. Since the potato blight of 1846, the Irish people have been gradually dispensing with the potato as a staple article of human food.

There are some districts in the counties of Donegal, Mayo, and Galway where this improvement, however, is not quite so apparent. It would be almost as unjust to make a similar charge against the Scotch, because the crofters of Skye and Lewis live in pretty much the same manner. I admit the potato is still a staple crop in Ireland, and deservedly so, but not a staple of human food as it was previous to the blight. As a proof of the importance of this crop, which Mr. Williams proposes should be destroyed, the loss on

the crop in Ireland, owing to the partial failure in 1879, was over two million pounds sterling. The English people must be fond of potatoes also, as it appears from the Board of Trade returns there was considerably over one million cwt. imported into England from France and Germany alone, during the first three months of last year, while the imports from the same countries this year, for the corresponding three months, is about 295,000 cwt., not much over the one-fourth, Ireland, no doubt, making up the balance from the splendid crop of last autumn. Mr. Williams admits that 14 lb. of potatoes are about equal in nutritive value to a 4 lb. loaf. The value of 14 lb. of potatoes in Ireland this year is from 2½d. to 3½d., according to quality. The 4-lb. loaf is 6½d.; consequently they are relatively much cheaper than bread, and the increase in the exports to England are consequently very marked. Poultry, pigs, and cattle are largely fed with potatoes; they are now the real assimilators, as the farmer finds it profitable to convert his potatoes into fowl, bacon, and beef, for which there is an unlimited demand. The food of the people consists principally of Indian and oaten meal, flour, potatoes, bacon, tea, and in a less degree of eggs and butter, as they are usually sold. The Indian meal makes porridge, also a very wholesome bread is made from about equal quantities of it and flour, using sour butter-milk and carbonate of soda instead of barm. From oaten meal is made porridge, flummery, and oaten cake, which is a very sustaining food. Another kind of bread made in Ireland is potato-cake. It consists of potatoes, finely bruised, mixed with about one-fourth their weight of flour, a little salt added. It is baked on a griddle, and is served up when hot with plenty of butter. It is very agreeable to eat, and quite wholesome. Wheaten bread is also made. Potatoes are used as a general rule only at dinner. The Irish people use plenty of butter-milk, which is the milk that remains after churning and taking off the butter; it is used as well for pigs, calves, and poultry, and can be safely recommended to the human subject as one of the very best medicines for a sluggish liver. The workpeople in the towns live more exclusively on tea and bread and butter; their food is not so varied or so wholesome as that of the country. It is well known that since the introduction of the potato, skin diseases have almost disappeared where they are used. The *Lancet* recommends that shipowners should be compelled to carry them, as one of the best antiscorbutics. Without multiplying words further, it cannot be denied that their moderate use is highly beneficial, and that to talk of introducing the Colorado beetle is simply nonsense. I deny that the Irish people eat inordinate quantities of potatoes; they use more of them, however, than the English, with whom they will compare in every respect—in physique, intellect, and intelligence. Instead of trying to exterminate the potato, which is so valuable for all feeding purposes about a farm, an effort is at present being made by a Belfast gentleman, who has spent years of his valuable time and a great deal of his means to introduce new varieties from the seed, which are more proof against disease than the older sorts.

This gentleman had the co-operation and sympathy of the late distinguished naturalist Mr. Charles Darwin during his various experiments in growing the potato from the potato-apple, and he is reported to have said that the loss of some of these new varieties would be nothing less than a national calamity. I will conclude by expressing a hope that Mr. Williams may favour us with a visit, that he may observe the progress made in the social condition of the people, and see sufficient to cause him to change his views about the extermination of the potato. SEATON F. MILLIGAN.

FRUIT TREES IN IRELAND.

[1219]—If Mr. Mattieu Williams had, in No. 1191, stopped short at ridiculing and denouncing what he calls my "curious theory," I should not have troubled you again. I never meant to do anything so arrogant as to propound a theory. I know all the facts he recites, and will make him a present of one which he has not mentioned, though possibly he is aware of it. There is (or was in 1875, when I saw it) a clump of bamboo in Muckross demesne, Killarney. I have propounded no theory; but I did mean to throw out a suggestion, and I will try to make my meaning clearer now. Does Mr. M. W. think that in Ireland fruit is likely to ripen with such steadiness and certainty that farmers could reckon on it as a source of profit or an auxiliary to diet? If he will say this, I will admit that I have been wrong in my *conjecture*. Theory I have none, and I am willing to defer to Mr. M. W. as an expert.

If I were the arrogant person he supposes me to be, I should, in my turn, ask him to defer to me, as one who has given special study to the laws of land tenure and the relations of landlord and tenant in Ireland. I do not ask this; but I think I am justified in challenging Mr. M. W. to name a sufficient number of instances where farmers' rents are raised, or a rise of rent was threatened, because fruit trees were planted on the farm. There are more than 500,000 tenant farmers in Ireland. Does Mr. M. W. know of 500 cases—one case in a thousand? Does he know of one case in 10,000? Can he name fifty cases where this was done or attempted? It is very remarkable that no mention of anything of the kind occurs in the evidence of the Richmond and Bessborough commissions, or in the pamphlets of Mr. Baldwin, the great champion of the small farmers.

I cannot ask you for sufficient space to expose the mis-statements and bad reasoning of Mr. M. W.'s concluding paragraph. But I must protest against the introduction of the late Earl of Leitrim's name. I cannot, and would not if I could, undertake to defend his conduct to Lord Carlisle; but what has an incident which took place in 1863 to do with the Land Act of 1870? Mr. M. W. writes that I "must know how the Earl of Leitrim publicly announced his intention of defying and violating the law of 1870, and how he proved his feudal power over his tenants by commanding them to publicly and ostentatiously insult the Lord Lieutenant, &c., &c., &c. . . . This was done to demonstrate his contempt for that same law which . . . any one of his tenants could enforce against him." Would not any one believe, reading Mr. M. W.'s words, that (1) Lord Leitrim *first* declared his intention to violate the law of 1870, (2) *then*, to show his contempt for the law of 1870, insulted the Viceroy, and (3) carried out the insult by a public demonstration of his tenants? This is the natural meaning of the words as they stand—What are the facts? The insult to the Viceroy, Lord Carlisle, took place in 1863, seven years before the Land Act of 1870 was enacted. It was carried out through a *single tenant*, who was a common innkeeper, and may not, for anything on record, have held a rood of land under Lord Leitrim. The licence of the inn was taken away at the next meeting of licensing justices. Mr. M. W. must be infatuated in his hatred of Irish landlords when he has recourse to such weapons of attack as this. It is quite true that Lord Leitrim did defy the Act of 1870; but he was in all respects an exceptional man. To quote him or any act of his as typical of Irish landlordism proves either ignorance or hastiness of judgment. Ignorance cannot be presumed in Mr. M. W.'s case. He has "explored Ireland" and edited Murray's Handbook—my authority, by the way, for the date and for some of the facts mentioned above.

I never accused Mr. M. W. of "joining in denunciations of the Saxon." I said, and I repeat, that he has lent his ears to false charges against Irish landlords as a class, and that he is now lending his pen to disseminate the like falsehoods. I am willing to believe that he thinks they are truths; but that is because he has not examined the evidence. Judging a whole class by an exceptional individual, he has accepted without inquiry any story told him to the discredit of that class.

Possibly Mr. Williams may refuse to meet my challenge, alleging that it is the *fear* of landlord "brigandage," and not the actual fact, which kept men from planting. If so, how does he account for fruit-trees on farms in England where there is no Registration Act, and no Act of 1870, but the fruit-trees are "the landlord's property"? This opens up the whole question of tenure and the relations of landlord and tenant in the two islands, and you will say that KNOWLEDGE is not a legal or political paper. That is not my fault. Mr. Williams was the first to introduce legal and political matter in his cookery article, and his letter (1191) repeats and adds to the irrelevancy. RESIDENT IRISHMAN.

MAN DURING THE GLACIAL PERIOD.

[1220]—With regard to Mr. Kirkdale's letter inquiring the *locality* of any glacial deposits containing remains of human workmanship, parenthetically referred to in my article on "Effects of the Glacial Period," and my *authority* for such reference, I may mention specially Prof. Rutimeyer's description of certain traces of man in the interglacial deposit near Wetzikon, in the Canton of Zurich, Switzerland. Such traces consist of pointed lignite rods, considered by Prof. Rutimeyer, who, along with his colleague, Prof. Schwendauer, very minutely examined the same, as undoubtedly the remains of some rough basket-like structures. They were discovered by Dr. Scheuermann in a lignite formation at Wetzikon, overlaid by a vast glacial deposit, described some years ago by Prof. Arnold Escher de la Linth. The lignite also rests upon a bed of an erratic nature, and is, in fact, an "interglacial" deposit. Besides the lignite rods referred to, remains have been found in this deposit of the elephant, rhinoceros, urus, and other animals often found along with the implements of man in other parts of Europe. ROBERT B. COOK.

SCIENTIFIC MORALITY.

[1221]—Although I have been a constant reader of KNOWLEDGE from the beginning, and have been interested to watch its development, I have not hitherto troubled you with any observations. I can no longer refrain, however, from expressing the satisfaction it has been to see that you do not exclude conduct and morals from the domain of scientific knowledge. I heartily agree with your

correspondent, Mr. T. Common, when he says, "Scientific morality is the true antidote to revolutionary ideas." But, what I wish to draw attention to is that this antidote must be administered *before* the poison has taken hold of the system; that is to say, *children* must be taught the principles of right conduct, and trained to act in accordance with those principles, before they become entangled, as men and women, in the meshes of revolutionary or communistic ideas.

That such teaching is practicable has been amply proved in the Birkbeck Schools (founded by my grandfather, the late Mr. William Ellis), where the systematic teaching of the "Conditions of Well-being" is a prominent feature. I myself know by experience that even young children can be brought to understand and take delight in such lessons, which, in the words of Mr. T. Common, "may determine the whole course of a person's life."

Mrs. Fenwick Miller (M.L.S.B.) has been struck with the importance of such training, and has lately made an attempt—but partially successful, I regret to say—to introduce it to our Board Schools. Perhaps, some day, they will decide that conduct and duty teaching is as needful as perspective drawing and Shakespeare; but, as the public seem hardly ripe for this yet, it is to such enlightened pioneers as KNOWLEDGE that philanthropists must turn for aid in arousing people to a sense of the importance of teaching Scientific Morality. ETHEL E. ELLIS.

WOLFSON *VERSUS* BIRLEY.

[1222]—It is not because my name has been incidentally mentioned in this matter that I now venture to claim permission to trespass on your space, but for the very good reason that I have been frequently, and am now, by many who ought to know better, regarded as the individual publicly known as "Parallax." But I have never in my life used this or any other name than my own. And, much as I am indebted to him for helping me to discover one of the greatest impostures of the day, I have not the slightest ambition to be identified with his unaccountable reluctance to pass in his own name. His conduct to me individually has been the very reverse of generous or equitable. Some twelve or fifteen years ago, when "Parallax" had not, perhaps, as many sixpences as he now has pounds, I paid him some £130 or £140, when I thought of placing my son as his pupil, besides spending an additional £30 in advertising his book by a pamphlet of extracts, entirely trusting to his honour to make me some compensation, if the time should ever arrive, when he should be able to do so. All the return I have had has been one pint bottle of his phosphorised medicine, and the privilege of having my name identified with the grand cause of which he is the unquestionable founder. I have letters from him at this very time, acknowledging how cordially and highly he approved of the selections I made from his book.

From these facts it may be gathered that I have not the slightest reason to place myself before your readers as Dr. Birley's champion, much as I admire him as a true scientist, and also for his medical skill, unprofessional as it may be. He is only one out of hundreds of others who are deservedly making good incomes out of medical compounds that may not be intrinsically worth much more than the glass bottles which contain them. When rich and suffering people are cured, they do not care about the actual cost of the physic so much as the cure effected. As for exposing the Zetetic cause, Mr. Wolfson may [as well], as I have told him, attempt to blow up Hampstead-hill with wet sawdust. And his inevitable failure will, I trust, be a lesson to him not [to] make threats till he has made sure of his ability to carry them into execution. With respect to the chief cause of his disagreement with "Parallax," I suspect [but Mr. Hampden's suspicions are not evidence, and must be omitted.—R.P.].

But, as I said before, Dr. Birley ("Parallax") has no claim on my interference on his behalf; and why he is silent, I cannot for a moment conjecture. But a man of 89 years ["Parallax" certainly did not look 69 at Plymouth in 1864. Surely Mr. Hampden must mistake, or "Parallax's" medicine has aged him tremendously in the last twenty years. I should have said he was not 60 then.—R.P.] is less inclined for letter-writing than he was half a century ago. Yet I think Mr. Wolfson ought to be grateful for his forbearance, if my informant is correct. [Mr. Wolfson is not enthusiastically grateful to "Parallax"; so probably Mr. Hampden's informant is *not* correct.—R.P.] JOHN HAMPDEN.

[I have much pleasure in finding space for Mr. Hampden's letter. He surely ought not to be confounded with "Parallax," considering thep mphlet in which the latter sat upon him, after the manner of speaking, for the Bedford Level wager. There "Parallax" describes very naïvely the simple way in which Mr. Hampden handed himself over as an early victim of the most childish paradox which has ever yet, probably, been propounded. I sympathise heartily with Mr. Hampden for the loss of his money, and still more for the association of his name with a cause which has so long been a laughing-stock for intelligent persons. (I have myself never heard it spoken of except with well-deserved ridicule.) But I venture to congratulate Mr. Hampden on the limited allowance of phosphorised medicine which he permitted himself to imbibe. All such medicines are dangerous when administered by unprofessional persons. Perhaps outsiders may understand *now* why "Parallax" has advocated an astronomical theory which—as I have again and again pointed out, both here and in the *English Mechanic*—he does not himself believe in (for otherwise he would not bolster it up with pretended and impossible experiments, and carefully garbled extracts). They may also understand why I have thought it worth while to show the absurdity of a theory which in itself is not worth the trouble of attacking.—R. P.]

LETTERS RECEIVED.

SENEX. Mr. Croll is quite right. The perihelion does not advance regularly, as so many of the books say, and often retrogrades. The length of the anomalistic year necessarily varies as these changes take place. I have thought of giving occasional diagrams showing the positions of the planets. Once a month would be unnecessary,—and the four outer planets could be sufficiently dealt with once a year.—THOS. AYERS. Hope the army of geologists will not utterly destroy you.—AB. HOLDEN. John Hill's "Arithmetic," published in 1750, may be mentioned in the catalogue of the British Museum.—W. H. FUNLING. Could not well publish astronomical notes a month in advance. The Kite problem would take up too much time and space. It is not by any means easy; but those who could follow the solution would be able to solve it for themselves.—J. C. MURRAY AYNSLEY. Your letter is marked for insertion.—H. WOODROFFE. On January 1, 1800, only 1799 years and part of a day had passed from the epoch when the present method of reckoning years starts. Now 1799 years are not eighteen centuries. It is clear that not until the end of December 31, 1800, have eighteen centuries been completed. Hence January 1, 1801, was the first day of the present century.—JOHN BELL. Thanks for kindly wishes. The price of KNOWLEDGE runs so close to cost of production that it would be impossible to bring out such supplementary illustrations. Consider, the *Illustrated Science Monthly*, which claims to be the cheapest science paper going, gives for sixpence no more *in mere quantity* than we gave last week for threepence, and its writers are not quite so well known as Messrs. Slack, Grant Allen, Williams, &c.—A. F. RAVENSHEAR. Fingers, &c., in due course.—H. Mystery of the infinitely small! Who can solve it?—E. BASIL LUPTON. The sun and moon appear larger near the horizon by an optical illusion,—the horizon seeming far away than the region overhead, yet the sun and moon no smaller. Hence they seem nearer than the sky, and the mind somehow translates this into seeming larger, being misled by the contrast between what is seen and what had (unconsciously) been expected. When measured by any angle-measurer the discs are found to be no larger, the moon's perceptibly smaller.—F. A. M. The proof that gravity acts with very much greater rapidity than light travels is not very difficult. A little consideration will show you that, if gravity acted as rapidly as light travels, the action of gravity would not tend towards the sun's centre, any more than the rays of light which show us the sun show his real place. There would be a constant force tending to accelerate the motion of the earth in her orbit.—L. KENNEDY. I can only say that I would not believe my own senses if such evidence seemed given me by them.—M. J. HARDING. The changed quality of paper will not affect the look of volume; the thinner paper was always used for numbers having 32 pages.—H. A. BULLEY. (1) All vulcanian phenomena are attributed by Mallet, Dana, and others, to heat generated by the mechanical displacements as the earth contracts. (2) Do not know the origin of the word CAUCUS: the Greek origin you suggest seems, as you say, rather wanting in connection.—J. H. M. (or J. HILL). Printers' errors cannot always be corrected in a paper coming out weekly. Articles may not be seen in proof, or corrections may be not quite correctly made. With books it is different because revises are seen. Scintillation has been treated since you wrote from Bombay.—J. H. Your coincidence story is curious. I have put it as a letter with names omitted as you wish.—F. STIFF. Thanks; but extract had already reached us.—V. A. WRAIGHT writes that he would be glad if any readers of KNOWLEDGE would furnish him with a few instances of catalysis.—LIEUT. M., ROXY, SW., and ST. HELEN'S. Thanks for the information obligingly conveyed; we have, however, more precise information still: his past has been traced farther back than you seem aware.—J. HALSWELL. The derivation of Jack o' Dover from *chef-d'œuvre* is likely enough to be correct.—POLYGLOT. Your article on the Patent Law shall be forwarded to you in type for correction. Thanks.

Our Mathematical Column.

NOTES ON EUCLID'S FIRST BOOK.

BY RICHARD A. PROCTOR.

(Continued from p. 276.)

PROP. 32 is very important, as are its corollaries. It is well to notice that the second corollary gives the easily remembered result that—

Each of the exterior angles of a regular polygon of n *sides, is equal to* $\frac{4}{n}$*ths of a right angle.*

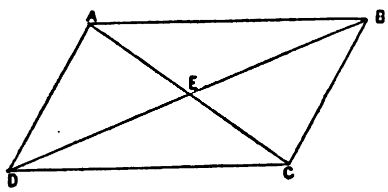
We shall refer to this result as Euc. I., 32, cor. 2, *schol.* As an instance of its application, take the following,—

Each exterior angle of a regular heptagon is $\frac{4}{7}$ths of a right angle; hence each of the interior angles is equal to $\frac{10}{7}$ths. (Euc. I., 13). Hence, also, the sum of the interior angles of a heptagon is equal to ten right angles.

The method here followed is the most convenient in cases of this sort, being so easily remembered.

To the properties established in Prop. 34, and their converse theorems, we may add the following,—

PROP. XVIII.—*The diagonals of a parallelogram bisect each other.*



Let A C, B D, the diagonals of the parallelogram A B C D, intersect in E. Then shall A E be equal to E C, and D E to E B. In the triangles A E D, B E C, the vertical angles A E D, B E C are equal, the angle A D E is equal to the alternate angle E B C, also A D is equal to B C. Hence (Euc. I. 26) the triangles are equal in all respects. Therefore A E is equal to E C, and D E to E B.

The converse of this property is also true. It is,—*If the diagonals of a quadrilateral figure bisect each other, the figure is a parallelogram.* It is clear that if A E is equal to E C, and D E to E B, then since the angle A E D is equal to the angle B E C (Euc. I. 15), the triangles A E D and B E C are equal in all respects (Euc. I. 4). Hence A D is equal to B C and the angle D A E is equal to the angle E C B; therefore A D is parallel to B C (Euc. I. 27): and since A D is equal and parallel to B C, A B is equal and parallel to D C (Euc. I. 33): therefore A B C D is a parallelogram.

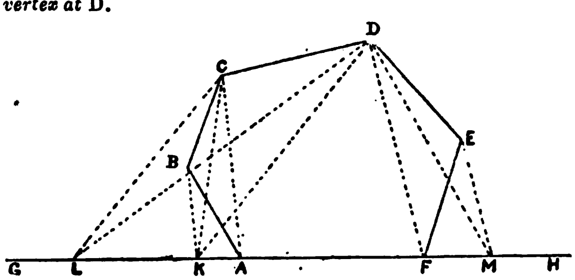
We shall refer to this proposition as Prop. XVIII. *conv.*

PROP. XVIII. and its converse are propositions of great utility, and the student should always be on the watch to apply them in problems of a certain class. Examples will be given further on.

Props. 35—41, and the converse theorems already discussed, are of frequent application.

Props. 42, 44, 45, have a useful application in surveying; since by taking the given angle as a right angle we learn how to reduce any rectilinear figure into a rectangle of equal area,—and, if necessary, of given length or breadth. The fourteenth proposition of the second book shows us further, how to construct a square equal to any given rectilinear figure. Hence these propositions may be looked on as completing the theory of the quadrature of rectilinear figures. But the following method of reducing a rectilinear figure to a triangle of equal area is worth noticing for several reasons.

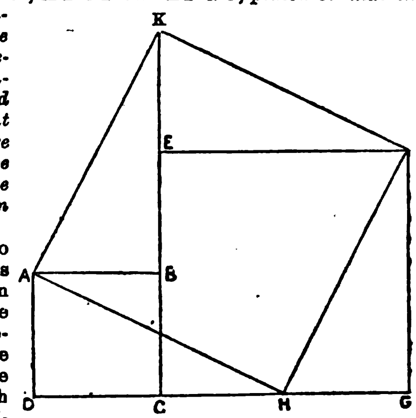
PROP. XIX.—Prob. *To reduce a rectilinear figure* A B C D E F *to a triangle of equal area, having its base in the line* A F *produced and its vertex at* D.



Produce A F indefinitely either way to G and H. Join C A, through B draw B K parallel to C A, and join C K; then the triangle A K C is equal to the triangle A B C, and (adding A F E D C to these equals) the figure F K C D E to the given figure A B C D E F. Again, join D K, draw C L parallel to D K and join D L; then the triangle K L D is equal to the triangle K C D, and (adding K F E D to these equals) the figure F L D E to the figure F K C D E, that is to the given figure. Lastly, join D F, draw E M parallel to D F and join D N; then the triangle D F M is equal to the triangle D E F, and the triangle L D M to the quadrilateral L D E F, that is to the given figure A B C D E F. And, in like manner, a figure of any number of sides may be reduced to a triangle.

It is hardly necessary to point out the importance of props. 47 and 48. The following problem, forming a well-known "puzzle" exhibits an interesting proof of the 47th proposition.

Let there be two squares, A B C D *and* E F G C, *placed so that the sides* D C, C G *are contiguous and in one straight line, and therefore* B C *and* E C *coincident. It is required to draw two straight lines, dividing the figure* A D G F E B *into three portions, which can be so combined as to form a single square.*



Take D H equal to E F, so that H G is equal to D C. Join D H and H F; then the figure is divided as required. For let the triangle A D H be placed as at A B K with its right angle coincident with the right angle A B E, the side A D being so placed as to coincide with A B. Join K F. Then B K being equal to D H, that is to E C, E K is equal to B C (that is, to H G); also E F is equal to F G, and the right angle K E F to the right angle H G F. Hence the triangle K E F is equal in all respects to the triangle H G F. Thus the figure A K F H is made up of three figures equal to those into which the figure A D G F E is divided by the lines A H, H F. Also A K F H is a square. For the four triangles A D H, F G H, K E F, and A B K being obviously equal in all respects, the four lines A H, H F, F K, and K A are equal. And each of the angles of A K F H is a right angle. For the angle K F H is equal to E F G since K F E is equal to H F G; K A H is equal to D A B, since K A B is equal to D A H; and A K F is the sum of the angles A K B, E K F, that is of the angles K F E, E K F, which together make up a right angle (Euc. I., 32): hence also A H F is a right angle, and A H F K is a square.

Of course this problem is, in effect, a proof of Prop. 47.

EASY RIDERS ON EUCLID'S FIRST BOOK,

WITH SUGGESTIONS.

PROP. 23.

65. If one angle of a triangle be equal to the sum of other two the triangle can be divided into two isosceles triangles.

66. Construct a triangle having given the base and the two angles adjacent to the base.

67. Construct a triangle having given the base, an angle adjacent to the base, and the sum of the two sides.

68. Construct a triangle having given the base, an angle adjacent to the base, and the difference of the two sides,—first when the greater side is adjacent to the given angle, secondly when it is opposite to that angle.

PROP. 24.

69. Prove that the point F in the figure to this proposition falls below E G.

70. Show how the proof of the proposition may be completed without assuming that F falls below E G.

In dealing with the assumption that F may fall within the triangle D G E, apply Euc. I. 21.

A BOARD of Trade report has been issued on the explosion of a vertical steam boiler in a printing office at Great Yarmouth, Norfolk. The boiler was reduced to from 0·094 in. to 0·125 in. in thickness, and burst at some pressure between 40 lb. to 50 lb. In his evidence the foreman said:—"I did not know whether it was fit for that pressure or not. About six or seven months ago I held the safety valve down by hand, and raised the pressure to 70 lb. by gauge. After that I was satisfied that the boiler was safe." This foreman, thinks the *Engineer*, would probably look for a gas escape with a lighted candle, put himself on ice to see if it were strong enough to carry considerable weight, and put a match on a keg of gunpowder to see whether it was wet or dry.

Our Chess Column.

BY MEPHISTO.

ANSWERS TO CORRESPONDENTS.

**** Please address Chess Editor.*

S. B. B.—Received with thanks.

Correct solutions received.—Problem I., p. 278, Donna, M. T. Hooton, W. Hanrahan, Julius Wahltuck, W., John Watson. No. II., John Watson, W., M. T. Hooton, Punch. No. 116, M. T. Hooton, W.

Solutions of Ending, p. 299, received from C. H. Brockelbank, W. H. L. M. (Dublin), and W.

Two of our correspondents assert that White's victory is more apparent than real; in other words, that in spite of White forcing a Pawn to Queen, he can only draw. We shall fully enquire into this next week. But as this does not affect the idea of the Problem, we still invite solutions.

ONE of twenty games played simultaneously at the North London Chess Club, Mephisto winning seventeen, drawing one, and losing two (to Messrs. Biaggini and Griffiths). Time, 3½ hours.

ALLGAIER THOROLD.

White. Mephisto.	Black. C. H. B.	White. Mephisto.	Black. C. H. B.
1. P to K4	P to K4	13. Kt to R3	B to K3
2. P to KB4	P × P	14. P to B3	P to KR4 (c)
3. Kt to KB3	P to KKt4	15. B to B2	Kt to KR3 (d)
4. P to KR4	P to Kt5	16. Q to Q2	B × B (e)
5. Kt to Kt5	P to KR3	17. R × B	R to B sq
6. Kt × P	K × Kt	18. QR to KB sq	Kt to Q2
7. P to Q4	P to Q4	19. R to B6 (f)	R × R (g)
8. B × P	B to Q3 (a)	20. Q to Kt5 (ch)	K to B sq
9. P to K5	B to K2	21. Q × Kt (ch)	K to K sq
10. B to Q3 (b)	B × P (ch)	22. P × R	Kt × P (h)
11. P to Kt3	B to Kt4	23. R × Kt	Resigns
12. Castles	K to Kt2		

BLACK.

WHITE.

Position after Black's 15th move.

NOTES.

(a) This move has the merit of avoiding the various attacks following upon Black playing P × P by B to B4 (ch). Nevertheless B to Q3 cannot be considered a good move, as by P to K5 White obtains a strong centre and gains time.

(b) No doubt P to KKt3 was a safe move, but B to Q3 has a relative value, inasmuch as it gains time for development, and prevents QB to B4.

(c) To enable him to play his Knight to R3, and, if necessary, to play B to B4 and R to B sq.

(d) See Diagram.

(e) 16. Kt to B2 looks a safe move. White would reply with R to B2, to be followed by QR to KB sq.

(f) Threatening, Q × Kt or Q to Kt5 (ch).

(g) Black has nothing better. If Kt × R, then Q to Kt5 (ch) would win. Kt to B2 would likewise lead to loss.

(h) White might have continued with 22. Kt to B sq 23. $\frac{\text{Q to Kt7}}{\text{Q to Q3}}$ 24. $\frac{\text{K to Kt2}}{\text{P to R3}}$ 25. $\frac{\text{R to K sq}}{\text{K to Q sq}}$ &c.

PROBLEM No. 117.

BY H. W. SHERRARD.

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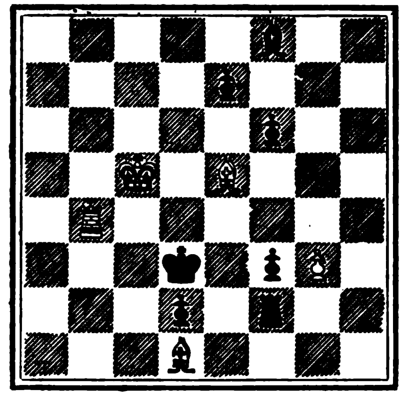

WHITE.

White to play and mate in three moves.

PROBLEM No. 118.

BY F. J. LEE.

(Habitué of Pursell's.)

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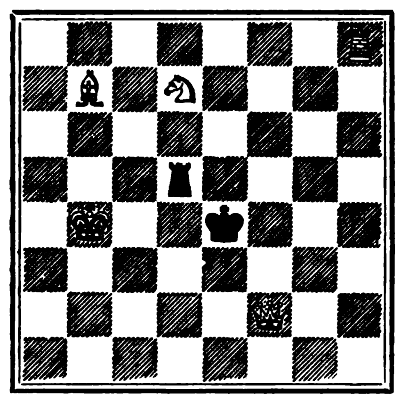


WHITE.

White to play and mate in two moves.

(A first attempt.)

SOLUTIONS.

PROBLEMS BY S. LOYD, p. 278.

I.*

1. B to Q5 (ch)
   - 1. K × KB — 2. Q to B6, mate
   - 1. K × QB — 2. Q to K2, mate
   - 1. K × P — 2. Q to B6, mate

II.

1. $\frac{\text{Q to K7}}{\text{K to B4}}$	2. $\frac{\text{P to B3}}{\text{K × P (a)}}$	3. $\frac{\text{Q to B5}}{\text{Mate}}$	(a) if P × P	3. $\frac{\text{Q to B7}}{\text{Mate}}$
1. $\frac{\text{Q to K7}}{\text{K × P}}$	2. $\frac{\text{Q × BP}}{\text{K × P (a)}}$	3. $\frac{\text{Q to R8}}{\text{Mate}}$	(a) if P to Kt5	3. $\frac{\text{Q to R4}}{\text{Mate}}$
1. $\frac{\text{Q to K7}}{\text{P to B4}}$	2. $\frac{\text{P to B4}}{\text{P × P (a)}}$	3. $\frac{\text{Q to R4}}{\text{Mate}}$	(a) if K × P	3. $\frac{\text{Q × P}}{\text{Mate}}$
1. $\frac{\text{Q to K7}}{\text{P × RP}}$	2. $\frac{\text{Q × BP}}{\text{P to R5}}$	3. $\frac{\text{Q to B3}}{\text{Mate}}$		
1. $\frac{\text{Q to K7}}{\text{P × KP}}$	2. $\frac{\text{Q × KP(ch)}}{\text{K × P}}$	3. $\frac{\text{Q to R3}}{\text{Mate}}$		

PROBLEM No. 116, BY H. W. SHERRARD.

1. $\frac{\text{R to K6}}{\text{K to Q4}}$ 2. $\frac{\text{R to B6}}{\text{K to Q5}}$ 3. $\frac{\text{K to K6}}{\text{K to K5}}$ 4. $\frac{\text{R to B4}}{\text{Mate}}$

* This Problem is the only two-mover I remember to have seen which begins with a check, and yet is worth examining. It was for this reason I selected it.—R.P.

Our Whist Column.

By Five of Clubs.

AN AMUSING HAND.

THE following hand appeared in the *Westminster Papers* on September 1st, 1877. It is described by the editor as showing how "to induce your partner to throw up his cards with the game in his hands."

THE HANDS.

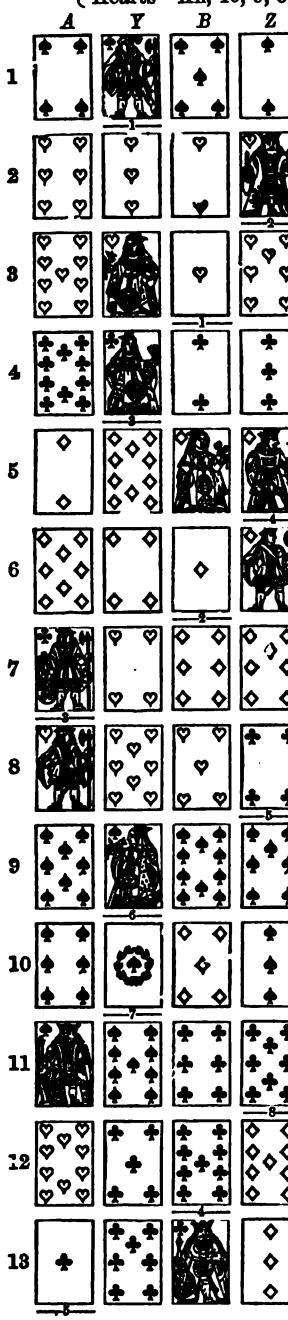
B { *Clubs*—K, 9, 6, 2. Hearts—A, 5, 2. Diamonds—A, Q, 6, 5. Spades—10, 5. }

Y { *Clubs*—Q, 7, 5. Hearts—Q, 8, 4, 3. Spades—A, Q, Kn, 9. Diamonds—10, 4. }

B A-B, 3.
Y Y-Z, 4.
Z Trump. C. 4.
A Leads.

3, 4, 8—*Clubs* 7, K—Hearts 2, 3, 7—Spades 3, 7, 9, Kn, K—Diamds { Z

A { *Clubs*—A, Kn, 10. Hearts—Kn, 10, 9, 6. Diamonds—8, 2. Spades—K, 8, 6, 4. }



NOTES AND INFERENCES.

1. *A* should have led his Heart Knave: Hearts are his best suit. *B* from his own hand and the fall of the cards infers that *A* has led from a long weak suit.

3. *B* infers that the winning Hearts are probably with *A*.

4. *A* makes an inexcusable finesse, and in trumps, led by his partner! *B* infers with confidence that the Ace lies with either *Y* or *Z*.

5. It is a fair inference from *Y*'s change of suit that he does not hold the trump Ace. But this is not yet certain.

6. *B* infers that *A* and *Y* have no more diamonds. *Y* certainly has none; therefore *Z*'s Knave cannot have been the highest of two left: it would therefore not have been led unless the highest of a sequence,—that is, unless (ten having been played) *Z* held the nine. Hence *A* can have no more.

7. *B*, very properly, forces his partner. It is now clear to him that *Z* holds the Ace of trumps.

9. At the end of this round *B* threw up his hand. It seemed clear from the play that besides the Ace of Spades, *Y-Z* must hold the Ace of trumps. It was therefore apparently idle to continue the contest.

10. *B*'s Diamond is called.

11. His smallest trump is called.

12, 13. *Y-Z* make two by cards, and win.

Obviously *A-B* would have won the game had not *A*'s preposterous play deceived *Z* utterly. But the game has another moral:—

Never throw up your cards while there is any possible chance of saving the game, even though that chance may be that your partner has played very badly. Even a good player might through mishap have played as *A* did,—either taking hold of the wrong card at trick 4, or overlooking the Club Ace in sorting. Both would indicate great carelessness, but sometimes even good players are careless.

ERRATUM.—In the game last week the King and Queen of Clubs (trumps) should change places.

THERE is a very fine set of bells in the Clock Tower of the Royal Courts of Justice, but, says a contemporary, they are so completely boxed in that only a muffled ghost of their fine tones can wriggle its way through the nearly closed louvres.

THE total number of coal mines of all kinds in operation in 1883 was 3,707, against 3,814 in the previous year. There was, therefore a decrease of 117 in the number of mines at work, concurrently with an increase of 7,237,350 tons in the quantity of coal produced.

THE total number of hands employed in the mines of the United Kingdom in 1883 was 514,933, against a total of 503,987 for 1882. This corresponds to a production of 346 tons per man in 1883, and and of 339 tons per man in 1882. It would appear, therefore, as if the workmen had generally done better average work in the former than in the latter year.

TELEPHONE companies still endeavour to meet the demand for underground wires by asserting that communication through such wires is impossible, except for two- or three-mile lengths, on account of mutual induction. The postal telegraph department is, however, using in their telephonic system underground wires upwards of fifteen miles in length.

IN the lead production of different countries Spain still holds the first place, the amount reaching some 120,000 tons in one year, or one-sixth more than America, which comes next on the list, while Germany follows with 90,000. Of Spain's total production some 67,000 tons are derived from one district, that of Linares, in which more than 800 mines are registered. Of this large number, however, only a comparatively small proportion are actually worked on a large scale, and there are only about fifty in which steam power is used. The total number of steam-engines employed is stated to be 180, nearly one-half of this number belonging to English companies; in fact, most of the mining machinery and pumping engines in all the mines are of English make.—*Engineer.*

CONTENTS OF No. 130.

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MAGAZINE OF SCIENCE
PLAINLY WORDED-EXACTLY DESCRIBED

LONDON: FRIDAY, MAY 9, 1884.

CONTENTS OF No. 132.

THE COMMON NEWT.

BY GRANT ALLEN.

IN the spring, according to the Poet Laureate, the wanton lapwing gets himself another crest. Just about the same time, the little water-newt, though unsung of bards, does precisely the same thing, and for precisely the same reason. There is hardly a clean pond or pool in England where one cannot find these pretty and common small creatures with a very little hunting; and yet I am often surprised to find how few people, comparatively, are at all on speaking terms even with *Triton punctatus*. At the present moment they are in their very best new spring finery; and it is well worth while looking for them, or their lordlier cousin, the great water-newt, before time has begun to dull the splendour of their first crimson and orange wedding-suit. All you have got to do is to stand on the edge of the pond, and peer down for a moment, where the water is clear, on to the soft, muddy bottom. There you will see lurking on the grey ooze, or floating half-way up among the waving tresses of water milfoil or crowfoot, a small, brown, lizard-like form, ill-defined against the similarly-coloured background, but discernible after a minute as a darker patch of shade upon the pale clay of the bottom. Put your hand down cautiously into the pond, approaching him, if possible, from in front (for he darts away forward, and is a bad hand at turning sharply), and you will probably find him the easiest water animal to catch and hold in all England.

When you have got him out and can examine him at your leisure, you will observe that he has a long crest down his back and along his lizard-like tail; and if he is just in his full prime, the jagged tips of the crest will be tinted with crimson, to make him still more attractive to his fastidious mates. In the autumn, these crests disappear altogether, being absorbed into the body of the animal; for then food is scarce, and no useful material can be wasted on mere external decorative adjuncts. But when the spring and the breeding season come round again, and when worms, and pond snails, and water insects are once more plentiful, the little newt begins to wax fat and kick, and to array himself in his wedding garment. His crest grows anew along his back and tail, and at the highest point of vigour it assumes its crimson tags, so that he becomes for a while a very handsome and ornamental little amphibian indeed. If you turn him over on his back (a position which he doesn't much relish, and from which he manages to wriggle right side up again with marvellous rapidity) you will see that his under side is suffused with a very bright orange hue, which, when the crest is crimson, assumes a tone of the most vivid brilliancy.

Of course, these beautiful colours and decorative appendages have been acquired by the male newts through the long-exerted selection of the handsomest among their ancestors by the females of their kind during an innumerable series of earlier generations. This is very well shown by several converging lines of evidence. In the first place, the males are much less frequent than the females, and Mr. Darwin has pointed out that decorative devices of this sort most frequently occur among polygamous species; because there each male has to acquire and preserve for himself a considerable harem, which he can only do by the combined attractions of superior strength and beauty. Then, again, the crest and colours are peculiar to the males, and do not occur, or only occur to a much less marked extent, among their mates. Once more, these decorative devices appear in the breeding season, culminate with the period of spawning, and disappear again upon the approach of autumn. I do not know whether battles have ever been observed between the male newts, as usually happens with decorated animals; but the great crested newt, at least, I am sure is a very pugnacious creature, and his characteristic attitude of craning his head and showing fight when taken out of the water has evidently given rise to the common belief of boys and country folk that "newts spit fire."

The female newt lays her eggs usually on a folded leaf of some water-plant, which she gums together into a sort of pouch by means of a sticky secretion. Sometimes, however, she hides them in the angle between a leaf-stalk and the stem, where they are almost equally safe from the attacks of their enemies. The tadpoles, when first hatched, may be looked upon as almost the nearest approach of any existing vertebrate animals to the common ancestor of the whole vertebrate race. They are tiny, helpless things, with long, thin, flat tails, external gills like much-divided leaves, and a pair of wee claspers by which they hang on to the stems of water-plants. As the tadpole grows, however, the claspers are gradually absorbed, and the fore-limbs, which at first were mere rudimentary fins, lengthen out into legs with minute knobby toes at their extremities. By-and-bye, the hind feet and legs also make their appearance, and the tadpole is now quite lizard-like in shape. Still, like all the amphibians in their early stages, the young newt is even so essentially a fish in the most important points of structure. But as soon as the tadpole has reached its full development a wonderful piece of evolution enacts (or rather recapitulates) itself before our very eyes. The external gills grow smaller and smaller, till at last they disappear altogether. Then the clefts in the neck by which is expelled the water taken in at the mouth grow covered with skin; and, finally, the newt turns from a water-breathing, fish-like tadpole, to an air-breathing adult amphibian. For the most part, the full-grown newts stick to their native pools, only coming up now and again at considerable intervals for a fresh pull of air: for being sluggish and cold-blooded creatures, they don't need any large amount of oxygen to carry on their tardy bodily movements and functions. But a fair number quit the water altogether, and creep over damp grass, or even stray into neighbouring cellars. I am sorry to say that when found in the latter situation, a horrid superstition, still rampant among housewives, condemns them to be

thrown into the back of the kitchen fire. Like their allies, the south European salamanders, they have a bad reputation, wholly undeserved; they are accused of being poisonous, of being unlucky, and most extraordinary of all, of being incombustible. As a matter of fact, they are absolutely harmless little creatures, and they help to keep the cellar clear of slugs, grubs, and small insects.

The tailed amphibians, such as the newts, are among the most interesting links in the whole of existing nature, as they lead us on in so unbroken a chain from the ganoid fishes to the frogs and true lizards. The West African Lepidosiren, a fish with rude legs (to put the matter plainly), and provided with both gills and lungs, comes very close to the Proteus, that strange, underground amphibian of the Carinthian caves, a newt, so to speak, which never passes beyond the tadpole condition, but retains both gills and gill-slits throughout its whole life. Above the Proteus, again, we get such other intermediates as the Amphiuma of the Carolinas, which lose the external gills in the adult state, but still retain the gill-slits, thus going through what may be called an imperfect metamorphosis. Then, once more, we have creatures like the Mexican axolotl, which does not usually undergo any metamorphosis at all, but which, under exceptional circumstances, gets rid of its external gills, and turns into something very much like a common salamander. Finally, we get our English newts, which can wander about on dry land if so they will; and, still more advanced, the black salamander of the Alps, which never goes near the water at all, and in which accordingly the tadpoles undergo their metamorphosis within the body of the mother. These perfectly terrestrial amphibians (excuse the obvious bull, for which scientific nomenclature rather than the present perpetrator is responsible) really differ from the lizards only in anatomical points, and though they cannot be regarded as in the strict sense a connecting link between the two groups, they at least serve to show us by analogy the steps whereby lizards may have developed from a common fish-like amphibian ancestor. Thus our little pond-newt helps us to reconstruct mentally the line of descent whereby man and all the other higher mammals have ultimately been derived from a primitive lung-bearing ganoid like the Lepidosiren.

HOW TO GET STRONG.

MUSCLES OF THE UPPER THIGH.

(*Continued from p. 279.*)

We have already noted that jumping is an excellent exercise for the upper thigh muscles; and that it is far better to practice easy jumping for some time than to try for a short time how high one can jump. The Hon. Geo. Lawless's practice (see "Frank Fairlegh"), however unsatisfactory as a cure for a wounded heart, may be confidently recommended as a remedy for weak thighs. Put a light stick across from one chair-seat to another, placing it so that it will fall if not cleared, and then if your room is large enough run lightly round and jump over the stick, continuing your circuits till you begin to feel tired and out of breath. Mr. Lawless, if I remember rightly, placed the stick across the tops of the chair-backs, and ran backwards and forwards, instead of round and round. But then he was probably in better jumping condition than most of our readers, who would be content with his number of jumps (100) over the height of the chair-seat. Take the jumps as lightly and easily as possible. Try to do it so that any one in the room underneath would not know you were jumping at all; you will fail in that probably, but your efforts will not be unrewarded nevertheless.

Steady running, not too far but carried on daily, is excellent for the thighs. Hopping also is good,—and dancing might be recommended but for the difficulty of persuading any one, especially any young person, to attempt so distressing an exercise. Let them take courage, however, and they may find even dancing bearable in the good cause they have in hand. I could tell wonders of what some young folks have endured in this way. Walking up hill and skating are both good for the thigh muscles. Unfortunately it is impossible to get much skating in this country.

All such work as digging, shovelling, lifting, and so forth strengthens the upper thigh muscles. Some who have much work of this kind to do, get these muscles overdeveloped, and rather stiffened than properly strengthened. But probably few of those for whom we are writing are likely to injure themselves this way. We know dozens of weak-legged folk,—especially weak about the upper thigh —who could do themselves a world of good by digging for half an hour or so every day.

Fencing, with plenty of good lunging, is admirable for the thighs. It is astonishing how few are able to go through even the extension exercises properly. Note that both legs are exercised in passing from the second to the third position in fencing, or sword exercise generally. In the first position the weight of the body rests on the left leg, and the right, though advanced and touching the ground, should bear scarcely any of the weight. Standing in this position, the swordsman ought to be able to carry the right foot backwards by the balance-motion (not that this movement belongs to fencing) without in any way changing the bearing on the left foot. The balance-movement should be executed thus several times, the left leg and foot remaining simply at rest. Next the body should be lowered and raised steadily on the left foot, the balance-movement steadily continuing. Then the right leg and foot should be made to take their turn, the left foot being swayed steadily backwards and forwards in the balance-movement. After both right and left leg and foot have been well exercised in this way, the third position should be assumed, in which the right leg is thrown forward as in lunging, the greater part of the weight of the body resting well on it (the right leg from the knee down forms the short upright leg of a right-angled triangle, of which the longest side is formed by the left leg and the thigh part of the right leg). In this position the gradual alternate lowering and raising of the body, give splendid exercise to the right thigh and knee. The right leg from the knee downward, sways through a slight arc round the foot as a centre; the left foot remains still—unless it slips, as it is apt to do on a carpet or smooth floor. Repeat this exercise with the left knee forwards. The arms can be exercised conveniently in divers ways at the same time, or may be crossed behind the waist, the hands holding opposite elbows.

If instead of such exercises, which are a trifle dull, you prefer actual fencing and singlestick, which may be made exceedingly lively, you should be careful to use the left hand as well as the right. It is just as good fun and the exercise tends much more to symmetry when thus balanced.

Riding is also good exercise for the thighs. Blaikie recommends walking with short steps as more likely to render the thighs stout and strong than walking with good stride, which makes the step easier: but I cannot recommend the adoption of a short stumpy walk. Possibly as an American Mr. Blaikie thinks it desirable to be much on the stump. But what is worth being done at all is worth being done well, and stumpy walking is not good walking.

THE THIGH NEAR THE KNEE.

The sedentary man seldom has the muscles of the lower thigh, that is, of the part of the thigh near the knee, properly developed. Walking lazily does not call these muscles into action. But if any one who has been in the habit of walking with limber legs, and knees rather bent, will try the experiment of straightening the knees as he walks, he will soon find the muscles of the lower thigh telling him emphatically how weak and how easily tired they are.

One of the best exercises for these muscles is that of touching or trying to touch the ground with the fingers without bending the knees. This exercise should be pursued steadily for a short time every day. I have already referred to its value in limbering and strengthening the waist muscles both front and back. It is excellent for most of the back muscles of the legs, and it is convenient to be able on occasion to reach to the ground without bending the legs. A little steady practice every day will astonishingly increase this bending power.

Walking uphill is splendid work for these lower thigh muscles, and good in many ways besides. Going up stairs is good work too, and though it is not easy to go in steadily for this sort of exercise—because there are always people who want to use the stairs—one can often get a good deal of work this way in the regular day's work. Not every one knows how to go up stairs properly, by the way. A man's way of walking up stairs affords a good measure of his intelligence. The average servant, if you notice, goes up stairs as if kicking through the top of the step were the object to be specially aimed at. Children and boys up to fourteen or fifteen—girls, I am afraid, quite as long—adopt the same unreasoning stair-top-kicking gait. I would have the art of getting up stairs taught at school before drilling, and the average absurdity known as calisthenics. What can be more pleasing than the springy gait of an intelligent person on his or her way up stairs. The foot is not driven at or along the top of each stair, but rested on it lightly; the weight of the body then brings down the spring of the foot nicely until, when the other foot has taken its stair in the same easy plantigrade way, the spring recovers itself and lightly lifts the body on. I would as lieve for my own part see a man hauled up stairs like a sack as see (aye, and hear and feel) him shuffle and kick himself up in the average way of "getting up stairs." (*Such* a getting up stairs, indeed!) The reward of adopting this light, springy, clean-treading way of going up stairs is that you go up with less labour, yet with more useful exercise, especially for the muscles of the foot, ankle, knee, and lower thigh.

But my own constant practice for the last twenty years, and the practice I mean to follow till gravity begins to get the better of me, is to go up stairs (as well as down) two steps at a time, aiming at such a movement, by a slight relative lowering of the body at each rising of the feet, that the actual motion of the centre of gravity is as near as possible on an inclined straight line. Going up stairs this way is capital exercise, and is satisfactory to the intelligence as well as pleasing to the understanding.

(*To be continued.*)

THE UNIVERSE OF SUNS.

BY RICHARD A. PROCTOR.

(*Continued from p. 286.*)

THE principle adopted by Sir W. Herschel in both the two last papers of the series which he contributed to the Royal Society is that the distances of stars and star-clusters may be inferred, taking one with another, from the amount of light we get from them. He recognised of course that taking two stars or two clusters of unequal apparent lustre the brighter might really be the farther; but he assumed that a great number of stars or clusters of about the same apparent lustre must have an average distance from us less than the average distance of another considerable number of stars or clusters, resembling each other in apparent lustre but less lustrous than the members of the former set or family. In the paper of 1817 he applied this principle to the stars, in the paper of 1818 he applied it to the nebulæ. Applied either to stars of to nebulæ, the principle is unsafe on *à priori* grounds; and under every test which has been applied to it, it has been found to fail.

Take first the argument from analogy.

If man's life were as the life of the day-fly, but his vision —natural or aided by instruments—were keen enough to show him all the members of the solar system, including the asteroids, moons, meteor streams, and so forth—he would suppose Venus, Jupiter, Mars, and Saturn to be the nearest, Mercury, Uranus, Neptune, a few of the chief asteroids, the moons of Jupiter, and the two largest moons of Saturn much farther away, most of the minor planets and the smaller Saturnian moons much farther away still, and the two moons of Mars at the very outskirts of the system—which he would regard as on the whole shaped like a flat disc. Meteor streams he would regard as probably much remoter parts of the system, or perhaps as outlying systems of the same nature, while comets would seem doubtful objects possibly made up of separate bodies but possibly consisting of some kind of luminous gas. He would form the same general views but there would be slight differences of detail, whether he estimated the distances of bodies by their lustre or by their apparent diameters.

These views would, we know, be entirely erroneous, and we can see that even if observations were not continued long enough to show the laws of planetary motion, the recognition of the arrangement of the various bodies, and in particular the gathering of moons in the neighbourhood of their primaries, and of cometic and meteoric systems in special directions, would show careful reasoners that the principle on which those views had been based was erroneous and misleading.

This analogy shows the unlikelihood that the principle adopted by Sir W. Herschel was true. The principle was practically proved to be untrue by his own observations, which had compelled him to recognise that the stars of the Milky Way are very differently strewn from those which lie in the immediate neighbourhood of the sun.* But the complete disproof of the principle belongs to later times, and to the observational work of Sir John Herschel (who, however, did not very strongly press the conclusions to which his work pointed), the computations of William Struve, the reasoning of Mr. Herbert Spencer, and to the combined processes of observation, computation, and reasoning which I have myself applied to the subject. This will be shown later on.

Assuming the principle, we need not, as Sir W. Herschel correctly pointed out, either suppose all the stars equidistant from each other, or that those of the same apparent

* The mere conclusions that they were differently arranged would not be inconsistent with the principle adopted by Sir W. Herschel, as the reasoning of the next paragraph shows; but Sir W. Herschel's careful explanation of the way in which their arrangement thus differs, and of their tendency to cluster within rounded regions, involves in fact the utter rejection of the principle, as certainly, though not so obviously, as Sir J. Herschel's later argument from the Magellanic clouds.

magnitude are equidistant from us. The principle justifies us however in assigning to each star a certain equal portion of space, and thence calculating how many stars a given region of space may contain. Obviously, reverting to the illustration derived above from the solar system, the very principle which had made the apparent smallness of the asteroids evidence of greater distance would lead to the conclusion that they are not arranged like the major planets, simply because within the limits of distance thus assigned to them there would not be room for so many asteroids as are visible, without much closer setting than the same principle would assign to the few larger planets.

Sir W. Herschel applied his principle to the stars of various magnitudes included in Bode's Catalogue of 14,414 stars. The result of the comparison was to show that if the order of magnitudes could indicate the distance of the stars, we should infer "at first a gradual and afterwards a very abrupt condensation of stars." But, says Sir W. Herschel, if we consider the principle on which these stars are classed, "their arrangement into magnitudes can only apply to certain relative distances, and show that, taking the stars of each class, one with another, those of the succeeding magnitudes are farther from us than the stars of the preceding order."

Which really amounts to this,—that if the results of applying the principle are interpreted on the assumption that the principle is sound, they lead to conclusions not inconsistent with the principle.

Sir William Herschel felt that this result was of no value. He therefore showed that to get at any useful result we must determine the true relative lustre of stars of different apparent brightness. "It will be admitted," he said, "that those stars whose light we can prove experimentally to be $\frac{1}{4}$, $\frac{1}{9}$, $\frac{1}{16}$ . . . . of the light of any certain star of the first magnitude must be 2, 3, 4 times as far from us as the standard star, provided the condition of the stars should come up to the supposed mean state of diameter and lustre of the standard star." He then showed how such comparisons might be made for the whole range from natural vision to the full extent of telescopic vision. In this part of his work we have again the William Herschel of 1789. By his reasoning here he established the general principle of space on which the estimate of the penetrating power of the telescope *for objects assumed to be of the same real size and brilliancy* depends.

Having now obtained a means, as he supposed, of estimating the relative distances of stars of different apparent lustre, Sir W. Herschel could examine the results of star-gauges made in different directions. He found that, thus dealt with, the stars of the tenth, eleventh, and twelfth orders of distance are not only more compressed than those in the neighbourhood of the sun, but moreover their compression in different parts of the heavens must be very unequal.

Let us grasp the meaning of this fully before we proceed. Sir W. Herschel here recognises that the number of the stars of the small magnitudes is such, that granting to them the average distance deduced from their apparent lustre they must be set more closely together than the stars of the large magnitudes. He also recognises that in certain directions they must be more closely aggregated than in others. It is as though from the observed numerical distribution of planets of small apparent size our imaginary shortlived observer should infer (wherein he would be right) that these bodies are more closely grouped in actual space than the larger planets. He might even infer that they formed a ring as they actually do, only he would be entirely mistaken as to the position of that ring. (He would set it far beyond the paths of Jupiter and Saturn.) Moreover he would infer, also rightly, that at the time of observation some parts of the regions occupied by these smaller looking planets were more richly strewn than others,—in other words that the average space allotted to each of them was less than elsewhere.

The conclusions to which Sir W. Herschel had thus been led were true, but they were only a part of the truth, and thus made in company with the unsound principle he had adopted, they became part of an entirely erroneous theory of the stellar universe, precisely as the conclusions of our imagined "astronomer of a day," respecting the distribution of the asteroids in a ring-shaped region of their own would be true in themselves, but only part of the truth, while combined with the theory that all the planets are of about the same size, these true conclusions would become part of the entirely erroneous theory that the asteroids form a gigantic ring of planets far outside the larger looking planets, Jupiter and Saturn.

And now Sir William Herschel made a discovery which should have shown him the incorrectness of his general principle, and would certainly have done so had he made it ten years earlier, just as a kindred discovery did earlier lead him to correct a kindred error of principle, and as the recognition of a kindred truth by Sir John Herschel later led him also to correct the very error of principle which now led Sir William astray.

When Sir W. Herschel had earlier found that in certain regions of the heavens the stars are more richly gathered in mere number than elsewhere, he did not fail to see that they must form a rounded cluster. He felt that the increase in the number of stars on a region round in *apparent* shape could not reasonably be explained as due to the enormous extension of the system of stars in that particular direction, the chances being so enormous against the extension lying in a direction tending exactly from the solar system. If it were conceivable in a single case that the stars seen in a round-shaped region of the sky really occupied an enormously long, cylindrical region of space, the axis of the cylinder tending directly towards the sun, it could not be supposed that in a second case, and in fact in several cases, cylindrical star clusters chanced to occupy the same singular and unlikely position. Besides which, he would probably have rejected the very idea of cylindrical star clusters as absurd on the face of it.

Yet now, when using the space-penetrating method instead of the star-gauging method, he comes on evidence pointing exactly the same way, he no longer with his former clearsightedness sees at once what the evidence means, but he interprets the evidence by the very principle which as a matter of fact that evidence overthrew.

(To be continued.)

THE PATENT ACT OF 1883.

BY POLYGLOT.

ON the 1st of January last came into force a new law relating to patents, trade-marks, and designs. Inventing is eminently the failing of the thinking part of humanity, and, as a desire to "patent" is its direct offspring, a concise practical explanation of the principal provisions of the present law cannot be out of place in this paper, which is expressly intended for people with brains. Therefore, *Salve, lector.*

The new Patent Act, like all innovations of importance, has been hailed with much enthusiasm by some, with much obloquy from others; yet it does not wholly deserve either. That it is a great boon in many respects is evidenced by

the great increase in the number of applications for patents. There have been four times as many in the first quarter of the current year as in the corresponding part of the last, and it is worth mentioning that this increase seems permanent, and will very probably gain volume when the practice under the new conditions is fully settled, and when obscure parts of the Act are cleared by judicial decisions.

For the purpose of this present paper it may be as well to consider the provisions of this law in the order in which they will come home to the inventor and patentee. The first question, therefore, is, What constitutes a patentable invention?

An invention, referring of course not to the process of excogitation, but to its result, may be one of several things, viz.:—

A new device, appliance, or apparatus, or a set of all or any of these serving to produce a known result.

A new method or process, or a novel manner of proceeding by known means a known result.

A novel article of manufacture obtained either by known or by novel means and methods.

Or, lastly, a combination of several of the foregoing.

In considering whether an invention possesses such novelty as will enable it to serve as the subject-matter for a patent, we must not assume that it is enough if the inventor has been previously unacquainted with that which he alleges to have invented, but that part of the public which are most likely to know things of the same character must not have, at the time of the application for a patent, been to such an extent aware of the invention, as to be able to practise it. Another condition is that the inventor shall not have worked his invention for profit (not made or sold articles made according to the same) nor publicly exhibited it, or described it in any publication before the date of his application for letters patent, nor must this have been done by any one else. This does not, however, preclude him from making the invention known to trustworthy persons binding them to secrecy. Publication outside the British Isles does not in any way affect the question.

Novelty alone is, however, not sufficient to entitle an inventor to a patent; another element is required, and this is utility of the invention. This means that the public derive or may derive some benefit by the use of the patented idea. The benefit may be cheapness, better quality, or greater convenience in use, or, in fact, any similar advantage. I think that, if the thing "sells," this would be held *ipso facto* proof of utility, even were the article a mere toy, like the—happily defunct—cri-cri, which the public took such a violent fancy for some ten years ago. It might be said that, by using the invention during the term of the patent solely for his own gain, the inventor really precludes the public from deriving profit from the invention; but, as the latter becomes the property of the world at large on the expiry of the term, this would be no objection, the more so as the exclusive enjoyment for a limited time is really the consideration for the disclosure of the invention.

The next consideration is, who is entitled to the patent? Clearly nobody but the inventor; or, if several persons jointly make an invention, all those (and they only) who are thus concerned in the invention. This is the theory, but, as it is often desirable for the inventor to associate himself with others in patenting and subsequently utilising the invention, the new law provides that the patent may be granted to him and his one or more associates jointly. It must, however, in such a case, clearly appear on the face of the application who has made the invention. If the inventor does not reside within the British Isles, the patent may be obtained by any person to whom he communicates his invention. This, in many cases, is a great convenience. I think, however, that the law ought clearly to state that a patent thus obtained is only to be held by the communicant in trust for the communicator, unless there is an understanding to the contrary. As the law actually stands, the patentee becomes the absolute owner of all the rights in the invention. It has been urged that communication patents ought to be abolished, because an unscrupulous person learning some new invention may go out of the country for a short time, cause the patent to be obtained by an agent (who may act in perfect good faith) in this country, and thus cheat the rightful inventor by avoiding the declaration. I think that the force of this reasoning is in great part, if not wholly, imaginary. If a man is unscrupulous enough to appropriate what is not his (whether it is a silver spoon, a slice off the village common, or an invention), he will certainly not hesitate about a bit of perjury, particularly as it is extremely difficult to prove that a man has not made a certain invention.

When the inventor has fully made up his mind on the points which I have already put before the reader and resolved to obtain a patent, he will naturally be desirous of knowing how to proceed. I may here state my conviction that the best course in ninety-nine cases out of a hundred is to put the matter into the hands of a patent agent of good repute. In this, as in all other matters, a skilled and practised specialist is sure to know more about the procedure, the manner of drawing up the requisite documents, and, above all, will know better how much to say and how much to leave unsaid, than any person, howsoever gifted, who for the first time (ay, and for the tenth) meddles with the thing in a tentative and uncertain way. Unfortunately, there are as many black sheep amongst patent agents as in other classes of men. It is not seemly for me to recommend any one firm; but I think I may be allowed to state that, if a patent agent is a member of the "Institute of Patent Agents," he is sure to be known to his brethren in the profession as an honest and upright man. I am in fairness bound to say that there are honest patent agents outside the "Institute" also, and that the fellowship of the "Institute" is not necessarily a guarantee of that degree of skill which I should seek for in applying to an agent; but at the same time it is not over-easy for an incapable man, and probably almost impossible for a rogue, to be admitted to the "Institute."

Nevertheless, the inventor may wish to do the work himself; if so, he will in the following lines find information which, I venture to hope, will be useful to him.

I will not occupy valuable space by quoting forms, but assume that these are already obtained either from the patent office authorities, through the post-offices who are supposed to sell them (I never tried the post-offices myself), or from a law stationer, a few amongst whom keep a stock of all the requisites for our purpose.

The declaration is easily filled up, the marginal directions are so clear that with a little common sense it is almost impossible to make a mistake. The only point to be noted is the title. Do not make it so short as make it meaningless, nor burden it with irrelevant verbiage. I will illustrate my meaning by a concrete example. If you have invented a process of making dry-plates, and if there be no novelty excepting that, do not call your invention "Photography." That is a title broad enough collectively to cover every method, and all the appurtenances used in the art. On the other hand, if you have invented new methods, not only of making dry-plates, but also of developing, printing, and, perhaps, even of mounting the prints, do not call your invention "Smith's dry plate." Your title ought to be

such as fairly to lead to the inference of all that is to be comprised in the patent, and "Smith's dry-plate" assuredly does not suggest that there are an albuminised paper and a special toning solution and other things besides Your title is, moreover, not an advertisement, and, therefore, no benefit is to be gained by encumbering it with any "fancy" name. Avoid barbarities, such for instance as the following :—

"A perfect improved musical instrument, to be called De Johnson-Brown's Tonic-sol-fa Musarion Jews-harp and Mouth-organ combined."

I know that this sounds incredibly stupid, but I can without difficulty find many parallels to it in the patent office records. Remember, if vanity makes you anxious to see your name in print, that not only will the title of the invention, together with your full name, and address, and the number and date of your application, appear in the official journal, but that most likely not one man in a thousand ever sees this publication unless special business causes him to do so. If you want the fancy name protected, you must register it as a trade mark, which is quite a different matter.

(To be continued.)

COINCIDENCES AND SUPERSTITIONS.

By R. A. PROCTOR.

(Continued from page 281.)

THERE arises, in certain cases, the question whether coincidences may not appear so surprising as to justify the assumption that they are due to a real though undiscerned association between the coincident events. This, of course, is the very basis of the scientific method; and it is well to notice how far this method may sometimes be unsafe. If remarkable coincidences can occur when there is no real connection—as we have seen to be the case—caution must be required in recognising coincidence as demonstrative of association.

Not to take any more scientific instances, of which perhaps I have already said enough, let us consider the case of presentiments of death or misfortune. Here, in the first place, the coincidences which have been recorded are not so remarkable as might at first sight appear, simply because such presentiments are very common indeed. A certain not unusual condition of health, the pressure of not uncommon difficulties or dangers, depression arising from atmospheric and other causes, many circumstances, in fact, may suggest (and do notoriously suggest) such presentiments. That some presentiments out of very many thus arising should be fulfilled is not to be regarded as surprising—on the contrary, the reverse would be very remarkable. But again a presentiment may be founded on facts, known to the person concerned, which may fully justify the presentiment. "Sometimes," says De Morgan on this point, "there is no mystery to those who have the clue." He cites instances. "In the *Gentleman's Magazine* (vol. 80, part 2, p. 33) we read, the subject being presentiment of death as follows :—'In 1718, to come nearer the recollection of survivors, at the taking of Pondicherry, Captain John Fletcher, Captain De Morgan'" (De Morgan's grandfather), "'and Lieutenant Bosanquet each distinctly foretold his own death on the morning of his fate.' I have no doubt of all three; and I knew it of my grandfather long before I read the above passage. He saw that the battery he commanded was unduly exposed—I think by the sap running through the fort when produced.* He represented this to the engineer officers, and to the commander-in-chief; the engineers denied the truth of the statement, the commander believed them, my grandfather quietly observed that he must make his will, and the French fulfilled the prediction. His will bore date the day of his death; and I always thought it more remarkable than the fulfilment of his prophecy that a soldier should not consider any danger short of one like the above sufficient reason to make his will." "I suppose," proceeds De Morgan, "the other officers were similarly posted. I am told that military men very often defer making their wills until just before an action; but to face the ordinary risks intestate, and to wait until speedy death must be the all but certain consequence of a stupid mistake, is carrying the principle very far."

As to the fulfilment of dreams and omens, it is to be noticed that many of the stories bearing on this subject fail in showing that the dream was fully described *before* the event occurred which appeared to fulfil the dream. It is not unlikely that if this had been done, the fulfilment, in many cases, would not have appeared quite so remarkable as in the actual narrative. Without imputing untruth to the dreamer, we may nevertheless—merely by considering what is known as to ordinary testimony—believe that the occurrences of the dream have been somewhat modified after the event. I do not doubt that if every person who had a dream leaving a strong impression on the mind, were at once to record all the circumstances of the dream, very striking instances of fulfilment would occur before long; but at present, certainly, nine-tenths of the remarkable stories about dreams fail in the point I have referred to.

The great objection, however, to the theory that certain dreams have been intended to foreshadow real events, is the circumstance that the instances of fulfilment are related, while the instances of non-fulfilment are forgotten. It is known that instances of the latter sort are very numerous, but what proportion they bear to instances of the former sort, is unknown; and while this is the case, it is impossible to form any sound opinion on the subject, so far as actual evidence is concerned. It must be remembered that in this case we are not dealing with a theory which will be disposed of if one undoubted negative instance be adduced. It is very difficult to draw the line between dreams of an impressive nature—such dreams as we might conceive to be sent by way of warning—and dreams not specially calculated to attract the dreamer's attention. A dream which appeared impressive when it occurred, but was not fulfilled by the event, would be readily regarded, even by the dreamer himself, as not intended to convey any warning as to the future. The only way to form a just opinion would be to record each dream of an impressive nature, immediately after its occurrence, and to compare the number of cases in which such dreams are fulfilled with the number in which there is no fulfilment. Let us suppose that a certain class of dreams were selected for this purpose. Thus, let a society be formed, every member of which undertakes that whenever on the night preceding a journey he dreams of misfortune on the route, he will record his dream, with his ideas as to its impressiveness, before starting on his journey. A great number of such cases would soon be collected, and we may be sure that there would be several striking fulfilments, and probably two or three highly remarkable cases of this sort; but for my own part, I strongly entertain the

* De Morgan writes somewhat inexactly here for a mathematician. The sap did not run through the fort, but the direction of the sap so ran.

opinion that the percentage of fulfilments would correspond very closely with the percentage due to the common risks of travelling, with or without premonitory dreams. This could readily be tested, if the members of the society agreed to note every occasion on which they travelled: it would be found, I suspect, that the dreamers gained little by their warnings. Suppose, for instance, ten thousand journeys of all sorts were undertaken by the members of the society in the course of ten years, and that a hundred of these journeys (one per cent., that is) were unfortunate; then, if one-tenth of the journeys (a thousand in all) were preceded by warning dreams, I conceive that about ten of these warnings (or one per cent.) would be fulfilled. If more were fulfilled there would appear, as far as the evidence went, to be a balance of meaning in the warnings; if fewer, it would appear that warning dreams were to some slight degree to be interpreted by the rule of contraries; but if about the proper average number of ill-omened voyages turn out unfortunately, it would follow that warning dreams had no significance or value whatever: and this is the result I should expect.

Similar reasoning, and perhaps a similar method, might be applied to cases where the death of a person has been seemingly communicated to a friend or relative at a distance, whether in a dream or vision, or in some other way, at the very instant of its occurrence. It is not, however, by any means so clear that in such instances we may not have to deal with phenomena admitting of physical interpretation. This is suggested, in fact, by the application of considerations resembling those which lead to the rejection of the belief in dreams giving warning against dangers. Dreams of death may indeed be sufficiently common, and but little stress could be laid, therefore, on the fulfilment of several or even of many such dreams. But visions of the absent are not common phenomena. That state of the health which occasions the appearance of visions is unusual; and if some of the stories of death-warnings are to be believed, visions of the absent have appeared to persons in good health. But setting aside the question of health, visions are unusual phenomena. Hence, if any considerable proportion of those narratives be true, which relate how a person has at the moment of his death appeared in a vision to some friend at a distance, we must recognise the possibility, at least, that under certain conditions mind may act on mind independently of distance. The *à priori* objections to this belief are, indeed, very serious, but *à priori* reasoning does not amount to demonstration. We do not *know* that even when under ordinary circumstances we think of an absent friend, his mind may not respond in some degree to our thoughts, or else that our thoughts may not be a response to thoughts in his mind. It is certain that such a law of thought might exist and remain undetected—it would indeed be scarcely detectible. At any rate, we know too little respecting the mind to be certain that no such law exists. If it existed, then it is quite conceivable that the action of the mind in the hour of death might raise a vision in the mind of another.*

I shall venture to quote here an old but well-authenticated story, as given by Mr. Owen in his "Debatable Land between this World and the Next," leaving to my readers the inquiry whether probabilities are more in favour of the theory that (1) the story is untrue, or (2) the event related was only a remarkable coincidence between a certain event and a certain cerebral phenomenon, in reality no way associated with it, or (3) that there was a real association physically explicable, or (4) that the event was supernatural. Lord Erskine related to Lady Morgan—herself a perfect sceptic—(I wish, all the same, that the story came direct from Erskine) the following personal narrative:—"On arriving at Edinburgh one morning, after a considerable absence from Scotland, he met in the street his father's old butler, looking very pale and wan. He asked him what brought him to Edinburgh. The butler replied, 'To meet your honour, and solicit your interference with my lord to recover a sum due to me, which the steward at the last settlement did not pay.' Lord Erskine then told the butler to step with him into a bookseller's shop close by, but on turning round again he was not to be seen. Puzzled at this he found out the man's wife, who lived in Edinburgh, when he learnt for the first time that the butler was dead, and that he had told his wife, on his death-bed, that the steward had wronged him of some money, and that when Master Tom returned he would see her righted. This Lord Erskine promised to do, and shortly afterwards kept his promise." Lady Morgan then says, "Either Lord Erskine did or did not believe this strange story: if he did, what a strange aberration of intellect! if he did not, what a stranger aberration from truth! My opinion is that he *did* believe it." Mr. Owen deals with the hypothesis that aberration of intellect was in question, and gives several excellent reasons for rejecting that hypothesis; and he arrives at the conclusion that the butler's phantom had really appeared after his death. "The natural inference from the facts, if they are admitted, is," he says, "that under certain circumstances, which as yet we may be unable to define, those over whom the death-change has passed, still interested in the concerns of earth, may for a time at least retain the power of occasional interference in these concerns; for example, in an effort to right injustice done." He thus adopts what, for want of a better word, may be called the supernatural interpretation. But it does not appear from the narrative (assuming it to be true) that the butler was dead at the moment when Erskine saw the vision and heard the words. If this moment preceded the moment of the butler's death, the story falls into the category of those which seem explicable by a physical theory,—as by the theory of brain-waves. I express no opinion.

(To be continued.)

* I remind the reader that this article is from my "Borderland of Science," (now out of print,—whatever advertising columns may say—and not to be reprinted); and that the opinions above quoted were expressed by me in the *Cornhill Magazine* many years ago.

WIREWORMS AND SKIPJACKS.

By E. A. BUTLER.

IN turning up the soil round garden plants, we sometimes find a stiffish, elongate, shiny, yellowish-brown, worm-like thing, about the thickness of a stout pin, and about three-quarters of an inch in length. Under the impression that any living creature found in garden-soil is an intruder that should be summarily disposed of, we may proceed to endeavour to put these ideas into practice, only, however to find that this is not quite so easy a matter as it

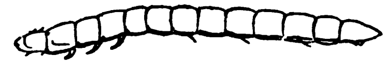


Fig. 1.—Wireworm, magnified.

seemed; the thing is so stiff and tough, that even a good hard squeeze seems to make but little impression on it. This tough, worm-like thing is a wireworm (Fig. 1), and so

dire a foe is it to vegetation, that we are perfectly justified in making all efforts to despatch it. On examining it more closely, we find that it is not truly cylindrical, like a piece of wire, but somewhat flattened beneath, and that it is made up of a series of thirteen segments, placed in line, one behind the other. The first of these is the head, and the next three carry six short legs, one on each side of each segment, with which the creature crawls along, trailing the remainder of its body after it. The head is black, and is furnished with a pair of stout, transversely-moving jaws, and a pair of short antennæ.

Wireworms are the larvæ of various kinds of beetles, called "skipjacks" or "click-beetles," from a peculiar habit of springing up into the air, and, at the same time, producing a sharp clicking sound. Skipjacks are narrow, elongate insects, with short legs and hard integuments

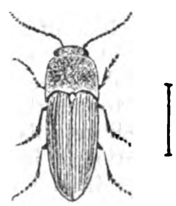


Fig. 2.—Click Beetle. *Agriotes obscurus.*

(Fig. 2). The head is small and often much sunk into the thorax, and carries a pair of long, distinctly jointed antennæ; the thorax is of large size, and, roughly speaking, more or less quadrangular in outline, and convex above and beneath. The elytra or wing-cases cover the body, and conceal a pair of ample membranous wings. Each is somewhat triangular in shape, and they form when closed a strongly arched, shield-shaped surface; they are usually marked longitudinally with parallel grooves or furrows, and covered more or less densely with short hairs. The under-surface also is strongly convex, and the legs are short, and capable, like the antennæ, of being folded close up to the body. When thus compactly folded up, the insect may easily be mistaken for a piece of stick or earth. When surprised or alarmed, it will thus feign death, relaxing its hold of what it may have been clinging to, and falling to the ground, as often as not, on its back.

Now usually, when a beetle gets into such a position, it frantically waves its legs about till one of them by chance strikes the ground; then, seizing any irregularities of surface with the sharp claws at the end of its feet, and assisting itself with the end of its shanks, it levers itself over sideways. But, owing to the convexity of its back and the shortness of its legs, a skipjack is unable to use this method, unless there happen to be close to it some objects of sufficient height to be reached by its waving legs; failing this, however, it would be, were it not for a remarkable contrivance, as helpless as a turtle in a similar position, and would stand a good chance of being doomed to continue its unavailing struggles, at the mercy of any passing foe, till exhaustion ended its woes by death. The contrivance is as follows:—The hinder edge of the thorax is produced in the middle underneath into a long curved blunt spine, which is received into a little pit at the base of the body. The thorax is loosely articulated to the abdomen, and can be freely moved up and down like the lid of a box on its hinge. When on its back, therefore, the skipjack arches its body by bending its thorax backwards, and so balances itself on the two extremities of its body; this movement releases from its hollow the spine above referred to. Having stretched itself to the utmost in this attitude, the insect suddenly and forcibly resumes its former supine position—a movement which has the effect of causing it to rebound from the ground and shoot upwards into the air to the height of several inches, at the same time bringing the spine back into its sheath with a sharp clicking sound. On returning to the ground, the insect generally manages to land itself right side up; if not successful the first time, however, it renews the attempt, and continues skipping till the desired result is obtained.

About 60 species of skipjacks belong to the British Fauna, and three or four of them, brownish insects belonging to the genera *Athous* and *Agriotes* are exceedingly common; the latter genus furnishes the most destructive wireworms. In their larval existence they are subterranean in habits, living for several years a little below the surface and spending their time in devouring the roots and underground stems of plants, and thus, of course, doing much more harm than can be measured by the amount of matter actually devoured. In the winter they retire to a greater depth, descending farther and farther as the frost increases, and pausing in their depredations only in the coldest weather. They devour all kinds of agricultural produce, destroying both root, grain, and fodder crops. Carrying on the ravages as they do in the complete obscurity of subterranean life, they are rarely detected when at work, and the first evidence that the fatal work has been done is seen in the apparently causeless withering of the plants.

It is fortunate that creatures so destructive have natural enemies. Among the most important of these is the mole, which devours the larvæ with avidity. It is aided in its praiseworthy efforts by several kinds of birds, such as rooks and lapwings. A variety of artificial remedies have been proposed for checking the spread of the mischief, such as the application of liquid manure, which has the twofold effect of strengthening the plants that have not been irreparably injured, and driving away or killing the wireworms; paring off a thin coating of the soil, which will contain most of the insects, and then burning it; imbedding in the soil at short distances apart slices of carrot and turnip to serve as traps, and then examining them and destroying the wireworms every other day. The latter method has been found serviceable in hop-grounds, as many as 150 wireworms having been trapped close to a single hop-hill. It should be remembered in this connection that the abundance of many agricultural pests is due in great measure to man himself. We greatly increase the supply of suitable food for these creatures, and in other ways make the surroundings more and more favourable to their existence, and we need not wonder, therefore, that the inevitable result follows, and that the additional task devolves upon us of devising means to counteract the excessive development we have ourselves unintentionally occasioned.

The group to which these insects belong possesses a few British representatives of considerable brilliance in colouring, but they are far surpassed, both in beauty and in size, by exotic forms, some of which are amongst the most brilliant of all beetles. To this group, also, belong the well-known and remarkable Fire-flies of the West Indies, not to be confounded with the Lantern-flies, which are members of a widely-different order of insects, the Homoptera. The light emitted by fire-flies proceeds from two patches on the thorax and from others concealed beneath the elytra when they are closed, but rendered visible when they are spread for flight. An old writer, Pietro Martire, gives the following quaint account of a method of catching these creatures: "Whoso wanteth cucuij, goeth out of the house in the first twilight of the night, carrying a burning firebrande in his hande, and ascendeth the next hillock that the cucuij may

see it, and hee swingeth the firebrande about, calling cucuius aloud, and breaketh the ayre with often calling and crying out 'cucuie, cucuie!' Many simple people suppose that the cucuij, delighted with that noise, come flying and flocking together to the bellowing sound of him that calleth them, for they come with a speedy and headlong course, but I rather think that the cucuij make haste to the brightness of the firebrande, because swarmes of gnattes fly into every light, which the cucuij eat in the very ayre, as the martlets and swallowes doe. Some cucuius sometimes followeth the firebrande, and lighteth on the grounde; then he is easily taken, as travellers may take a beetle, if they have need thereof, walking with his wings shut. In sport and merriment, or to the intent to terrify such as are afrayed of every shadow, they say that many wanton, wild fellowes sometimes rubbed their faces by night with the fleshe of a cucuius, being killed, with purpose to meet their neighbours with a flaming countenance, as with us wanton young men, putting a gaping vizard over their face, endeavour to terrify children or women who are easily frighted."

VESTIGES OF CREATION.*

I AM glad that the authorship of this fine work has been at length publicly acknowledged. It has long been an open secret; yet many, not knowing the facts of the case, exercised their minds over the matter as a puzzle, and formed strange theories about the work. I was myself offered an elaborate MS. proving that Robert Chambers could not have been the author of the "Vestiges," and "going near to prove" that the author was no other than Sir Charles Lyell. But my real reason for rejoicing that the book has now been definitely assigned to its real author is that the world owes very much to him for this admirable work. Boldly conceived, effectively treated, and admirably written, the book has done more good than hundreds of volumes by the working section of scientific inquirers. It has been found easy to point to errors in matters of detail, especially in the earlier editions, but those men of science who most carefully indicated mistakes in points of detail,—from which their own works were doubtless freer, could not see that the work presented justly a great general truth which they had failed entirely to grasp. Robert Chambers, compared with Prof. Sedgwick and the rest who so scathingly denounced him for errors of detail, might be compared to an architect who should be ridiculed for mistakes about details of mason-work. He selected, here and there, materials which were not so good as he had supposed. But the beauty of the edifice he reared was not impaired by such mistakes as those. His scientific critics on the other hand, collected excellent materials for a great work; but they had no more idea how to plan such a work than the mason has respecting the architectural details suitable for a noble temple.

Robert Chambers himself was surprised at the hostility to his views shown by many men of science. He seems to have expected to find "many men of science" to be great, like those who erect a grand edifice of science, and falling back to look at it feel like archangels, as Wendell Holmes has finely said. But nine out of ten of them, nay ninety-nine out of a hundred, are either but as the working masons, or put together—far from the real temple of science, of whose proportions and beauty they know nothing,—the mere fittings and ornaments of the temple. Chambers presently felt how the case really was. He had felt "embarrassed in presenting himself in direct opposition to so many men possessing talent and information." But being led by his study of the great problems of nature in their nobler aspect, to consider the work of those who analyse the details of natural phenomena, he saw the truth. In words deserving of most careful study, he shows why so many men of science disapproved of views which now are (in their general aspect) the accepted truths of science. "The ability of the purely"—he might have said rather the "narrowly"—"scientific class to give at the present time a true response" to the great question he had propounded, "appears extremely challengeable. It is no discredit to them that they are, almost without exception, engaged each in his own little department of science, and able to give little or no attention to other parts of that vast field. From year to year, and from age to age, we see them at work, adding no doubt much to the known, and advancing many important interests, but at the same time doing little for the establishment of comprehensive views of Nature. *Experiments in however narrow a walk, facts of whatever minuteness, make reputations in scientific societies*: all beyond is regarded with suspicion and distrust."

It is gratifying to consider, however, that despite the inane enmity of those who imagined that the grand and elevating views in the "Vestiges" were in some way opposed to religion, and the torpid hostility of the scientific detail-workers, the work made its influence felt at once. If Chambers was pained sometimes at the virulence of the attacks made on his book, he recognised all the time that the very violence of these attacks showed how his work was telling. It was reaction, equal and opposite to action, and being energetic showed that it was aroused by most effective work.

It would be difficult to say how much of the freedom and strength of men's present modes of thought in these matters is due to Robert Chambers. The mere number of readers of his book, though they were to be counted probably by hundreds of thousands, would give no true measure of its influence. Other books, in some respects more valuable (though they have done no such good work), have replaced the "Vestiges." Our Lyells, who at first were deterred by just scientific objections from accepting the particular mode of development indicated by Chambers, have been able to accept what was after all the vital principle of his work, so soon as our Darwins and Wallaces showed (at least in part) what the real system of development is. The principle of Chambers's book,—no new principle in fact, but newly presented as advanced by him—lies at the very foundation of all our recent treatises on biology, geology, physiology, and psychology. Darwin's works may be regarded as owing the just esteem in which they are held to this chiefly—that they show *how* the process of development has gone on of which Chambers had only been able to show that it certainly has been and is the great law of the universe. In Mr. Spencer's works alone of all those in which the doctrine of development has been advanced is a wider field entered and surveyed than that which Robert Chambers entered, and which Darwin, Wallace, Huxley, and their fellow-workers have partly surveyed.

Of the attacks made on the "Vestiges of Creation" by the unbelieving crew who shuddered at the thought that the laws seen around us on the small scale extend to all nature on the fullest scale, it is hardly necessary to speak. These poor in soul we have always with us. They are always in a state of alarm lest "some hysteric sense of

* "Vestiges of the Natural History of Creation." By Robert Chambers, LL.D. Twelfth Edition. With an introduction relating to the authorship of the work, by Alexander Ireland. (W. & R. Chambers, London and Edinburgh.)

wrong or insult should disturb the throne where Wisdom reigns Supreme," and lest *they* should be punished because others reject the strange god which they have made after their own likeness. To them as science reveals more and more of the mysterious glory of the universe, and as the dread of Infinite Law falls more heavily on their souls, we may say—(though it will avail nought)—"O, ye of little faith, wherefore do ye fear?" Shuddering as they stumble along in the rear of the thought of their age, these feeble and fearful ones raise their shrieks of mingled anger and alarm, as science steps confidently onwards. They ever dread lest science should rashly tread on forbidden ground. They need not fear. Science has long since put the shoes from off her feet in full consciousness that wherever her footsteps may tread, the ground on which she stands is holy ground. But she has no fear lest the Holy of Holies should be reached. She has long since learned that

"End there is none to the Universe of God: lo! also there is no beginning."

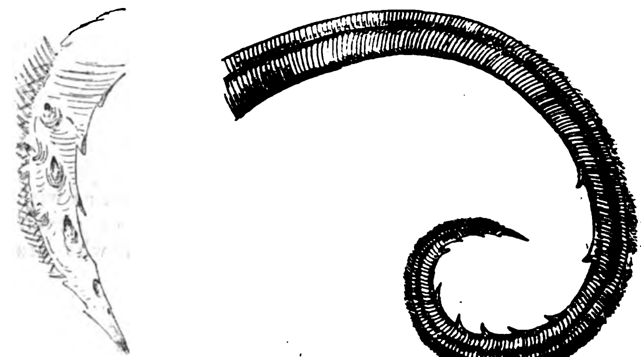
RICHARD A. PROCTOR.

PLEASANT HOURS WITH THE MICROSCOPE.

By HENRY J. SLACK, F.G.S., F.R.M.S.

IN former papers of this series, the mouth-organs of some interesting insects have been described. In the Breeze Flies, we found an alarming set of knives and saws, adapted to pierce the skin of such animals as the horse; in bees, the biting mandibles are not directly concerned in obtaining the creature's food, but other mouth parts are developed to form a long tongue, with which the nectar of the plants is both swept up and sucked in. Wasps, we found with stronger biting mandibles, and a much shorter tongue. In the butterfly and moth groups the mandibles are long and pliable, and when pressed together make a sucking-tube able to reach deep down in the corolla of flowers. It used to be thought that no member of these groups was furnished with the means of piercing through any tough substance; but if there is no breach in the continuity of nature, it would be expected that either in living or extinct species some connecting-links between different kinds of sucking and biting, or rather piercing, insects would be discovered, and the Lepidoptera would not appear an isolated group.

Entomologists, however, did not, a little while ago, reason in this way, and in 1871, when a French botanist, named Thozet, who had established himself in Roehampton, a little town on the Tropic of Capricorn, in Australia, accused a lepidopter, belonging to the genus *Ophideres*, of perforating oranges to feed upon their juice, he met with no attention. M. Thozet sent specimens to Herr Künckel, who made no proper examination of them until 1875, when he saw in the *Capricornian*, a Roehampton paper, an anonymous account of the damage done to the fruit by *O. Fullonica*. The German naturalist then examined the specimens sent him, and found them provided with a proboscis, which he thus described:—"The two adpressed maxillæ terminate in a sharp triangular point, furnished with two barbs. They then swell out, and present on the lower surface three parts of the thread of a screw, while their sides, on the upper surface, are covered with short spines, springing from a depression with sharp, hard sides. These spines are to tear the cells and the pulp of the oranges, as a rasp opens those of beetroot to extract the sugar. The upper portion of the proboscis is covered from below and on the sides with fine serrated striæ, disposed in a half helix, which give it the properties of a file. These striæ are from time to time interrupted by small, non-resisting spines which serve as tactile organs. The orifice of the canal by which the liquids ascend is situated in the lower face, below the first thread of the screw." This account, taken from *Comptes Rendus*, Aug. 30, 1875, was published by the writer in the *Monthly Microscopical Journal* for November in that year. The credit of first describing and figuring this proboscis belongs, however, to Mr. S. J. McIntire, whose paper and drawings (which we copy) will be found



in the *Monthly Microscopical Journal* for May, 1874. Mr. McIntire was engaged in studying the scales of the Lepidopters, and among a lot of damaged specimens, said to have come from West Africa, he found a brown moth with the peculiar apparatus. He at once divined the use of this unusual implement. He saw that it could pierce most vegetable structures, and that, after penetration had been effected, the recurved spines would act as holdfasts and allow the creature to take his food in a leisurely way. He added that "the unwary captor of such a moth might find, to his surprise, that it could, in self-defence, inflict a puncture of his skin by no means insignificant."

A few years ago, Mr. Reginald Reid, of Sydney, was kind enough to send me a mounted specimen of this proboscis, which is a most interesting object. The hairiness of the terminal part of the organ reminds one of the bee's tongue. When a ripe orange is punctured by the boring-tools, the juice must flow freely, and the mop-like part of the organ must be just the thing to take it up. The spines which Mr. McIntire figures as rising out of depressions, suggest, when seen under a magnification of about 100 linear, that they *may* be movable from their base, and thus oppose less obstacle to the withdrawal of the organ when the meal is over. The other spines have cutting edges on both sides, so that they could make their way in or out with about equal facility.

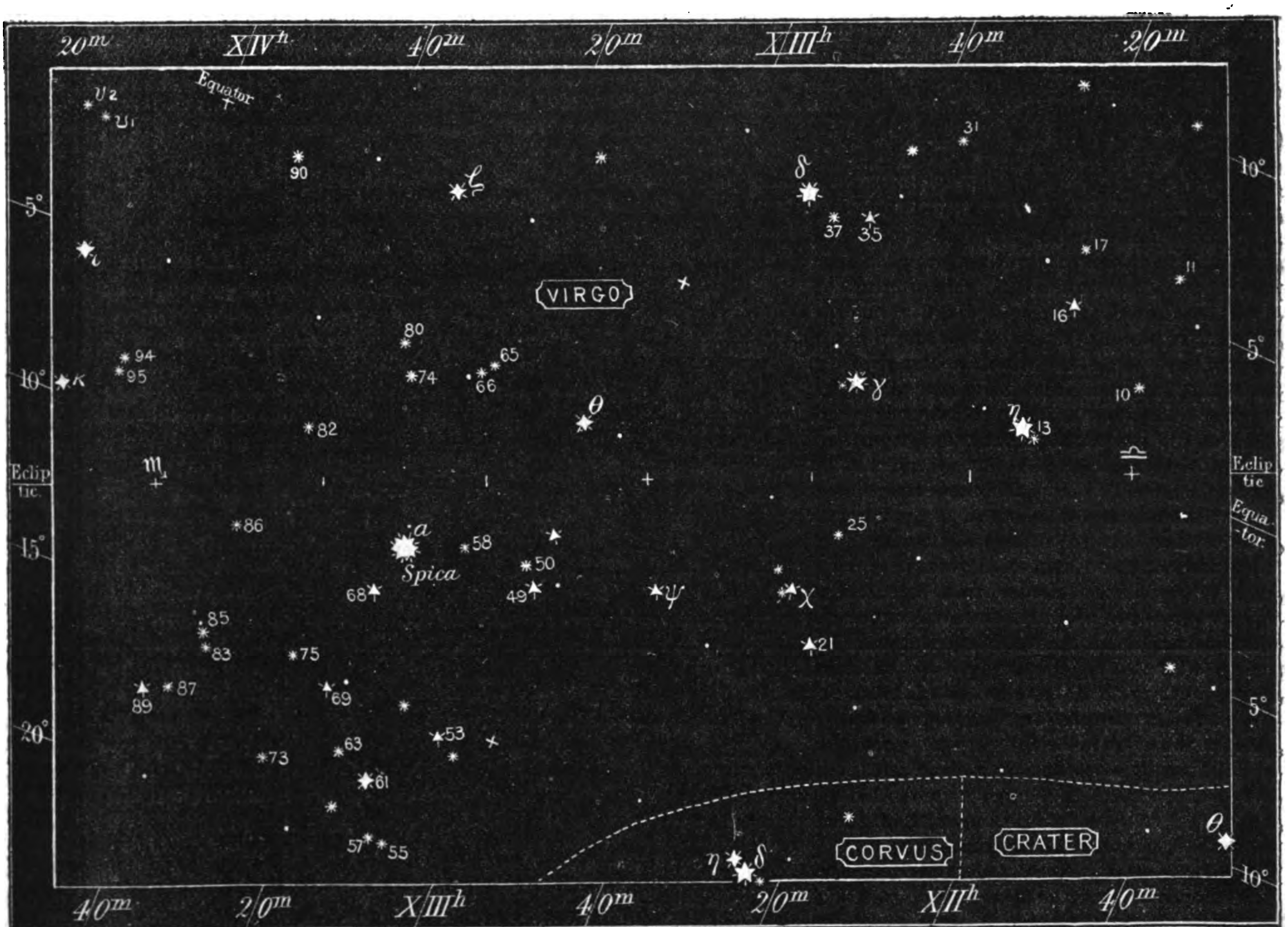
As KNOWLEDGE goes all over the world, I hope observers in various climes will look out for other *Ophideres*, and for any other Lepidopters of a like character. The creature must be tolerably easy to watch and to catch during its attack upon orange, and perhaps other fruit gardens, when once its feeding-times are known. Herr Künckel studied *O. Salaminia*, *O. materna*, and *O. imperator*, as well as *O. Fullonica*, and found all supplied with the same sort of tool.

Mr. McIntire's moth was drab coloured, inclining to reddish-brown, and about 2½ in. across the wings. He found the proboscis stout in proportion to its length. This could not be said of the specimen I received from Mr. Reginald Reid. So far as I am aware, no addition has been made to the information given by Mr. McIntire, and Herr Künckel, and it is important to stimulate further search. A proboscis adapted to piercing as well as sucking is found amongst the *Hemiptera* and the *Diptera*, but the

exact way the two functions are provided for in the *Ophideres* could not be an abrupt variation from any form hitherto known. There must have been, and probably still are, extant, a series of modifications leading through the great orders of insects from an early pattern of mouth-organ, through all the modifications that now exist. Even in butterflies the antlia, or proboscis, is not always alike. In *Vanessa atalanta* (red admiral), for example, the two elongated maxillæ are supplied towards their tips with a great number of little elongated barrel-shaped bodies terminated by smaller papillæ, which are thought to be taste-organs. In the common white butterflies they are scarcely perceptible.

Mr. Newport, writing in the "Cyclopædia of Anatomy and Physiology" concerning the mode in which the fluid food of the Lepidopters is made to ascend up their proboscis, stated that he gave sugared water, coloured with indigo, to two specimens of *Pontia rapi* (cabbage butterfly), and that he could see, under the microscope, the syrup went up in quick streams for one or two seconds, as if pumped up energetically, and then the motion was slower for an interval. He conceived that when the insect alights on a flower, it makes a strong expiration, which sends air out of the tracheal tubes of the proboscis, and out of those in the œsophagus, alimentary canal, and other parts. The inspiration which follows carries the fluid up, assisted by the action of the muscles of the proboscis. This is much more likely than an ascent of the fluid by capillary attraction, and corresponds with what appears when a bee is feeding and watched, as explained in a former paper.

If the reader compares one or two flexible maxillæ of the butterflies and moths with the stiff, knife-like ones of the Breeze Flies (as shown in a former number), he will see that the Lepidopter's organ is far more complicated. It is not only well supplied with the breathing-tubes—tracheæ—with their wonderful spirals to keep them open, but with a most elaborate and delicate system of muscles, which enable the instrument to be coiled up when not in use. Any part of an insect which abounds in tracheæ is sure to be the seat of some very active function, and, in the case of mouth-organs, to be connected with touch, or taste, as well as with a more mechanical use.



OUR NIGHT SIGN.

IN the map showing the Night Sign for April, certain details were left uncancelled which belong to the Day Sign for October. We therefore give the map, in its correct form, to make the series for the year complete. The star γ Virginis was inadvertently omitted.—R. A. PROCTOR.

ON the railways of the United Kingdom during the past year the grand total of accidents of all kinds to persons reached 1,167 persons killed, and 4,187 injured. Of this large number, only twenty-two were killed by accidents to trains, rolling stock, or permanent way. Eleven, or half of these, were railway servants, so that were it not for carelessness, thoughtlessness or suicides, the casualties on railways would be wholly insignificant.

ODD COINCIDENCES.

By Richard A. Proctor.

IF I believed in luck I might regard journeys into Birmingham as unlucky for me. It may be remembered that a few months ago I described how oddly I was removed from my proper carriage and put into one not going to Birmingham. I was all right and alone; *two* passengers got into a wrong carriage; a second class passenger was put into mine (first class) because of a loose coupling; and at the next station, a nervous station-master, wired to about the two wrongly-placed passengers, rushes to the wrong carriage, asks if we are for Birmingham instead of some other place, as he intended, and receiving "Yes" for reply, bundles us—the wrong passengers—into the carriage for that other place,—so that I get to Birmingham much later than I intended, and my companion, who had a special appointment there, finds it useless to go there at all, and returns home.

Well, last week running into Birmingham from Knowle, I narrowly escape "a real misfortune" as Mrs. Brown would say. I put down the bag containing the pictures for my lecture (the train was only to reach Birmingham five minutes before the lecture hour); go across a bridge to send off a telegram, and returning, enter train without my bag. I had not made such a mistake since February 3rd, 1881, when I let a train run out from Sacramento, Cal., with my lecture pictures. *That* time I was saved from shame and disaster, by a rainstorm so violent that my lecture had to be postponed; and by next day kind friends in the eastward bound train had sent back my pictures. *This* time, kind friends (one of them the same as in my Sacramento trouble) wired for my bag, and eventually it was brought in to me at the Birmingham Town Hall, just as I had gotten through the introductory part of my lecture.

This was rather an odd coincidence, but this was not the end of these Birmingham-nearing sorrows.

A few days later I am leaving Coventry for Birmingham, meeting my wife (from London) at the former station. "I will be careful," I remark to myself, "that no mistake of mine causes me any trouble." Vain hope! I see my heavier luggage labelled. On the arrival of the London train a lady and gentleman get out from the carriage in which my wife is sitting. They claim their smaller luggage, and my porter puts my precious lecture-bag on a seat. I turn to put my umbrella in the rack, or perchance to salute my wife. "There was our error, boys," as old Belarius says. "Oh, you mistook; you should have seized your bag" (as Milton does not say), I might have said to myself. But, unconscious of disaster, I begin to inquire about the little ones at home. In the midst of these domestic cares suddenly I miss my bag. You guess what had happened? A wretched porter, sent back by my wife's fellow travellers to see that nothing was left, had opened the door the instant my back was turned, and had seized *my* bag as his lawful prey!

We wired from Stetchford and anon from Birmingham. Reply comes, "All luggage put out at Coventry claimed by Mr. B., of So-and-So Park." More urgent telegrams bring us news that So-and-So Park is seven miles from station. How again the same good friend (and somewhat more) who at Sacramento had sent back my bag, and at Knowle had helped to restore it to me, had to go to Coventry,—while I lectured (*minus* some of my best pictures) at Birmingham —how Mr. B. had seen nothing of the bag, but how it turned up eventually along with his luggage, which had been sent unsorted to a lumber-room at the top of the house, need not here be told. The triple coincidence was complete, and I may hope to have no more trouble of the kind when nearing Birmingham. But I can understand how some folk get to imagine that particular places are unlucky to them.

Editorial Gossip.

THE *Daily News* (May 1) remarks that "there are few innocent pleasures greater than yachting or coaching." Why the distinction? Are we to understand that guilty pleasures are greater than innocent pleasures? It is a singular doctrine, and moreover (a detail) untrue.

COLONEL KING-HARMAN described the Cremation Bill as "a crude attempt of scientific men to foist their opinions on the House" of Talk. Scientific men may be defined as men who are always wanting to know, and persons of Colonel King-Harman's turn of mind as folks who would much rather not know. Any attempt to teach such folk must of necessity be crude and ill-considered. I say this, only in relation to the worthy Colonel's general principle. As regards cremation it seemed, so far as Parliamentary talk over it went, to resolve itself into the question whether it was worse that many living should be poisoned than that a few who may have been poisoned should remain unavenged.

THE real trouble about all questions of the disposal of the dead is that not one in a thousand really *feels*, as well as knows, that the dead body has no more real relation to its former occupant than the clothes he wore or than the arm-chair he used to sit in. If there was the slightest reason for imagining that the clothes of the dead might carry infection to the living no one would hesitate to burn them. If we cared to consider facts as they are we should think in like manner of the "muddy vesture of decay" which once did "grossly close in the defunct."

MR. HERBERT SPENCER has recently directed attention to a point in connection with deaf-mutism which is of considerable importance. The inquiries of Tylor, Lubbock, and others, into the beliefs of savage races have not thrown so much light as could be wished on the degree to which religious ideas are entertained by races in various stages of savagery or on the origin of those ideas which appear to be most widely extended. Yet it seems clear that among the very lowest savages the idea of something other than body—call it soul, spirit, ghost, or what we will—has no existence. The phenomena of external nature, the occurrence of dreams, the remembrance of the dead, slowly arouse in savage races the conception of the immaterial, the sense of mystery, the hope of a hereafter, the idea of beings outside the sphere of cognition, and finally—after long processes of change and purification—faith in a Supreme Being. Without touching on the question of direct revelation, so much is certain and admitted by all, seeing that the facts lie before us. But many have naturally been led to ask what would happen if any members of civilised and advanced races could be allowed to grow to a knowledge of what modern science teaches about physical and physiological matters, but without any instruction in the religion of those around them. Would any intuitive perception of the immaterial appear? Would any religious conceptions, any ideas as to a future state, a Creator of all things, a Supreme Being Unknowable but necessarily Infinite in Power and Wisdom, manifest them-

selves? Mr. Spencer seems to think that we find an answer to this question in the case of intelligent deaf-mutes, who have remained through childhood and youth without any instruction because not as yet brought into communication with those around them by any substitute for spoken language which could properly introduce abstract ideas to their cognition. Whether this is so or not, the matter is clearly one of great interest, and now that so much is being done to give language to the deaf-mute, it might be interesting to ascertain what their ideas had been respecting the great mysteries of nature, or rather respecting the Mystery of Mysteries which we call the Unknowable. So far as is yet known it appears that the deaf-mute not only has no idea of a future state, but he has not even any idea of death,—a circumstance which should prevent any from hastily assuming that the absence of religious ideas in the case of the deaf-mute is evidence against their truth,—for death at any rate is certain, however unsuspected by the untaught deaf-mute. The Rev. Samuel Smith, who has been for 28 years in almost daily contact with deaf-mutes, says that in not a single instance has an uneducated deaf-mute been found to have "any conception of the existence of a Supreme Being as the Creator and Ruler of the Universe."

THE DISCOVERY OF "PRE-HISTORIC" REMAINS IN LINCOLNSHIRE.

IN KNOWLEDGE of May 2 a paragraph is given which first appeared in the *Banner*, "a Church and constitutional newspaper," relating to an alleged extraordinary discovery of neolithic works in the plain of the Ancholme, near Brigg. The report was communicated to the *Banner* by the Rev. C. W. Markham, rector of Saxby. At it was of a very extraordinary nature, I determined to seek further light upon the matter, not without a strong suspicion, founded partly on the source whence the intelligence came, and partly upon the wording of the paragraph, that it would turn out "a mare's nest." The case is not a little instructive as showing how cautiously statements about such matters ought to be received. I soon found that the folk in the neighbourhood of the supposed wonder were somewhat excited by the marvellous things they had been induced to believe concerning it. On the other hand, I found that people as experienced in such questions as members of the Government Geological Survey were indifferent about it. When writing to an intelligent tradesman in the locality, I mentioned that it had been reported to me that the cross-beams in the wooden way were secured by trenails inserted in *square* holes (evidently cut by a mortice chisel), I got the reply "that the Government Geological Surveyors have no information on the matter worth a button-top. They are mere surveyors; they know nothing of antiquities; nor are they acquainted with geology in its lessons—at least those with whom we have come in contact." This looked very funny—all the more so when I found that my correspondent, so far from believing in this as a neolithic structure, "of enormous age, *almost* beyond calculation," describes it as "one of those ancient ways placed over low and boggy places by the Romans, or perhaps Anglo-Saxons." This, to him, was almost as marvellous as if the work had been that of neolithic man! Hence his impatience with the "Geological Surveyors," who seemed to him to think too little of the discovery. The facts, as near as I can obtain them, are that the beams of oak were found associated with boughs of yew under six feet of (probably warp) clay with a peaty surface. There is no proof of the extension of the roadway beyond the excavation in which it was exposed. As to the Roman road, there are no traces of one above the planked way in the vicinity of the latter, but signs of Roman roads are said to have been found in surface peat higher up the Ancholme Valley. Two pins, or trenails, pass through each beam, one near each end, and the holes are *squared*. W. MAWER.

LOST-CAR agents, the *Railroad Gazette* says, find cars in all sorts of curious places; but Mr. Rogers, of the Pennsylvania Road, recently had an experience which was out of the way even for him. In search recently of two missing coal cars out in the flooded district in Ohio, he found one of them in a cornfield—which was not so uncommon—but soon after he found the other on the top of a tree.

THE FACE OF THE SKY.

FROM MAY 9 TO MAY 23.

BY F.R.A.S.

THE Sun will be the subject of daily examination whenever the sky is cloudless. The night sky is shown on Map V. of "The Stars in their Seasons." Mercury is indifferently placed for the observer to-day, and travelling towards his inferior conjunction (where he arrives at 10 o'clock at night on the 17th), he gets worse and worse in this respect. Venus is a magnificent object; by far the most brilliant and conspicuous in the night sky. She still does not set until well on towards midnight, and her glittering crescent forms a crucial test of the excellence of a telescope after dusk. She is travelling through Gemini. On the night of the 13th, she is rather more than 1° N. of ϵ Geminorum ("The Stars in their Seasons," Map II.) Mars, as a big red star, is visible until close upon midnight. He is in a blank part of Leo, and is travelling towards Regulus (Zodiacal Map, p. 70). Jupiter is getting very rapidly towards the West, and must be looked at the moment it is sufficiently dark. His position in Cancer may be seen in the Zodiacal Map on p. 40. The visible phenomena of his satellites occurring before 1 a.m. during the next fortnight are these:—To-night (the 9th) the egress of the shadow of Satellite III. will happen at 9h. 59m.; the transit of Satellite I. begins at 10h. 26m.; and its shadow follows it on to Jupiter at 11h. 38m. p.m. On the 10th, Satellite IV. will disappear in eclipse at 10h. 2m. 25s.; and Satellite I. reappear from eclipse at 11h. 9m. 21s. p.m. On the 16th, Satellite III. will leave Jupiter's disc at 9h. 16m. The passage of this satellite across and off the face of the planet should be carefully watched for any abnormal appearances. Later, Satellite II. will be occulted at 10h. 11m., and the shadow of Satellite III. enters on to Jupiter's limb at 10h. 18m., an hour and two minutes after the satellite casting it has quitted the opposite one. On the 17th, Satellite I. will be occulted at 9h. 38m. p.m. On the 18th, the egress of Satellite I. after transit will happen at 9h. 14m. The shadow of Satellite II. will pass off at 9h. 57m. (the *colour* of this should be noted), and the egress of the shadow of Satellite I. will happen at 10h. 22m. p.m. Lastly, on the 23rd, Satellite III. will enter on to the planet's face at 9h. 55m. p.m. As in the case of the transit of this Satellite on the 16th, the student is again counselled to watch this phenomenon carefully. Saturn has left us until the autumn or winter. Uranus may still be found a little to the west and north of β Virginis ("The Stars in their Seasons," Map V.). Neptune continues to be invisible; coming, as he does, into conjunction with the Sun during the early morning of the 11th. The Moon's age at noon to-day is 13·9 days; and quite obviously 27·9 days at the same hour on the 23rd. Three occultations of fixed stars will occur during the next fortnight. The first one will happen to-night (the 9th); it is that of 6th mag. Star ν^1 Libræ, which will disappear at the Moon's dark limb at 9h. 16m. p.m., at an angle of 350° from the vertex, and reappear at her bright limb at 291° from her vertex, at 9h. 57m. On the 13th, another 6th mag. Star, B.A.C. 6292, will disappear at the bright limb of the Moon, at 11h. 54m. p.m. at an angle from her vertex of 117°; to reappear at her dark limb 40 minutes after midnight, at a vertical angle of 196°. Lastly, ρ^1 Sagittarii, a star of the 4th magnitude, will disappear at the Moon's bright limb at 11h. 35m. in the night of the 14th, at a vertical angle of 33°. It will reappear at her dark limb at 12h. 39m. p.m. at an angle of 275° from her vertex. The Moon is in Libra all day to-day and until midnight to-morrow, when she leaves that constellation to enter the narrow northern strip of Scorpio. This she takes 12 hours, as nearly as possible, to cross, and emerges in Ophiuchus at noon on the 11th. Her passage through this and a little corner of Serpens occupies until 10 a.m. on the 13th, at which hour she enters Sagittarius. It is 11 p.m. on the 15th before she quits Sagittarius for Capricornus, out of which latter constellation she moves at 5 p.m. on the 16th into Aquarius. She does not cross the boundary between this and Pisces until noon on the 19th, and her journey through this great constellation is not completed until the same hour on the 22nd. She then enters Aries, where we leave her.

"Excelsior" (letter 1213, p. 314) surely does not call the mention of Venus in the "Elements of Occultations" on p. 408 of the "Nautical Almanac" a prediction that it would be visible in the United Kingdom! The delay in the appearance of the article on Mercury in a Three-inch Telescope has arisen from the condition of the sky having precluded the possibility of making an accurate sketch of the planet at a suitable altitude. We might, of course, have copied out an account of Mercury from some work on descriptive astronomy, but we must reiterate here that every single object mentioned in the series of papers to which "Excelsior" refers has been specially seen, drawn, and described for them at the telescope itself.

"Let Knowledge grow from more to more."—Alfred Tennyson.

Only a small proportion of Letters received can possibly be inserted. Correspondents must not be offended, therefore, should their letters not appear.

All Editorial communications should be addressed to the Editor of Knowledge; *all Business communications to the* Publishers, *at the Office, 74, Great Queen-street, W.C.* If this is not attended to delays arise for which the Editor is not responsible.

All Remittances, Cheques, and Post Office Orders should be made payable to Messrs. Wyman & Sons.

The Editor is not responsible for the opinions of correspondents.

No communications are answered by post, even though stamped and directed envelope be enclosed.

AN ODD COINCIDENCE.

[1223]—As you are discussing coincidences and superstitions, I will mention an odd one which happened to me not long ago.

I looked into a book in which occurred the statement that the Duke of Orleans, son of Louis Philippe, gave some flowers to his wife upon the morning of his fatal accident. These flowers, it was said, were of a kind (I forget what) regarded in France as appropriate to widows. The gift, therefore, seemed ominous of the coming catastrophe.

Having read this, which, I will add, is quite out of my usual line of reading, I went to the London Library and took up, by mere accident, a book upon botany which was lying upon the table. I never look at botanical books as a rule, but I opened this, and opened it upon the statement that the flowers mentioned in the anecdote were regarded in France as appropriate to widows.

The odd coincidence that I should come upon this statement twice in one morning, though neither a botanist nor a reader of French memoirs, was as curious as the coincidence of the Duke's giving the ominous flowers on the morning of his fatal accident. The only difference was that in my case there could be no apparent meaning.

L. S.

COINCIDENCES.

[1224]—As an example of curious coincidence, the following may be worth your notice. I have recently been appointed editor of the *X.* When appointed, I found the circulation very small and "growing" smaller. A new paper had been started in the neighbouring town of B——, which was injuring our prospects in that town very seriously. In one of its issues there was a verbatim report of a lecture on spiritualism, or some subject of a like nature, the author of which dealt rather freely with "orthodox" religious views, and the result was a vigorous correspondence in the columns of my rival, which seemed to draw all attention from the *X.* in that town. I was determined to do something to divert a portion of the correspondence into the columns of the *X.* I must here say that I did not know a soul in B—— at the time, either by name or otherwise. Well, the lecturer's name was, we will say, John Jones. One morning, then, the day of publication, I was in my *sanctum*, wondering "how to do it," when this idea flashed into my mind, complete and full-grown as I present it:—

"Answers to Correspondents."—F. O. S. (s. i. l.), B——.—We have ventured to add the three last letters in order to complete the significance of your *nom de plume.* Mr. Jones is, in our opinion, perfectly right, and the intolerance and bigotry you display is, of all intolerances, the most disagreeable and (pardon us) the most senseless.

This was, of course, intended to catch Mr. Jones's eye, and so lead him to give us a turn. It didn't have that effect, as it happened, doubtless as a punishment for my sin in committing the fraud. But now comes the coincidence. Some months after, a friend of mine, who knows B—— and most of its inhabitants well, met me, and during our conversation he said: "By-the-way, that was rather too bad of you in connection with Mr. S——." "Who's he," I asked. "Why, Fossil," he replied. "Don't know him." "Oh, everybody in B—— knows him!" he said. "But I'm not a B—— man, and know no one in B——," I said. "Well, that's curious," he remarked. "Everybody who knows F. O. S. supposes the hit was aimed at him; and, what is more singular, there had been a previous controversy between him and Mr. Jones on the very subject of the newspaper correspondence, and to cap the curiosity, he, 'F. O. S,' is regarded by everybody who knows him as a 'fossil' in matters of that kind. He ('F. O. S.') himself takes the thing as a personal affront, and won't have anything to do with the *X.* any more."

Here, then, without the slightest knowledge or clue, I had fixed on the initials of a gentleman's name. I had described his character, and had, so to speak, put my finger on a phase of it at that time more prominent than usual, viz., his hostility to Mr. Jones.

Ed. of *X.* (not Ex-Editor.)

P.S.—You must understand that I had had no communication whatever on the subject of the correspondence either from "F. O. S," or anyone else. The answer was an invention addressed to an imaginary correspondent.

APPARITIONS.

[1225]—Mr. Budd's article, No. 1,181, in Knowledge, of April 11, in which he suggests that a separate action may possibly be carried on in each of the two hemispheres of which the human brain is composed, emboldens me to ask for space in its columns to describe what I myself have frequently fancied I experienced, but have till now thought was due to imagination only, viz., that I am conscious of using the *left* side of the brain under certain circumstances, and the *right* in others.

When studying any interesting book, or when occupied with any subject requiring much reflection, I involuntarily use the *left* hemisphere of the brain. The *right* side I would style the sympathetic one; I seem to use it chiefly when the feelings are roused to sympathy with others, when listening to the strains of fine music, taken as a whole, or when taking part in ordinary conversation; but, if the subject under discussion takes a deeper form, or if I begin to analyse in my own mind the component parts of orchestral music then the *left* side is called into play. I am thus sensible of independent action in the two hemispheres of the brain.

In my case, as far as reading is concerned, this state of things may be induced by the circumstance that the sight of my *left* eye is much longer than that of the *right* one; but of course this cannot apply to thinking out a subject.

I have never as yet made any observations in my own person on the duality of the brain as regards the recalling of past impressions. This is a point which seems much more difficult of determination.

We are not unfrequently told of certain persons who possess the power of dictating several letters, or of playing more than one game of chess at the same time. It would appear that, should such a thing really exist as the independent action of the two hemispheres of the human brain, such persons have this faculty developed in an extraordinary degree, though they may perhaps use it unconsciously.

Cosmopolitan.

[It seems to me very difficult to determine that the right or left side of the brain really *is* called alone into play,—very easy to be deceived. In playing more chess games than one simultaneously (I have often played two, blindfold, at once) there is no use of the two hemispheres separately: I feel sure of that.—R. P.]

A FALSE ALARM.

[1226]—In your article on "Ghosts," in this week's Knowledge, you speak of mistaken noises. I can tell you of a remarkable instance. An old gentleman I knew many years ago had taken to his bed on account of some slight indisposition, when he complained of a "singing" in his ears. His medical attendant, an old-fashioned practitioner, repeatedly bled and cupped him, but the singing still continued, until, one warm day, the window was opened, and he was instantly relieved. The "singing" for which the poor old gentleman had been copiously bled was produced by the wind rushing through a chink in the closed window.

Edgar Flower.

GHOSTS AND GOBLINS.

[1227]—Forty years ago, the nurse of a family living at Lee, in Kent, named G——, was in the frequent habit of visiting our house with the children under her charge, to give them and us an opportunity of playing together, and on one occasion I remember she, with tears and alarm, informed us that Mrs. A. (aunt of her mistress, Mrs. G.) had that morning inquired of her (the nurse) what her mistress was doing at the front gate last night at 12 o'clock? She answered she was not there, she was in bed, having retired before ten o'clock; and that she (nurse) was the last up, and had fastened up the house before eleven o'clock. Mrs. A.

replied that could not be so, as she was certain she saw Mrs. G. at the front gate, looking up and down the road about that time (12 o'clock), that she saw her face quite plainly—although there was no moon—from her bedroom window, after she had put her candle out and was about to get into bed. Nurse here commenced crying and asserting that she was sure her mistress (Mrs. G.), although quite well then, would die soon, or that "something was going to happen." She shortly after left us, and the day after the next called again with the startling and melancholy intelligence that her mistress (Mrs. G.) had died suddenly that morning.

With the exception that the death may have taken place a day earlier or later, I am prepared, notwithstanding the length of time which has elapsed, to make affidavit of the truth of the facts above related (except, of course, those in the nurse's statement) as I was present on both occasions of the nurse's visit. T. E. W.

"TWINKLE, TWINKLE, LITTLE STAR."

[1228]—If Mr. J. W. Howell will refer to *Notes and Queries* (6th S., III., 177), he will find it stated that the original Latin version is to be found in *Arundines Cami*, written by the late Dr. Henry Drury, and it runs thus:—

"Mira, mira parva stella,
Miror quœnam sis tam bella;
Splendens eminus in illo
Alba velut gemma cœlo."

And further, that Dr. Drury attributed the English verses to the "Ryght Pithy Pleasaunt and Merie Comedie intytuled Gammer Gurton's Nedle" [*sic*, "Neele is the word.—R. P.]. Now "Gammer Gurton's Nedle" was published, I think, as early as 1574, and, therefore, it would not be quite accurate to say that the sisters Jane and Ann Taylor "first gave to the world this graceful little poem," although they may have published a version of it "fifty-five years after the death of the pious Dr. Watts." G. G. H.

INFINITY.

TRANSFORMATION OF AN OLD POEM.

"Can the millionth part of an inconceivably minute atom, or a million worlds, be logically termed small or great when we enter upon the consideration of infinity?"

[1229]—Take every man and beast that ever lived, and thereon reckon every single hair; these multiply by *each* atomic mote ever displaced by earth's rotatory sphere. Should these be known, take many millions more as grains of sand on every ocean shore. What! these are told! As many add thereto, as blades of grass that ever on earth grew. Infinituple: still you're far behind as forest leaves e'er shaken by the wind: for many millions more are on the march as starry lamps that gild the spangled arch; and these transpired, as many myriads more as units in the myriads told before, and when all these* are pondered o'er in vain and multiplied by myriads yet again, could we as *finite* beings *then* suppose that vast infinity was near her close, we might *still* persevere—but thought is vain! Infinity's not yet *commenced* her reign! MOLECULE.

SUNSET GLOWS.—SPECTRUM OF THE GLOW.—PON'S COMET.—EARTH TREMORS.

[1230]—We, in this remote corner of the world, are almost precluded from taking part in the discussions that may be going on in European circles, from the fact that by the time accounts of such discussions reach us, and anything we may have to say upon it can return, the subject itself is generally stale. Unfortunately the December number of KNOWLEDGE reached me more than usually late.

The first article in that number was on a subject in which I am particularly interested, namely, the sunset glows. Perhaps, from the fact that I have been not a mere casual observer, but a careful student of the phenomena in question, I may be excused if I offer a few remarks upon the subject. I have discussed pretty freely, from time to time, in our local press, various theories that have been put forward, to my mind more or less absurd and unreasonable. I was, however, at first, rather favourably impressed by Mr. Ranyard's theory in the article referred to; but, on thinking it over, a difficulty presented itself to my mind in connection therewith, to which I feel somewhat diffident in giving expression, from a suspicion that, if real, it must have been obvious to those able scientists in England who have discussed the question. It is this—the idea of a cloud of meteoric dust lying, as it were, asleep in the earth's pathway, is of course absurd, and the thing impossible.

* Miles, years, or worlds, as it may suit the reader's mood in his determination of "Infinity" of distance, time, or bulk, in increasing or diminishing ratio.

Such a body must be moving, and that with planetary or cometary velocity, and in a planetary or cometary orbit. Now, in whatever direction its pathway cuts that of the earth (unless the two orbits are nearly coincident), the velocity of the collision, both bodies moving at a rate exceeding a million miles a day, would produce such a display of celestial fireworks as the world has never seen, at least since it became habitable; and the result would be the destruction of everything sublunary. I cannot, for my part, see how such a conclusion can be evaded, except by supposing both bodies to be moving in nearly coincident orbits, and, of course, in the same direction. But, then, the phenomenon would have been an old and familiar acquaintance, which it certainly is not. I admit that there are some, at least, apparent difficulties in the way of the volcanic-dust theory; but, to my mind, they are of less weight than those attaching to any other theory that I have met with. Adverting to the subject of *dust deposit*, collected from snow-water in Europe, I may mention that I observed, during our January rains, that the rain-water collected in the rain-gauge had a milk-and-watery appearance, as if whiting had been stirred in it. I have carefully preserved the whole of the month's deposit, but have not yet had time to analyse it.

Observing the glow with the spectroscope, I have noticed that it gives a very strong broad band about midway between C and D; its position by Roscoe's frontispiece scale being, as nearly as I can measure it, at 41·5. The band is, I should think, three or four times as broad and deep as the D line. There is also another very broad band on the edge of the yellow, just between the yellow and the green, shading off into the green. I must leave this to others to interpret, as I cannot.

I obtained spectroscopic observations of Comet Pons between Jan. 26 and Feb. 3. I noticed three broadish light lines. These were pretty sharp and decided on the side next the red, but shaded off towards the violet. The spaces between were perfectly dark, although I cannot say that I might not have obtained a continuous spectrum had I been able to grasp more of the comet's light. My telescope is only a three-inch refractor. By means of a "ghost" scale of my own contrivance, specially designed for double star work, I got as accurate a measure as I could of the position of these lines, as follows, reduced to Roscoe's scale: 59·3, 72·2, and 99·5, the D line being at 50. By means of the reflecting prism, I projected the spectrum of common gas flame, turned down to a minute blue point, into juxtaposition with that of the comet, when I found the lines of the two spectra to coincide perfectly, so far as the comet lines were concerned.

We have been experiencing during the last nine months a sensation of which the novelty is beginning to wear off—namely, a continuous succession of minute earth-tremors, sometimes several in a day, very few days passing without them. A. B. BIGGS.

Launceston, Tasmania, March 16.

POTATOES AND FRUIT-TREES IN IRELAND.

[1231]—As the writer of No. 1,218 denies that the poor Irish cottiers are a potato-fed people, and No. 1,219 denies that rack-renting has prevailed in Ireland, it is pretty evident that I am invited to a Donnybrook; but KNOWLEDGE not being a fit arena for a prolonged Irish debate on the Land Question I must decline to tread on the coat-tails so alluringly trailed before me. I may, however, state that I hope to make the further visit suggested by Mr. Milligan as soon as the security of tenure and fair rents enforced by the Land Act have had time to effect the changes which I anticipate as the necessary consequences of securing to the tiller of the soil the fruits of his own industry. Then I may hope to verify his account of the potato being used only as a vegetable supplement to the peasant's dinner, instead of being what I found it during the summers of 1876, 1877, and 1878. When I next have supper, bed, and breakfast in the hut of a worthy and intelligent Irish farmer I hope to find some of the appetising cakes described by Mr. Milligan, "served up hot with plenty of butter" instead of seeing the contents of a cauldron of potatoes turned out on a cloth without dishes, plates, knives, forks, or spoons, and the father, mother, sons, daughters, and self picking them up in our fingers, casting aside their skins, and dipping them into the little pile of salt, which was the only addition to the meal. I hope to find that such will no longer be the daily fare of the honest, gentle, industrious, and intelligent people with whom I have shared it.

In return to Mr. Milligan's desire that I shall thus revisit Ireland, I recommend him during the coming summer to visit *Keel* and *Keem*, on the Island of Achill, and other villages in the midst of the glorious scenery of the west coast of Mayo, Galway, and Donegal. If he would learn something concerning the true condition of his own fellow-countrymen, he must not, when there, merely lodge at the hotels, but try the sheebeens and clay-floored cottages. To see these thoroughly, let him make acquaintance with the

parish priest, and ask him to act as mentor in visiting these interiors; he will thus learn a good deal concerning Ireland of which resident Irishmen of Dublin and other cities are curiously ignorant. W. MATTIEU WILLIAMS.

VEGETABLE FOOD AND FLESH FOOD.

[1232]—I am sorry not to have seen J. Bindon Carter's letter before this, but many engagements have prevented me reading KNOWLEDGE as issued. I may reply that I am a believer in a non-flesh diet on scientific principles, for all my studies so far seem to indicate that such a diet is most suited for man. If I could prove to my own satisfaction that a mixture of flesh and vegetables was more beneficial than the use of a non-flesh diet, then I would at once adopt it, cruelty to animals notwithstanding. But my own experience of over two years, my knowledge of scores of vegetarians, and the testimonies of hundreds of others, all point to the benefit of a non-flesh diet—at least in their cases.

The question of the nutritive values of various foods is very important, and can only be arrived at by experiment. The amount of carbon, nitrogen, &c., a food contains is one thing, the amount the system extracts is another. Experiment seems to show that we never get all the possible nourishment from our food; the length of man's intestine points to a diet in which the nourishment should not be too concentrated. Bulk is a necessity, for it allows of more proper filling up of the intestinal canal; innutritious particles are also necessary, as they allow the gastric and intestinal juices to penetrate more, and also by their presence give rise to daily laxation. Foods which are too rich in nourishment flood the system with material which it has to get rid of, so that by eating a certain quantity of bulky but little nourishing food daily, we tend to keep ourselves in good condition.

A table, showing the chemical composition of food alone is fallacious, and should be verified by actual experiment. Some foods reckoned low in the nutritive scale are found high in the experimental, and *vice-versâ*.

Weight is no criterion to go by, except between certain limits, to exceed or be below which is unhealthy. The diseases of the digestive and excretive organs are all intensified by the use of flesh. Believing as I do in man's evolution, he must be subject to a certain amount of disease, but I think that the disease a man should suffer from should be rather hereditary than acquired, and that with care and an average constitution a man ought never to have any serious illness, *i.e.*, if he lives according to the physiology of his being. The special diseases of vegetarians I have not as yet been able to discover, but it is possible that time and experience may point some out. The vegetarian inhabitants of a country—all things being equal—seem to be superior to those who are mixed feeders. They are taller in stature, more healthful, more cheerful and longer lived, such at least is my experience from observation and reading.

As to the query, if a person may not feel better when he is told that he is looking better, or expects that he should be better, it is a fact that such is the case, as all hypochondriacs relate when put under new treatment. With the editor's permission, I will give next week a table of dietaries showing the value of some foods as shown by experiment. As they are almost unknown they may be of great value to future inquirers and experimenters.

T. R. ALLINSON, L.R.C.P.

A STRANGE INCIDENT.

[1233]—I send you the following facts. On April 23, at 5 p.m., I was trimming my grape-vine in one of my hot-houses, when suddenly a stone, about 1 oz. 13 dwts., came through the largest pane in the house, fell with some considerable force on the brick floor, and split in two pieces. I picked it up, and it burnt my hand most severely before I could leave go of it. I have the stone, which looks a very common one (in two pieces), and if you wish, I will forward it to you.

The hot-houses are 380 yards from the road, and the urchins here seem unacquainted with the sling. I have made inquiries, and no one ever saw an Essex boy with a sling. No one that I know of would have cast a stone on to the hot-house, but, of course, such a hypothesis is possible. The only thing against this is, the stone was red-hot, and burnt my fingers very much. It measures about 3 in. round, and inside appears to be quartz or feltspar, which looks as if it had been exposed to intense heat.

If you feel any interest in this, to me, rather curious incident, I will send you the stone. J. T. NORGATE, General.

[The case seems to me very singular. The stone should be sent to one who has made the structure of meteorites a special study. —R. P.]

CYCLISTS AND FOOT PASSENGERS [ON THE CARRIAGE-WAY].

[1234]—The ignorance and stupidity which foot passengers show in relation with cyclists seem invincible. I was particularly distressed to read in KNOWLEDGE of this week an account by Mr. Browning of the annoyance to which so eminent and responsible a person (in a cycling sense) as himself was subjected by two ladies. They were it seems, quietly walking along the high road, and no doubt, in their own opinion, harmlessly enjoying a common right, and safe in trusting to the most elementary observance of law and courtesy on the part of their fellow-subjects. Alas for their ignorance! They were, in fact, grossly *in delicto*. Firstly, they were in the line which Mr. Browning and his companion, Mr. Cooper, had been pleased to choose for their progress on a tricycle. Only a few eccentrics would care to defend such conduct, for it cannot be too emphatically stated that the purpose of a steering mechanism (in this case the steering of the machine " was marvellous," and could be managed " with perfect certainty within an inch ") is solely to avoid objects dangerous to the rider. Secondly, the ladies were somewhat deaf, and failed to hear the bell, which was condescendingly tinkled for their warning. This fault, though sufficiently irritating, did not exhaust the forbearance of the gallant tricyclists; it seems, however, fortunately for the ladies, to have evoked from Mr. Cooper a mode of expressing his feelings probably quite natural and proper to his disposition. He gave vent to a savage yell. Thus were the ladies saved. But pedestrians must not expect to find the same kind patience in all tricyclists. In the paper this morning it is stated that at Southport a man was fined £1. 4s. for running over a child. This is clearly a misprint. It was the father of the child, no doubt, who was fined for allowing his brat to get in the way.—Yours, &c., VERBUM SAP.

[The principle which our humorous correspondent supports is sound enough. The tricyclist should doubtless show courtesy and consideration to his fellow-passengers. But " Verbum Sap " overlooks the facts of the case. If the ladies really were deaf, it was surely unwise on their part to walk in the part of the roadway intended for vehicles, without frequently looking back to see that no vehicles were coming. Apart from any deafness a foot-passenger who uses the carriage-way ought to keep from the mid-track. Supposing any one, man woman or child, deaf or otherwise, chooses to walk in a part of the roadway not intended for foot-passengers, there is no course for the driver of a vehicle overtaking such an obstructionist but to signal his approach in such a way as to be heard. If, instead, he changes his course at the moment of close approach or even earlier, there is always the risk that the foolish person will at the last moment jump out of the mid-track into the course taken to avoid overrunning him. I am an advocate of the most careful consideration of the rights of others, and I think I carry out the principle to an even unwise degree, for some are not always considerate, and take advantage of the consideration shown by others: but I must confess I can see no other course for Mr. Browning and his fellow-tricyclist than than which was pursued—*i.e.*, to signal with constantly increasing vehemence till attended to.—R. P.]

TRICYCLE PROBLEM.

[1235]—In watching a bicycle or tricycle wheel travelling through dust, one sees a portion of the dust following up the tyre, especially clustering round what would be the outer edge of the tyre. This dust does not complete the circle with the wheel, but is thrown off in a small jet, which, provided there was not a strong wind blowing at the time, would settle as a ridge between the sides of the rut. Mud, being heavier than dust, is thrown off with greater velocity, and as most bicyclists find, lodges on the coat, neck, and hat of the rider.

Hoping this will satisfy " O. M.,"—I am yours,

C. COLVILL, Hon. Sec. Orion B.C.

THE EARTH'S ROTUNDITY.

[1236]—With reference to your familiar illustrations of the rotundity of the earth's surface, I send you one or two easily corroborated by any intelligent observer out here. You are, perhaps, not acquainted with the vast expanses of the " plains of India," prodigious stretches of cultivation unencumbered with tree or mound. On these, when in crop or reaped, you can distictly *see* the swell of the earth's bosom, and others have seen it with me, and didn't require *the backs* of distant grazing kine to corroborate the fact. I have *often* seen the same receding swell in the Bay of Bengal from the deck of a steamer, between me and the hulls down on the horizon. Our great railways crossing over plains also

abundantly illustrate the rotundity of the earth. Take any sketch and you will see it commencing and terminating in the hazy quiver of mirage. Watch for a due train, and you will see the smoke first, then the funnel, and lastly the body of the engine. Do so at night and you will see the *apex* of the red lights' triangle first, and *then the base*.

I might enlarge on the marvellous panorama of distance which you catch, say from the Hill Station of Mussooree (6,653 ft. a. t. s.) from whence you can under favourable circumstances see Meerut, 100 miles south, but the illustrations advanced, familiar to every one, will suffice. R. F. H.

Moror, April 4, 1884.

LETTERS RECEIVED.

J. R. SUTTON.—Have not had time to read your long letter, manifestly wrathful and absurd. I supposed your communication meant for a letter. You are quite mistaken in supposing that letters of that length are not sent in that form or with stamped envelope for return if not used. It is the commonest thing in the world. I thought you had taken a great deal of trouble over it, and that your ideas (which are in fact much the same as I have myself expressed in lecturing on the moon) were sound enough. Therefore, though the communication was incomplete, I inserted it among the letters, with the idea that the writer (obviously a beginner) would be well pleased. I regret that what I regarded as a favour was taken as an offence; and—well, that is about the end of the matter.—J. HARTLETON, NEBRASKA. You have not rightly understood me. Professor Ruskin has done such excellent service to art, he has shown himself so great a master of language, that one cares ill to speak of him in other terms save such as imply esteem and respect. He represents a school of thought moreover which even those who do not greatly sympathise with its teachings must regard with interest. Men who look backward through the blue haze of the past and see a beauty in the remote which had not been felt (if we can judge from what they say of it) by those who saw it close at hand interest us, as a Turner interests us by showing beauties in a haze-wrapt landscape which less poetic vision had failed to realise. If a Turner or a Ruskin however claims to see more through a haze than through a clear air, or more through what we call clear air in England than through the purer air of Italy or America, we cannot but protest against the mistake even if we be not tempted to regard it as a pretence. Such, however, is what Professor Ruskin claimed to do in the lecture at the London Institution on which I commented. He spoke with contempt of scientific researches as showing nothing worth seeing, while artistic vision sees, he said, all that is worth knowing. So utter was the scorn, indeed, with which he denounced the teachings of science, that a tone of similar warmth might seem not unfair if imparted to comments on his own teachings (where at least he left his own domain). But science ought at least to have learned the lesson that in the search for truth which should be the work of science, warmth and scorn and rancour are out of place. Let us rather see whether there is any element of truth in what Professor Ruskin actually found to say against "the scientific people." His subject was a kind of storm-cloud which he regards as new, and claims to have discovered,—a storm cloud or more accurately a plague cloud. Before 1871 the various forms of cloud were healthy and wholesome. Professor Ruskin classifies them rather oddly. He recognises two sorts of clouds, one either stationary or slow in motion, the other fast-flying; the former reflecting unresolved light, the other transmitting resolved light. He has "a suspicion that the prismatic cloud is of finely comminuted water or ice, instead of aqueous vapour." The other cloud therefore he regards (we may safely assume) as *consisting* of aqueous vapour! For he laughs to scorn the scientific notion that aqueous vapour is no more discernible in the air than oxygen or nitrogen. "Not so. With cloud vapour, it is as with most other things, that are seen when they are there and not seen when they are not there, nor has cloud vapour so much of the ghost in it that it can be visible or invisible when it likes, and might perhaps be all unpleasantly and malignantly there just as much when we do not see it as when we do." (We must really not allow "the scientific people" to say that rain or water or cloud vapour can be palpable in one place and impalpable somewhere else, visible here but invisible there. It would be as unreasonable almost as to say that water could be soft under one set of conditions and hard under another). But outside this classification lies another sort of cloud. Consisting neither of comminuted water or ice, nor of cloud vapour, the storm cloud or plague cloud is formed of devil's dust and chimney vomit. With a thickness of perhaps five miles the plague wind chokes the sun out of the whole heaven all day long. But this "thin, scraggy, mangy, miserable cloud, for all the depth of it, could not turn the sun red as a good business-like fog does with a hundred feet or so of itself." This wind in other respects behaves very ill. It blows tremulously, hissing instead of wailing as a respectable wind should, and it simulates that sound so familiar to us all, the tone of a flute made of a file. Moreover, "it pollutes and intensifies the violence of all natural and necessary storms." These are the facts; the meaning Prof. Ruskin suggests is what men would have suggested in the good old days before Magnus and Tyndall and J. W. Draper confused counsel by physical and chemical investigations. It is for our sins we have this wind of darkness, this mangy, miserable, malignant cloud. When we bring back honesty and cheerfulness, cease to trouble our own passions, and watch against the insolence of our own lips, we may again have clear skies or skies only flecked by the fleecy clouds of summer or the darker but transient rain-clouds of former times. Surely the student of science is justified in smiling at all this as sheer windy nonsense.—ONE TAKEN IN BUT STILL HAPPY. I know and have said nothing whatever about the originality of the system; but I suspect that could it so easily be shown to be borrowed as you say, we should have seen this proved long ago.—THOS. S. WILKINS. I fully agree with you as to the great value of F.R.A.S.'s "Nights" (may his days be long). I hope he may be able to arrange for the publication of these and the "Moon Papers" as separate volumes.—J. E. GORE and SENEX. Thanks. Corrected.—H. ASKEW. Thanks.—B. J. HOPKINS. I think letter is in type.—J. B. WILLIAMS. Thanks. Letter shall appear.—KENGE. Your early solution of Mr. Lewis's problem was as you say quite correct. The absence of "Five of Clubs" led to some letters being mislaid.—EL. C. PEARCE. The lecture will be delivered at Richmond (Surrey) some time in June. Nowhere else near London before autumn.—C. H. P. Thanks; will use your odd coincidence if I can find space.—J. W. Streams of cold air coming in through the moisture-laden air within the tent would cause condensation. Had the entering air been cold enough, small snowflakes would have been formed,—a phenomenon which has been frequently observed.—BAUERNSOHN. The writer of the "How to get Strong" papers not being a medical man would not care to advise. But I may mention from my own experience that such swellings are commonly enough noticed after hard rowing. I have been lamed for a week by them in old racing days. But they did me no permanent injury.—A. VON. Y—Z. Fear some readers would not appreciate your idea, in which doubtless there is a germ of truth.—T. H. So many valued friends,—all my *most* valued friends,—are examples to the contrary, that I remain unconvinced. I will not dwell on the fact that I personally object intensely to much that is contained in the treatise you so much admire. The utter immorality of the conduct pursued, (according to the opening pages of the first section) by the leading character in the romance, is *par-trop fort*, though on a par with the principles or utter absence of principle attributed to him later on. That he improves immensely towards the end, I am ready to admit. But one cannot forget his earlier ways. On the whole, the work is not at all to my mind,—so strangely do tastes differ!—J. MACANDREW. Men form their own idea, mostly, of the nature of Deity, each evolving his conceptions from his own inner nature. From what you tell me you think on the subject, I am obliged to infer that you are by nature cold, hard, cruel, and unforgiving. You select descriptions of deity which correspond with your own nature, and overlook those which gentler minds and more long-suffering natures have suggested. Streams cannot rise above their source, nor even "meander" (as that wonderful poet (?) Mr. Robert Montgomery tried to make them) "level with their fount." The God of the unjust is an unjust God; the God of the oriental slave is an oriental despot. I would rather hearken to those who picture Deity as just and merciful. May I venture to make a new proverb for your benefit,—Show me a man's God and I will show you the man.—LLANGOLLEN. I am not myself a Welshman, though in considerable degree of Welsh blood. But I am told that the proper way of pronouncing "ll" in Wales is conveyed in the following formula addressed to a Bishop. "May it please your right reverend lordship to place your episcopal tongue lightly against your right reverend teeth, and hiss like a goose."—J. R. WILLIAMS. "Give it up."—EXCELSIOR. Thanks for suggestions. But you are quite mistaken about the zodiacal signs. At the vernal equinox, the earth's heliocentric position is at the first point of Libra,—the sun's geocentric position being, of course, at the point of Aries.—I. J. COLLINS. Your interesting case of coincidence shall certainly appear.—W. H. FRANCE. Many thanks; will insert if can get space.—G. H. WILKINSON. We must make allowances for a daughter defending her father's memory.—E. A. M. Thanks; many readers seemed to think we had dealt already too fully with the subject.—H. JONES. "Five of Clubs" does not play Nap; nor do I.—B. M., F.R.C.S. Article on "Ghosts" is *not* "intended altogether to demolish the existence of those occurrences as supernatural facts."

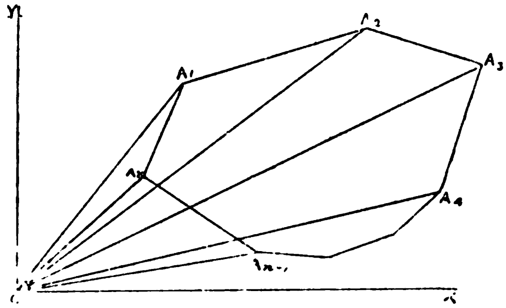
Our Mathematical Column.

EASY LESSONS IN CO-ORDINATE GEOMETRY.

By RICHARD A. PROCTOR.

(*Continued from page* 298.)

17. *To express the area of a polygon in terms of the co-ordinates of its angular points.*



Let $A_1, A_2, A_3, A_4 \ldots\ldots A_{n-1}, A_n$ be the angular points of a polygon, and let the co-ordinates of these points be respectively $x_1y_1, x_2y_2, x_3y_3, \ldots\ldots x_{n-1}y_{n-1}, x_ny_n$; then the area of the polygon.

$$= \Delta OA_1\,A_2 + \Delta OA_2\,A_3 + \ldots. - \Delta OA_{n-1}\,A_n - \Delta OA_n\,A_1$$

$$= \frac{1}{2}\left\{x_2y_1 - x_1y_2 + x_3y_2 - x_2y_3 + \ldots + x_ny_{n-1} - x_{n-1}y_n + x_1y_n - x_ny_1\right\}$$

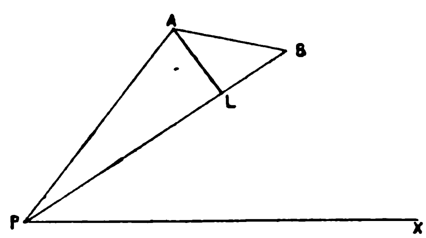
which may be written in either of the forms

$$\frac{1}{2}\left\{x_1(y_n - y_2) + x_2(y_1 - y_3) + x_3(y_2 - y_4) + \ldots. + x_n(y_{n-1} - y_1)\right\}$$

or $$\frac{1}{2}\left\{y_1(x_2 - x_n) + y_2(x_3 - x_1) + y_3(x_4 - x_2) + \ldots. + y_n(x_1 - x_{n-1})\right\}$$

We have supposed in 14, 15, 16, and 17 that the axes are rectangular; if the axes are oblique, and ω is the angle of ordination, it is evident the expression obtained in Article 14 must be multiplied by $\sin\omega$, to give the correct area. Hence all the expressions obtained in 15, 16, and 17 must be multiplied by $\sin\omega$, when the axes are oblique.

18. *To determine the area of the triangle* P A B, *one of whose angles is at the pole, in terms of the polar co-ordinates of the other angular points.*



Let the co-ordinates of A be r_1, θ_1; those of B, r_2, θ_2. Draw A L square to P B. Then

$$\text{Area APB} = \tfrac{1}{2}\,\text{AL}\cdot\text{PB}$$
$$= \tfrac{1}{2}r_1r_2\sin(\theta_1 - \theta_2)$$

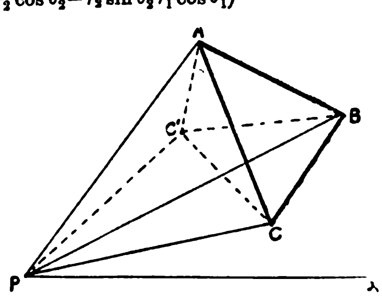
This expression always represents the area correctly in quantity, but is positive or negative according as $\theta_1 > \text{or} < \theta_2$.

SCHOL. Writing the expression just obtained

$$\tfrac{1}{2}(r_1\sin\theta_1\,r_2\cos\theta_2 - r_2\sin\theta_2\,r_1\cos\theta_1)$$

we see that it might have been obtained from the expression in rectangular co-ordinates, or conversely that expression might have been obtained from the expression in polar co-ordinates.

19. *To determine the area of a triangle* A B C, *in terms of its polar co-ordinates.*



Let the co-ordinates of the points A, B, C, be respectively r_1, θ_1; r_2, θ_2; r_3, θ_3. Then

$$\Delta ABC = \Delta AOB + \Delta BOC - \Delta AOC$$

$$= \frac{1}{2}\left\{r_1r_2\sin(\theta_1 - \theta_2) + r_2r_3\sin(\theta_2 - \theta_3) + r_3r_1\sin(\theta_3 - \theta_1)\right\}$$

This is always the proper quantitative expression for the area. It might have been obtained from the expression in rectangular co-ordinates, and conversely the latter expression might have been obtained from the above. The condition for the expression to be positive is sufficiently obvious,—as is also the change which has to be made if the third angle falls as at C′ instead of as at C.

I have included 19 among the examples which follow, and a further proposition which the reader will find no difficulty in solving relating to the expression for the area of a polygon in terms of the polar co-ordinates of its angular points.

EXAMPLES.

(1) Indicate in a figure the points whose rectilinear co-ordinates are

(i) $x=2, y=3$;	(ii) $x=3, y=2$;
(iii) $x=-2, y=3$;	(iv) $x=3, y=-2$;
(v) $x=2, y=-3$;	(vi) $x=-3, y=2$;
(vii) $x=-2, y=-3$;	(vii) $x=-3, y=-2$;

first, when the axes are rectangular; secondly, when the axes are oblique and the angle of ordination $=60°$.

(2) Indicate in a figure the points whose polar co-ordinates are

$$\text{(i) } r=2,\ \theta=\frac{\pi}{6};\ \text{(ii) } r=-2,\ \theta=\frac{2\pi}{3};$$

$$\text{(iii) } r=2,\ \theta=-\frac{\pi}{6};\ \text{(iv) } r=-2, \theta=\frac{4\pi}{3};$$

(3) Write down the rectilinear co-ordinates of all the points in Ex. 2; and find the polar co-ordinates of all the points in Ex. 1, the axes being supposed to be rectangular.

(4) Determine the lengths of the lines joining the following pairs of points in Ex. 1:—

(I) points (i), and (ii); (II) points (i) and (iii);
(III) points (i) and (iv); (IV) points (i) and (vi);
(V) points (i) and (vii); (VI) points (i) and (viii).

Determine also the lengths of the lines joining every pair of points in Ex. (2.)

(5) Find the co-ordinates of the middle point of the line joining the points x_1y_1, x_2y_2.

(6) The co-ordinates of the angular points of a triangle are x_1y_1, x_2y_2, and x_3y_3. A straight line is drawn from any one of the angular points to the bisection of the opposite side, and trisected. Find the co-ordinates of the point of trisection remote from the angular point.

Hence show that the three lines drawn from the angular points to the bisections of the opposite sides pass through one point.

(7) There are four points whose co-ordinates are x_1y_1, x_2y_2, $x_3\,y_3$, and $x_4\,y_4$. Any two of these points are joined, and also the remaining two, and the lines thus drawn are bisected. Find the co-ordinates of the point of bisection of the line joining the two points thus obtained.

Hence show that the lines joining the bisections of opposite sides of a quadrilateral, and the line joining the bisections of the diagonals, pass through one point, and are all bisected in that point.

(8) Show that the area of the triangle the co-ordinates of whose angular points are $(a-b)$, $(a+b)$; $(b-c)$, $(b+c)$; and $(c-a)$, $(c+a)$, is

$$= ab + bc + ca - a^2 - b^2 - c^2$$

(9) Show that the area of a triangle the polar co-ordinates of whose angular points are (r_1, θ_1), (r_2, θ_2), and (r_3, θ_3), is

$$\pm\frac{1}{2}\left\{r_1r_2\ \sin\ (\theta_1 - \theta_2) + r_2r_3\ \sin\ (\theta_2 - \theta_3) + r_3r_1\ \sin\ (\theta_3 - \theta_1)\right\}$$

(10) Show that the area of a polygon the polar co-ordinates of whose angular points taken in order round the polygon are (r_1, θ_1), (r_2, θ_2), (r_3, θ_3), (r_4, θ_4), ... (r_{n-1}, θ_{n-1}) and (r_n, θ_n) is

$$\pm\frac{1}{2}\left\{r_1\,r_2\sin(\theta_1 - \theta_2) + r_2r_3\sin(\theta_2 - \theta_3) + r_3r_4\ \sin\ (\theta_3 - \theta_4) + \ldots\right.$$
$$\left.\ldots\ldots + r_{n-1}r_n\ \sin\ (\theta_{n-1} - \theta_n) + r_nr_1\ \sin\ (\theta_n - \theta_1)\right\}$$

THE production of coal in the United Kingdom in 1883 was 163,737,327 tons, against a total output of 156,499,977 tons in the preceding year. This is an increase of 7,237,350 tons, or 4·6 per cent., on the production of 1882. To this increase the chief contributors were:—Glamorganshire, with an advance of 1,315,487 tons; Yorkshire, with an advance of 1,037,399 tons; Scotland, with an advance of 710,633 tons; Lancashire, with an advance of 704,672 tons; Monmouth, with an advance of 623,542 tons. The total production of coal in 1882 exceeded by 2,315,677 tons the output of 1881, and by 9,530,568 tons the output of 1880. Within the last three years, therefore, the production of coal in the United Kingdom has increased to the extent of 16,767,918 tons. Mr. J. S. Jeans, secretary of the British Iron Trade Association, remarks:—"It is not without interest to note that this amount of increase is within 4,000,000 tons of the whole coal output of France, is approximately the same as that of Belgium, and is more than equal to the whole annual production of the United Kingdom previous to 1810.

Our Chess Column.

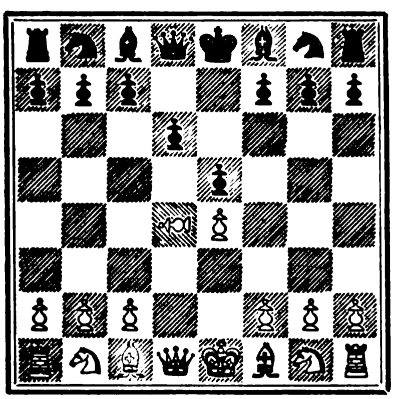
EASY NOTES ON THE CHESS OPENINGS.

By RICHARD A. PROCTOR.

OUR Chess Editor "Mephisto" keeps his eye, at my especial request, on this part of our Chess matter. He has played, I suppose, five hundred games at least, for every game I have played, and is a Chess genius besides; so that he can at once mark any weak point in my remarks on the various openings. Thus, dealing with the line 1. $\frac{\text{P to K4}}{\text{P to K4}}$ 2. $\frac{\text{Kt to KB3}}{\text{P to Q3}}$ 3. $\frac{\text{P to Q4}}{\text{P takes P}}$ 4. Kt takes P. I remarked (in my original MS.) that in response to 4. $\overline{\text{Q to KR5}}$ a move of my own devising, White's best play seems to be 5. Q to Q3. "Mephisto" substitued 5. Kt to QB3 as probably a better move. His objection to my suggested fifth move for White is probably that Q at Q3 is in the path of the KB. I should like to have a note by "Mephisto" on the move 5. $\overline{\text{B to KKt5}}$ in reply to his suggested fifth move. If White Queen now moves she hampers one or other of the Bishops; if King's Bishop interposes, Black can exchange Bishops, and equalise the positions; if KKt interposes at KB3, 6. $\overline{\text{Q to KR4}}$ seems good for Black. However "Mephisto" may see some weak point in this move 5. $\overline{\text{B to KKt5}}$.*

We now proceed to examine the consequences of Black declining at the third move to take the proffered Queen's Pawn. It is well known, though oddly enough Chess writers never seem to think it worth while to show it, that in the Scotch Gambit, where White at his third move pushes the Queen's Pawn to its fourth square, after Black had defended his King's Pawn with the Queen's Knight, Black gets a very bad game if he fails to take the proffered Pawn. Therefore if Black after 1. $\frac{\text{P to K4}}{\text{P to K4}}$ 2. $\frac{\text{Kt to KB3}}{\text{P to Q3}}$ 3. $\frac{\text{P to Q4}}{\quad}$ reply with 3. Kt to QB3, he has a bad game; but the discussion of its weakness may properly be left to the time when we are dealing with the Scotch Gambit. As we have already noted the best reply to White's third move, if the Pawn is not captured, is Kt to Q2. But three others are available. The position is

BLACK.



WHITE.

Position after 3. P to Q4.

I leave to the last the move which really completes Philidor's defence—viz., 3. $\overline{\text{P to KB4}}$, considering the three following lines, one of which bifurcates:—

3.	$\overline{\text{Kt to Q2}}$	$\overline{\text{Kt to KB3}}$		$\overline{\text{B to KKt5}}$
4.	B to QB4 (a) / Kt to Kt3	Kt to QB3 / P takes P	P takes P (b) / Kt takes P	P takes P / B takes Kt
5.	B to QKt3 / P takes P	Q takes P / B to K2	B to QB4 / P to QB3	Q takes B / P takes P
6.	Q takes P* / B to K3	B to K3 / Castles	Castles / P to Q4	B to QB4 / Q to Q2
7.	Kt to QB3 / Kt to K2	Castles (Q) / Kt to QB3	B to Q3 / Kt to B4	Q to QKt3 / P to QB3
8.	B to K3 / Kt to QB3	Q to Q2 / P to QR3	B to K3 / B to Kt5	P to QR4 / B to Q3
9.	Q to Q3 / Kt to K4	B to Q3		Castles
	Equal game.	White has the better game.	Equal game.	White has the better game.

These four lines are precisely as given in Mr. Cooke's excellent "Synopsis of the Openings," of which work (I am glad to hear it from headquarters) a new edition is now in the Press. As one of my chief objects in these simple analyses has been to bring the various openings before home Chess players as presented by the leading authorities, and Mr. Cooke's book does this effectually and very cheaply, I shall now (that I know his book is soon to be reprinted) adopt a slightly different course, showing *not* what are the various lines which may be followed in the different openings, but what lines, among those which might be followed, the student of home Chess may conveniently select for study. I shall in fact continue these papers with the object of simplifying as much as possible the study of the openings, endeavouring to correct at the outset the idea that to have a chance of success in Chess play with those who know all the openings the learner should be equally well versed in them. Just as in Whist "Five of Clubs" showed that half-a-column could be made to contain all that the learner need know about the leads (which in the "Correct Card" look like legion) so shall I here endeavour to show that a few nights' study of certain leading lines will put the home player on an equal footing, so far as book-lore is concerned, with the player who knows the approved play on every known line of opening hitherto adopted.

NOTES.

(*a*) Or White may play P to QB3, as recommended by Steinitz, a move which practically prevents the capture of the Queen's Pawn by Black.

(*b*) B to KKt5 is better, because practically it forces Black to take the Queen's Pawn, after which if White plays sound developing strategy (not retaking the Pawn at the moment), he seems to me to get the better game.

(*To be continued*).

ANSWERS TO CORRESPONDENTS.

**** ***Please address Chess Editor.***

W.—Thanks for Problem, which is welcome. We shall be pleased to receive a few more such selected masterpieces.

W. SHERRARD.—Problem received with thanks. Your three-mover reached us twelve months ago, when, after examining it, did not there and then destroy it; hence its appearance.

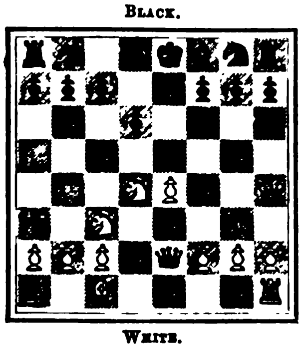
CORRECT solution of End Game received from Clarence, W. H. L. M. (Dublin), Fui, James Rayner, S. B. B., George Robson, A. Rutherford, Wm. H. Curtis, W., A. K. Lane, A. H. S., A. Hill, Herbert F. Lowe.

PROBLEMS correctly solved by Donna, Punch, S. B. B., W., Clarence, H. Seward, John Watson, Uncle John, Edward Sargant, Fill.

CORRECTION Problem, No. 117, can be solved by 1. R to Kt3 (ch). This, however, is not the author's intended solution.

* In reply to B to Kt5 White plays 6. B to K2, B takes B. 7. Q takes B. A little study of this position will prove that White has the best of it. White has two pieces developed, and all his forces have great liberty of action; he may also Castle. Black has no pieces developed, his Queen will soon have to retire, and his forces are cramped. White's Kt on Q4 occupies a very strong position. If Black plays P to QB4 his Q's P would be hopelessly compromised, as White subsequently could bring Kt, B, and R to bear upon it. Besides these general disadvantages, Black is threatened with a check by Q to Kt5. If to prevent this Black plays 7. P to QR3, White again gains important time for Castling, to be followed by P to KB4, or he may play 8. Kt to Q5 at once. 7. P to QB3 would not be an improvement on P to QR3, as that would render the QP weak. In any case White obtains a superior development. I take this opportunity of drawing attention to the commanding position occupied by a Kt on Q4 if the adverse KB is blocked in. This is the case in some variations of the Scotch Gambit and the Three Knights' Game, and forms a great objection to this opening.—MEPHISTO.

BLACK.



WHITE.

A CORRESPONDENT asks if, after 1. P to K4, P to K4; 2. Kt to KB3, P to Q3; 3. P to Q4, P takes P; 4. B to B4, Black could not now play B to Kt. 5. There is nothing to prevent him doing so. White might continue with 5. P to B3, threatening to play Q to Kt3.—MEPHISTO.

* I prefer 6. Kt takes P.—MEPHISTO.

Our Whist Column.

By Five of Clubs.

ILLUSTRATIVE GAME.

THE following game from the *Westminster Papers* illustrates Whist Malignity. We believe, but are not sure, that our correspondent Pembridge, the esteemed author of "Bumblepuppy," was the unhappy *A*.

THE HANDS.

B { *D.* Kn, 10. S. 10, 9, 4, 3. C. Q, 8, 3. H. K, 8, 7, 4. }

Y { *D.* 3. S. Kn, 8. H. Kn, 5 C. K, Kn, 10, 9, 7. }

A-B, 0. B

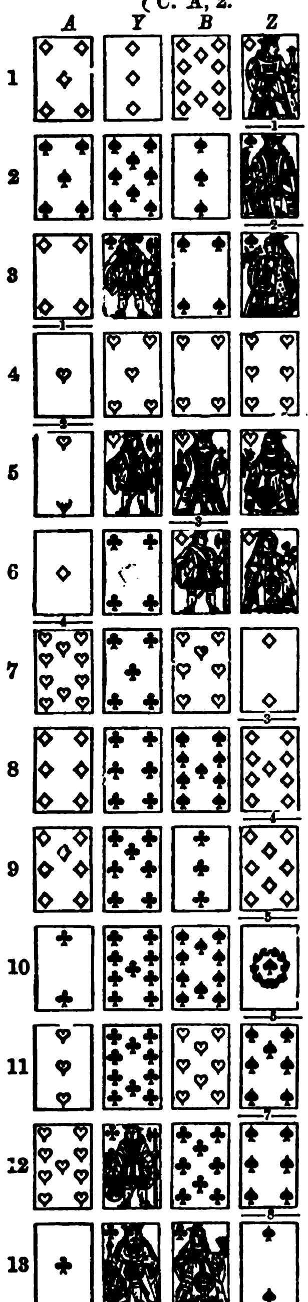
Y *Y-Z*, 2.

Tr. DQ. Z

A Leads.

Z { 2, 8, 9, Q, K. *D.* 6, 7, Q, K, A. S. 6, Q. H. [6, 5, 4. 2]. None. C. }

A { *D.* A, 7, 6, 5, 4. S. 5. C. A, 2. H. A, 10, 9, 3, 2. }



NOTES AND INFERENCES.

1. *A* properly leads trumps from Five; and in trumps it is better to lead the penultimate than the Ace. *Z* plays the King, because the Queen was turned up.

3. *B* now knows that all the remaining Spades except the two he holds, or four Spades, are with *Z*. Let us see what use he will presently make of this knowledge.

4. Having been forced, *A* does not go on with trumps. He leads Ace from his five-card plain suit.

6. *B* leads trumps (though his partner has discontinued) because of the Queen turned up. In this *B* was right, as there is good reason for expecting that after this second round *A* would draw two trumps for one. But *Y* failing to follow suit shows *B*'s lead to have been unfortunate.

8. *B* sees that *Z* means to draw out *A*'s last trump, and to bring in Spades, which make up the rest of *Z*'s hand. But he himself holds second and third best. So he holds the command in the suit, and has nothing to do but to discard two small Clubs, suffer his Spade Nine to fall to *Z*'s Ace, and then, winning the next trick in Spades, bring in his partner's Hearts. Wherever Club Ace and King may be, this course is absolutely sure, and three tricks must thus be made. But *B* craftily seizes on the only possible course by which every remaining trick can be made over to *Y-Z*.

9, 10, 11, 12, 13. *B*'s triumph and *A*'s discomfiture.

To "Pembridge" whose interesting letter on "Whist Decisions" appeared in these columns a few weeks ago, is due the theory that *B*'s play in the above game was suggested by malignity. The theory seems supported by strong evidence, especially as we are told that *B* deliberated for two minutes before playing to trick 8. He cannot have been deliberating about his play, for there could be no manner of doubt about that. Clearly a contest was going on between hatred on the one hand and the compunctious visitings of conscience on the other. Alas that his evil feelings should have prevailed at last!

LANGENHOE CHURCH AND THE RECENT EARTHQUAKE (Extract from *Standard*, April 24, 1884).—"I had heard that Langenhoe Church was practically destroyed, but was harldy prepared for such a scene of wreckage as I found. It is, however, now little better than a ruin. The earthquake shook the old Norman tower to and fro, dislodging some tons of the stone battlements upon the roof, one half of which it carried away in its downward course, smashing the small gallery below, and filling half the nave breast-high with a confused heap of timbers and stone-work. At the same time the outer walls cracked in various directions, and the roof over the chancel fell in upon the communion-rail and pews below. The east window has become displaced, all the walls, even of the brick porch, a comparatively new part of the structure, are bulged and warped, and the tower, in which several fissures are observable, is considerably out of the perpendicular." We have received from Mr. Browning a very interesting photograph of the church, showing its condition since the earthquake. It is well worth studying.

CONTENTS OF No. 131.

SPECIAL NOTICES.

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The following arrangements are complete: the numbers in brackets referring to above list.

WORCESTER, May 8, 9, 15, 16 (1, 2, 3, 4).
OXFORD, May 12, 13, 19, 20 (1, 2, 3, 4).
BANBURY (The Moon), May 14.
MALVERN (The Moon), May 17.
CAMBRIDGE, May 21, 22, 23 (1, 2, 3).
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NOTE.—*All communications respecting Lectures should be addressed to Mr. John Stuart, Royal Concert Hall, St. Leonards.*

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AN ILLUSTRATED
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PLAINLY WORDED—EXACTLY DESCRIBED

LONDON: FRIDAY, MAY 16, 1884.

Contents of No. 133.

SENT TO THE BOTTOM.

By Richard A. Proctor.

After every great accident by fire or by water or by storm, too much is said and too little is done, about measures of prevention. Unpractical men suggest impossible devices; precautions are urged which could not possibly be adopted; and then after a little time matters return to their old course until the next great accident of the same kind sets men talking again but too often doing little or nothing.

But in the case of collisions at sea and especially of such collisions as have recently occurred, there is so wide a recognition of the utter inadequacy of the measures actually adopted to avoid accident, that one really marvels how those whose lives and property depend on proper arrangements being employed can for a single year or even a single month allow matters to proceed without change. Indeed if it were not that so much were at stake, so many lives and such valuable property involved, one might be disposed to smile at the almost childish way in which sailors ("Sing, hey, the simple seamen that they are") propose to guard steam-vessels from the danger of collision in foggy or hazy weather, or at night. Everything it should seem depends on a ready and rapid recognition of the real position and course of a ship that is sighted by the look-out; yet by the arrangement actually adopted the signal lights tell little or nothing even when they have been sighted, and matters are so ingeniously planned that they are sighted only at distances far within those over which, were proper measures adopted, they might well be seen.

Let it be noticed in the first place that the use of coloured lights at sea is a delusion and a snare. They do well enough on a railway (except in foggy weather); for there they are only required to throw their rays over a defined and moderate distance. But at sea the further a signal light is seen, no matter what its purpose is, the better; and the interposition of coloured glass limits in marked and measurable degree the range over which a light casts its beams. If there were no other way but by the use of colour that the meaning of a signal light could be indicated there might be some excuse for the preposterous course by which the power of these safety-lights (as they should be) is diminished. But there are many ways in which white lights can be made to serve all the purposes of coloured lights in the way of signalling, and as they can be seen much further, they signal much more effectively.

Again the use of but two sidelights, one on the port the other on the starboard beam, would in these days be an absurdity if it were not an atrocity. What can a single light *say?* Let it be as strong as it may it can only tell the people of another ship that the port side or the starboard side of another ship is turned towards them at the moment, when perhaps everything depends on their knowing *in what degree* that side is turned their way. The other ship may be steering on a course at right angles to the direction in which she is seen, or at a sharp angle to that direction so as to be rapidly drawing nearer, or at an obtuse angle so as to be rapidly drawing away. And note that the uncertainty which the people on one ship have as to the position and bearing of the other is reciprocated by equal uncertainty on the part of the people on the latter ship as to the position and bearing of the former. Consider what is likely to come of such uncertainty, at night, when it is remembered that even in broad daylight ships that have been in sight of each other for some time, and have been able severally to watch each manœuvre, have been known to come into collision. If with everything to favour them, two ships do sometimes run into each other (through some such confusion of purpose as sometimes causes two passing pedestrians to dodge from side to side, while each is as anxious as the other to pass clear) how much greater must the risk always be at night or in hazy weather, no matter what precautions may be adopted? Yet with an almost childish simplicity those who have arranged the Rule of the Road at sea provide for no other precaution than that a red light shall be hung on the port side and a green light on the starboard side of a steamship at night.

Now it may be laid down as a tolerably sound proposition that even the smallest steamship is worth proper signal lights, even if so many as a dozen, instead of two or three, should be necessary to ensure safety. For passengers or for freight, a slightly higher charge might, if necessary, well be made to secure at least such protection as proper side-lights might give. If a steamship herself is so utterly worthless that her owners cannot afford either for her or for any freight live stock or passengers she may have to carry the extra expense which half-a-dozen more lights at night would give, she ought not to be free to run happy-go-lucky over the seas (as all steamships do now,—and sailing ships are worse). She may cause destruction to ships and cargoes of much greater value. If sunk she may obstruct navigation for weeks. She is in fine not worth sending to sea if she is not worth such proper lighting as will make her safe herself and no longer a source of danger to other ships.

We may go a little further in this direction. Every steamship is worth proper lighting at sea, and not one is now properly lighted; but the large and swift ocean steamers are at once better worth effective lighting, and more urgently need it, whether we consider their swift course as regards themselves or as it affects the safety of others. If it is not too much to say that the smallest steamship afloat in the open sea should carry at least eight good white lights, it certainly may be as truly said that the *Germanic* and the *Oregon*, the *City of Rome* and the *Gallia*, should carry as many strong electric signal lights at least in foggy or hazy weather and when nearing the sea regions where steamships most do congregate. The same remark may of course be extended to large steam war vessels.

It may very well be that a long time will elapse before the owners of our great ocean steamers will recognise the economy of suitable electric lighting. A certain amount of mechanical power devoted to work a dynamo machine may seem to them wasted, when they want every cubic inch of steam devoted to the propulsion of the mighty steamship across the ocean. It seems so unlikely that the big ship will be the one to suffer seriously in any collision, so certain that aught which may come in her way will simply be sent to the bottom, that a wealthy and selfish owner may be tempted to disregard the rights of others and to overlook how largely the loss brought about by one such catastrophe as the destruction of the *City of Brussels* must overvalue the expense of efficient electric signal lamps in constant use for twenty or thirty years.

But apart from this it should be noticed that the time when electric lights would be most useful—that is in thick and hazy weather—is a time when they could be maintained without any loss of power at all. When the engines are at rest or slowed down, there is an abundance of energy available for working a dynamo-electric machine by which if necessary a score of powerful electric lights could be maintained. In a very short time, when the *City of Brussels* had been brought to rest on account of the fog which actually brought about her destruction, electric lights might have been set aglow, which would have shown her position to the *Kirby Hall* in ample time to have prevented that disaster. The *Kirby Hall* herself might equally have born electric signal lights at the time if as is stated she was going dead slow.

What electric lights would be worth in time of haze and fog at sea those can judge who have ever witnessed experiments for comparing the haze-penetrating power of the electric light, gas, the best oil lights, and coloured lights. It is certainly no exaggeration to say that in an ordinary sea fog, a good electric light would show at least ten times as far as the best coloured side lights now in use. If half-a-dozen electric lights or so were shown on each side, the distance at which the neighbourhood of a ship would be recognised would be increased in yet greater degree, for the glare would be discernible before the individual lights would be seen.

(*To be continued.*)

THE CHEMISTRY OF COOKERY.

XXXIV.

By W. Mattieu Williams.

RESPECTING the rationale of the change that takes place in reheating stale bread, thereby renewing it and making it appear moist by actually driving away some of its moisture, the results of my investigations are as follows:—

I find that as bread becomes stale, its porosity *appears* to increase, and that when renewed by reheating, it returns to its original *apparently* smaller degree of porosity. That this change can be only apparent is evident from the facts that the total quantity of solid material in the loaf remains the same, and its total dimensions are retained more or less completely by the rigidity of the crust. I say "more or less," because this depends upon the thickness and hardness of the crust, and also upon the completeness of its surrounding. Lightly-baked loaves shrink a little in dimensions in becoming stale, and partly regain the loss on reheating, but this difference only exaggerates the apparent paradox of varying porosity, as the diminished bulk of a given quantity of material displays increased porosity, and the increase of total dimensions accompanies the diminished porosity.

A reconciliation of this paradox may be obtained by careful examination of the structure of the crumb. This will show that the larger or decidedly visible pores are cells having walls of somewhat silky appearance. This silky lustre and structure is, I have no doubt, due to a varnish of dextrine, the gummy nature of which I have already described. Now look a little more closely at this inner surface of the big blow-holes with the aid of a hand-lens of moderate power. It is not a continuous varnish of gum, but a net-work or agglomeration of gummy fibres and particles, barely touching each other.

My theory of the change that takes place as the bread becomes stale is that these fibres and particles gradually approach each other either by shrinkage or adhesive attraction, and thus consolidate and harden the walls of each of the millions of visible pores, *i.e.*, the solid material of which the loaf is made up. In doing so they naturally increase the dimensions of these visible pores, while the invisible interstices or spaces between the minute fibres of the cell walls are diminished by the approximation or adhesion of these fibres to each other.

This adhesion is probably aided by an oozing out or efflorescence of the vapour held by the fibres, and its condensation on their surfaces. This point, be it understood, is merely hypothetical, as the efflorescence is not visible.

When the stale bread is again heated, a general expansion occurs by the conversion of liquid water into aqueous vapour, every grain of water thus converted expanding to 1,700 times its former bulk. As this happens throughout, *i.e.*, upon the surface of every one of the countless fibres or particles, there must be a general elbowing in the crowd, breaking up the recent adhesion between these fibres and drawing them all apart in the directions of least resistance, *i.e.*, towards the open spaces of the larger and visible pores, producing that *apparent* diminution of porosity that I have observed as the visible characteristic of the change.

This explanation of the change may be further demonstrated by cutting a loaf through the middle from top to bottom, and exposing the cut surfaces. In this case the bread becomes unequally stale, more so near the cut surface than within. The unequal pull due to the greater adhesive approximation of the fibres and small particles causes a rupture of the exposed surface of the crumb, which becomes cracked or fissured without any perceptible alteration of the size of the visible pores. If the two broken faces be now accurately placed together, the halves thus closely joined firmly tied together and placed for an hour in the oven, it will be seen on separating them that the chasms are considerably closed, though not quite healed. Careful examination of the structure of the inside, by breaking out a portion of the crumb, will reveal that loosening of the structure which I have described.

I should add that, in quoting the figures given by Boussingault in my last, I inadvertently omitted to reduce them from the French to the English thermometric scale. 130° to 150° Centigrade is equal to 266° to 302° Fahrenheit, which is considerably below the temperature required for starting the original baking.

"Popped corn" is a peculiar example of starch cookery. Here a certain degree of porosity is given to an originally close-compacted structure of starch by the simple operation of explosive violence due to the sudden conversion into vapour of the water naturally associated with the starch. The operation is too rapid for the production of much dextrin.

As most of my readers doubtless know, peas, beans, lentils and other seeds of leguminous plants are more

nutritious, theoretically, than the seeds of grasses, such as wheat, barley, oats, maize, &c. I was glad to see at the Health Exhibition a fine series of the South Kensington cases displaying in the simplest and most demonstrative manner the proximate analyses of the chief materials of animal and vegetable food. I refer to them now because they do not receive the attention they deserve. On the opening day there was, out of all the crowd, only one other besides myself bestowing any attention upon them. I soon learned in conversation with him that he is a reader of KNOWLEDGE. These cases show 1 lb. of wheat, oats, potatoes, peas, &c., &c., on trays; by the side of these are bottles, containing the quantity of water in the 1 lb., and other trays, with the other constituents of the same quantity, the starch, gluten, casein, the mineral matter, &c., thus displaying at a glance the nutritive value of each so far as chemical analysis can display it. Those Irishmen and others who think I have been too hard upon the potato, will do well to take its nutritive measure thus, and compare it with that of other vegetable foods.

They will see that all the leguminous seeds, the groundnuts, &c., have their nitrogenous constituents displayed under the name of "casein." The use of this term is rather confusing. In many modern books it does not appear at all in connection with the vegetable kingdom, but is replaced by "legumin." Liebig regarded this nitrogenous constituent of the leguminous seeds, almonds, &c., as identical with the casein of milk, and it was a pupil and friend of Liebig's—the late Prince Consort—who devised and originally supervised this graphic method of displaying the chemistry of food.*

I will not here discuss the vexed question of whether the analyses of Liebig, identifying legumin with casein, or rather those of Dumas and Cahours, who state that the vegetable casein is not of the same composition as animal casein, are correct.

The following figures display my justification for thus lightly treating the discussion:—

	Casein.	Legumin.	Legumin.	Legumin.
Carbon	53·7	50·50	55·05	56·24
Hydrogen	7·2	6·78	7·59	7·97
Nitrogen	16·6	18·17	15·89	15·83
Oxygen } Sulphur }	22·5	24·55	21·47	19·96

The first column shows the results of Dumas for animal casein; the second those of Dumas and Cahours for legumin; the third those of Jones for the same; and the fourth those of Rochleder; all as quoted by Lehmann. Here it will be seen that the differences upon which Dumas and Cahours base their supposed refutation of the identity of the animal with the vegetable principle are much smaller than the differences between the results of different analyses of the latter. These differences I suspect are all due to the difficulty of isolating the substances in question, especially of the vegetable substance, which is so intimately mixed with the starch, &c., in its natural condition that complete separation is of questionable possibility.

This will be understood by the following description of the method of separation as given by Miller ("Elements of Chemistry," Vol. 3). "Legumin is usually extracted from peas or from almonds, by digesting the pulp of the crushed seeds in warm water for two or three hours. The undissolved portion is strained off by means of linen, and the turbid liquid allowed to deposit the starch which it holds in suspension; it is then filtered and mixed with dilute acetic acid. A white flocculent precipitate is thus formed, which must be collected on a filter and washed."

This is but a mechanical process, and its liability to variation in result will be learned by anybody who will repeat it, or who has separated the gluten of flour by similar treatment.

Practically regarded in relation to our present subject, casein and legumin may be considered as the same. Their nutritive values are equal and exceptionally high, supposing they can be digested and assimilated. One is the most difficult of digestion of all the nutritive constituents of vegetable food, and the other enjoys the same distinction among those of animal food. Both primarily exist in a soluble form, both are rendered solid and insoluble in water by the action of acids; *both are precipitated as a curd by rennet*, and both are rendered soluble after precipitation or are retained in their original soluble form by the action of alkalis. They nearly resemble *in flavour*, and John Chinaman makes actual cheese from peas and beans.

These facts, coupled with what I have already said concerning cheese and its cookery, will doubtless lead my readers to expect something concerning peas-pudding and potash in my next.

* Shortly after the close of the great Exhibition of 1851, when the South Kensington Museum was only in embryo, I had occasion to call at the "boilers," and there found the Prince hard at work giving instructions for the arrangement and labelling of these analysed food products and the similarly displayed materials of industry, such as whalebone, ivory, &c. I then, by inquiry, learned how much time and labour he was devoting, not only to the general business of the collection, but also to its minor details.

LIFE IN MARS.

By R. A. PROCTOR.

(*Continued from p. 304.*)

BUT we are naturally led to inquire whether the phenomena which our meteorologists have to deal with—clouds, fog, and mist, wind-storms and rain-storms—can be recognised, either directly or in their effects, when Mars is studied with the telescope. The answer is full of interest. We have been able to learn much respecting the meteorology of this distant world. In the first place, we see that at times the features of his globe—those well-recognised markings which indicate the figure of oceans and continents—are hidden from view as if by clouds. A whitish light replaces the well-marked red colour of the continents or the equally well-marked green-blue tint of the oceans. But more. We can at times actually watch the gradual clearing up of the Martial skies, for we can see the whitish region of light gradually growing smaller and smaller, the features it had concealed coming gradually into view. On one occasion Mr. Lockyer was observing Mars with an excellent telescope, when he became aware that a change of this sort was in progress. A certain well-known sea was partially concealed from view by a great cloud-mass spreading over many thousand square miles of the Martial surface. But as the hours passed, the clouds seemed to be melting away, whether by the sun's heat or because they had fallen in rain was, of course, not determinable. When Mr. Lockyer ceased observing for the evening—at about half-past eleven—a large proportion of the sea before concealed had come into view. But on the same night, the eagle-eyed Dawes, the prince of modern telescopists, as he has been called, was also studying the Planet of War. Waiting until the outlines of the oceans and continents had become clearly discernible, he made ("in the wee sma' hours ayont the twal") an excellent drawing of Mars. When this was compared with the drawing made at an earlier hour by Mr. Lockyer, it was seen that the clouds which had concealed a portion of the planet had, at the later hour, passed completely away, insomuch that the whole of the shore-

line, which was at first concealed, had been restored to view. And it is worthy of notice, that, referring these events to Martial time, the cloudy weather in this part of Mars appears to have occurred in the forenoon, the midday hour (as often happens on earth) bringing clear weather, which would seem to have lasted until the Martial afternoon was far advanced.

But we can also learn something of the general progress of the weather during a Martial day. It would seem that, as a rule, the Martial mornings and evenings are misty. This, at least, seems the most satisfactory explanation of the whitish light which is usually seen all round the planet's disc; for the parts of the planet which lie near the edge of the disc are those where the sun is low—that is, where it is either morning or evening out yonder on Mars. The presence, therefore, of this whitish light would seem to indicate misty mornings and misty evenings in Mars.

It seems clear too that—as with ourselves—winter is more cloudy than summer; for it is always noticed that near the Martial solstices the markings on that half of the planet where winter is in progress are very indistinctly seen, a whitish light sometimes replacing the red and green markings altogether in these regions. On the contrary, the regions where summer is in progress are generally very well seen.

The reader will infer from what has been said on these points, that the study of Mars cannot be carried on very rapidly by our astronomers; for, in the first place, Mars only returns to our midnight skies at intervals of more than two years, and he remains but for a short time favourably placed for observation. Then one half of his surface only can be seen at a time, and nearly one-half even of that hemisphere is commonly concealed by clouds, which also extend all round the disc, so that perhaps, but about one-eighth of the planet's surface can be favourably studied. When we add to these considerations the circumstance that not one night out of ten in our climate—or, perhaps, in any —is well suited for the use of powerful telescopes, while even favourable nights cannot always be devoted to the study of Mars (other celestial objects often requiring special attention), it will be understood that the progress of discovery has not been so rapid as, at a first view, might be expected. When we are told that more than two centuries have elapsed since the telescopic study of Mars began, it seems as though ample time had been given for research; but the time which has been actually available for that purpose has been far more limited than that estimate would imply.

And now, returning to the consideration of the probable condition of Mars, with respect to those circumstances which we regard as associated with the requirements of living creatures, let us briefly inquire how far we can determine aught as to the geological structure of the planet. Here the spectroscope cannot help us. The telescope, and such reasoning as may fairly be applied to the relations already dealt with, must here be our main resource. We see, then, that the land regions of the planet present a ruddy tinge. Sir John Herschel has suggested, and I am not here concerned to deny, that this is probably due to the ochreish nature of the soil. The planet, in fact, is to be regarded as perhaps passing through a geological era resembling that through which our own earth was passing when the Old Red Sandstone constituted the main proportion of her continents. But it certainly must be admitted, as a remarkable circumstance, that we can trace no signs of extensive forests in Mars, nor any such appearances as we should imagine that our prairies must present to telescopists in Venus or Mercury. One is almost invited to adopt the bizarre notion of that French astronomer who suggested that vegetation on Mars is red instead of verdant —that in this distant and miniature world the poet may sing of spring, more truly than our terrestrial poets, that

She cometh blushing like a maid.

As respects the absence of forests, we may perhaps find a sufficient explanation in the fact that lofty trees would exist under somewhat unfavourable conditions in Mars; for gravity being so much less than on our own earth, the stability of objects having equal dimensions would be correspondingly reduced. On the other hand, the winds which blow in Mars are probably, as Professor Phillips has pointed out, exceedingly violent; so that to quote a striking paper which appeared long since in the *Spectator* (in a review of my "Other Worlds"), "if currents of air in Mars are of more than usual violence, while the solidifying force of friction which resists them is much smaller than here, it may be a reasonable inference that "natural selection" has already weeded out the loftier growing trees, which would stand less chance in encounters with hurricanes than our own." The absence of prairies is not so easily explained, however; and the idea is, in fact, suggested that some of those regions which have hitherto been included among the Martial seas, are in reality regions richly covered with verdure. Nor are we wholly without evidence in favour of this view; for there is a certain very wide tract in Mars respecting which Mr. Dawes remarked to me that he found himself greatly perplexed. "At times," he said, "I seem to see clear traces of seas there; but at other times I find no such traces." These regions have accordingly been regarded as extensive tracts of marsh land. But the idea seems at least worth considering that they may be forest regions or extensive prairies.

There must needs be rivers in Mars, since the clouds, which often cover whole continents, must pour down enormous quantities of rain, and this rain-fall must find a course for itself along the Martial valleys to the sea. Indeed, we can have no doubt that Mars has been the scene of volcanic disturbances like those to which our own mountains, hills, valleys, and ravines owe their origin. The very existence of continents and oceans implies an unevenness of surface which can only be explained as the effect of subterranean forces. Volcanoes must exist, then, in Mars; nor can his inhabitants be wholly safe from such earthquake throes as we experience. It may be questioned, indeed, whether subterranean forces in Mars are not relatively far more intense than in our own earth,—the materials of which the planet is formed being not only somewhat less massive in themselves, but also held down by a gravity much less effective.

It would seem, also, that the Martial oceans must be traversed by currents somewhat resembling those which traverse our own oceans. There is, indeed, a very marked difference between our seas and those of Mars. For apart from the circumstance that the terrestrial oceans cover a much greater proportion of the earth's surface, the Martial seas are scarcely traversed by appreciable tides. Mars has indeed two moons, but neither can appreciably sway his ocean waters, and though the sun has power over his seas to some slight extent, yet the tidal waves thus raised would be very unimportant, even though the seas of Mars were extensive enough for the generation of true tidal oscillations. For, in the first place, Mars is much farther from the sun, and the sun's action is correspondingly reduced—it is reduced, in fact, on this account alone more than threefold. But further, Mars is much smaller than the earth, and the dimensions of our earth have much to do with the matter of the sun's tide-raising power. Every one knows how the explanation

of the tides runs in our books of astronomy and geography. The sun is nearer to the water turned directly towards him than he is to the centre of the earth; he therefore draws that water away from the earth, or in other words raises a wave; but again, says the explanation, the sun is nearer to the earth's centre than to the water on the side turned away from him, and therefore he draws the earth away from that water, or a wave is raised on the further as well as on the nearer side of the earth. If the earth were smaller, the sun would not be so much nearer to the water turned towards him, nor so much farther from the water turned away from him—so that both waves would be reduced in dimensions. Applying this consideration to the case of Mars, whose orb is much smaller than the earth's, we see that any tidal wave raised by the sun in Martial seas must needs be of very small dimensions.

But the existence of ocean currents appears to depend very little on the presence of tidal waves. In the Mediterranean Sea, the Red Sea, and the Baltic Sea well-marked currents exist, although the tidal wave scarcely affects these seas. Sea-currents would indeed seem to be due to the effects of evaporation taking place extensively over certain portions of the sea surface; and we know that evaporation must proceed very freely in the case of the seas of Mars, since clouds form so marked a feature of his atmospheric economy. We may conclude, therefore, that his seas are traversed by currents, and further that most of those effects which our students of physical geography ascribe to ocean currents, take place also in the case of Mars.

Summing up the results here considered, we seem to recognise abundant reasons for regarding the ruddy planet which is now shining so conspicuously in our skies as a fit abode for living creatures. It would seem, indeed, unreasonable to doubt that that globe is habitable which presents so many analogies to our own, and which differs from our own in no circumstances that can be regarded as essential to the wants of living creatures.

THE MORALITY OF HAPPINESS.

By Thomas Foster.

CARE OF OTHERS.

(*Continued from p. 284.*)

BUT we recognise the necessity of a more thorough altruism than that which merely considers the rights of others. That a community should progress as it ought, each member of the body social should feel that it is a part of his personal duty to consider the wellbeing of the rest. The weakness and the want of skill, the ill-health and the imperfect education of his fellows are injurious to him and to all. In such degree as weakness or want of skill affect the productive power of some members of the community, the comfort and happiness of the stronger and more skilful are affected. The weak and inefficient members, who cannot provide for themselves, must be provided for somehow. The trouble to the community which would arise from any plan for leaving the weak and unskilful unprovided for, would be much more serious than the loss arising from the efforts made to help them. But these efforts being so much deducted from the general efforts of the stronger and more skilful members of the body social must be counted as loss. So that it is the interest of all to see that there may be as few weak and unskilful persons in the community as possible.

In like manner the sickness of our fellows is a matter in which we are interested. Apart from the necessity of restoring the sick to such health and strength as may fit them to take their part in the work of the community, the illness of others may bring illness to ourselves. Fever and pestilence, though they may first attack the weak, presently extend their attacks to those who had been strong. If even a man should feel no anxiety on his own account, those dear to him, those dependent on him, or those on whom perhaps he is in greater or less degree dependent, may succumb to such attacks. Considering all the evils, near and remote, which may follow from an epidemic, we recognise the necessity of adopting all such altruistic measures as may avail to diminish the chance of such diseases arising, or to limit their range of action when they have once found footing. No doubt egoistic considerations here seem to suggest altruistic duties; but these altruistic duties cannot be properly undertaken or discharged unless they have become habitual and are referred to a real care and regard for others independently of consequences, more or less remote, to self. Apart from which, the discharge of such altruistic duties will be more satisfying and more pleasant if they are spontaneously undertaken.

Similar considerations apply to education in all its various forms. In other words, we must consider the mental as well as bodily weaknesses, and the mental as well as bodily diseases, of our fellow-citizens. Where those around us are stupid and unintelligent, where they attempt no improvements, where they have little inventive capacity and little readiness to use even such as they have, we suffer along with them. The mere stupidity of the great mass of most communities with regard to the system of Government they consent to be ruled by, may mean most serious injury and discomfort to all, foolish and intelligent alike. Those who see what is needed, or at least the direction in which improvement may reasonably be sought, yet remain silent in the belief that it is no business of theirs, are as unintelligent as those who stupidly assent to what—without thinking—they suppose to be good for them and to be provided for by those who know better than themselves,—though often, when traced to their source, the measures in vogue are found to be of no better origin than the body itself which submits to them.

A low standard of intelligence in the community affects the welfare of all, in many different ways. Wrong ideas about the relation of the nation to other nations may seem unimportant in the case of persons who take no direct part in political matters. But in reality a very notable influence is exerted by the community generally on the conduct of those who have charge of political affairs. Wrong counsels in the cabinet may be advanced or right counsels hampered by stupidity in the country at large. Statesmen themselves are not always so wise or often so firm that they are not influenced by prevalent ideas; and so far as mere numbers are concerned prevalent ideas are likely to be foolish ideas. Fortunately mere numbers may not suffice to give weight to prevalent stupidity. Many of the unwise are influenced by the observed fact that such and such men conduct affairs successfully, and so are led to support the wiser sort, not through sound judgment on their own part, but from that kind of sense which leads the ignorant to defer to the judgment of the better-informed. But this does not prevent the average intelligence of the community from being a matter of great moment even in political matters,—supposed to be guided always by the wisest, despite the true saying that the world is governed with but a small amount of wisdom. What I have here said has no relation to the action of kings, princes, and the like, who in English speaking communities cannot now injuriously influence political relations except through the weakness or

folly of statesmen. Yet the argument might be strengthened by calling attention to the way in which, even within the last thirty years, our own country has suffered in this special direction, statesmen weakly or foolishly yielding to public pressure by which the unwise counsels of princes have been supported. A hundred years ago our country saw in still more marked way how the average want of intelligence of the many, supporting the stupidity of a king (of alien race, in that case), may go near to wreck the fortunes of a great race. We may hope however that no such trouble is in store for us hereafter as afflicted the British people when a foolish people insanely strengthened the hands of a mad king.

In social matters a low standard of general intelligence is a serious evil, which a wise altruism will endeavour to diminish. "*I do not mean,*" I may here say with Mr. Herbert Spencer, "*such altruism as taxes ratepayers that children's minds may be filled with dates and names and gossip about kings and narratives of battles and other useless information, no amount of which will make them capable workers or good citizens; but I mean such altruism as helps to spread a knowledge of the nature of things, and to cultivate the power of applying that knowledge.*"

It is hardly necessary to multiply examples. We are confronted at every step by the harmful effects of prevalent want of intelligence. The fire which is intended to warm your room is so stupidly placed that it sends the better part of the heat up the chimney and creates cold draughts round your legs. Equally obnoxious to the understanding is the window by which you seek to ventilate your room. It is a struggle to open it, a struggle to close it, unless when your head is in the way, when it generally descends in effective guillotine fashion. The carpeting of your room is an absurdity, the papering (apart from any question of beauty) a monstrosity. The gaseliers are so ingeniously arranged that you get a minimum of light, and a maximum of heat and foul air. The chair you sit on seems intended to make you uncomfortable; as you draw it up to the table you find that the senseless people who plan furniture have provided sharp corners just where your knees are most likely to be caught. If you wish to lie down or to recline on a sofa, you find the head of the sofa so ingeniously padded that while too sloped for reclining, it is not sloped enough for you to lie on it comfortably.* Your child running in for a kiss from papa, stumbles over a footstool so carefully coloured like the carpet that it did not catch his eyes but his feet; and falling, is hurt severely by a sharp projection on chair, sofa, table-leg, fender, scuttle, or what not, where no sharp projections are wanted, and none ever should be. In numberless ways miseries, individually small, but effectively diminishing happiness, result from general want of intelligence. "Unpunctuality and want of system" again, as Mr. Herbert Spencer points out, "are perpetual sources of annoyance. The unskilfulness of the cook causes frequent vexation and occasional indigestion. Lack of forethought in a housemaid leads to a fall over a bucket in a dark passage; and inattention to a message, or forgetfulness in delivering it, entails failure in an important engagement."

It is thus the interest of each one of us, and being also for the good of all becomes the duty of each, to be altruistic in regard to the mental progress of the community,—"we benefit egoistically by such altruism as aids in raising the average intelligence."

But we are equally interested in the improvement of the moral feeling pervading the social body. The happiness of the whole community is diminished by the prevalence of unconscientious ways. In small matters as in large the principle prevails. We are all interested in helping to teach men the duty of considering the rights and claims of others. From the man who hustles others off the pavement or occupies an unfair share of what should be general conversation, to the man who swindles by gross aggressions or serious breach of contract, the products of a state of low average morality diminish the happiness of the community. The aggregate of discomfort wrought by paltry offences is serious though each separate offence may produce but slight mischief. Moreover offences paltry in themselves may produce very serious results. The disobedience of a nurse in some small matter (such as taking her charge to this or that place), may lead to accident affecting life or limb, or to disease ending in permanent injury or in death. In other ways, mischievous results of greater or less importance are brought about by defective moral sense in small matters, while when we consider the effects of want of conscientiousness in business we recognise still more clearly how much we are all concerned in the moral improvement of the community. "Yesterday," says Mr. Herbert Spencer, "the illness of a child due to foul gases led to the discovery of a drain that had become choked because it was ill-made by a dishonest builder under supervision of a careless or bribed surveyor. To-day workmen employed to rectify it occasion cost and inconvenience by dawdling, and their low standard of work, determined by the unionist principle that the better workers must not discredit the worse by exceeding them in efficiency, he may trace to the immoral belief" (well put!) "that the unworthy should fare as well as the worthy. To-morrow it turns out that business for the plumber has been provided by damage which the bricklayers have done." And so daily and hourly do we feel that the moral imperfections of the community are fit subjects for such altruistic efforts as may help to raise the average morality.

(*To be continued.*)

CREMATION.*

By Richard A. Proctor.

THE debate on cremation in the House of Commons afforded a singular illustration of the operation of our constitutional system on matters connected with the social requirements of the people. Mr. Herbert Spencer, in his fine article on the "Coming Slavery," points out the danger of our handing ourselves over as slaves to the body which we elect theoretically to be the servant of the body social. He shows how we have given, and propose to give, to Government, through our great Talking House, the power of regulating many social matters which should be regarded as in no way falling within its scope. We have made Government our deliverer of letters and messages, our chief carrier, the manager of our system of education, the supervisor of medical matters (even within our own households, as in the case of vaccination), and the controller of certain matters affecting morality. Every year bills for

* I fear Mr. Foster refers to that abomination of desolation, the Alexandra Sofa, which certainly for hideousness and utter unfitness for all the uses of a sofa is a marvel of idiotic absurdity. Nine-tenths of our sofa and armchair patterns, however, are "too absurd for any use" as they say in America. Among my own pet abominations I may mention nearly all the methods (save the mark) for curtaining windows, the ridiculous ways in which looking-glasses are swung, the preposterously unscientific forms of ink-stands, and some others *quæ nunc perscribere longum*.—R. P.]

* From the *Newcastle Weekly Chronicle.*

extending Parliamentary influence in this mistaken direction are introduced, and we imagine we are advancing where we are in reality retrograding, that we are securing fuller freedom when in reality binding ourselves under a growing slavery, and attending to the business of our country when we are really handing over our own proper business to those who cannot be nearly as much interested in seeing to its being properly done as we are ourselves.

Cremation naturally has taken its turn. It is permitted now, and every one who desires either to be cremated himself in due course, or to cremate (also in due course) the bodies of departed relatives, can make an arrangement for having his wishes fully carried out. But, of course, the advocates of cremation cannot be quite happy until their system of burial is put under Government control; so they try to get a bill passed for the regulation of cremation. So much for *their* mistake. It is but an example of the class of mistakes with which our Parliamentary system makes us yearly more familiar. The way in which their proposal is met and, as it happens, defeated, illustrates a more serious defect still in the prevalent mode of thinking about such matters. Officialism, by its representatives both in office and out of office, opposes not the regulation of cremation, but cremation itself, on grounds utterly stupid and blunder-headed, after the customary manner of officialism. The one element of sense in their opposition resides in the undoubted objection that a certain very small proportion of cases of a form of crime by which a still smaller proportion of deaths are occasioned, would possibly remain undetected and unpunished if cremation were practised. Possibly one person in 100,000 of those who die annually dies by poison. It may be that in one poisoning case out of a hundred suspicion is not aroused till after burial. In these few cases among deaths, themselves exceptional in character, detection would be prevented were cremation universally adopted as the mode of disposing of the dead; but only a very small proportion even of these would be affected so long as cremation remains, which it will always do, the exception rather than the rule. But to hear our officials in Parliament on this subject, one would imagine that at least a fourth of our deaths were due to poison, that in every case suspicion would only be aroused some time after death, and that every dead body would be disposed of by cremation if once they passed a bill for regulating what is already permitted. The other chiefly sentimental and silly objections might have been urged with greater force on the other side, seeing that they are considerations which will always render cremation unpopular, and therefore prevent its being so commonly resorted to as its enemies fear—or say they fear—that it may be.

Then besides the official and therefore (almost of necessity) the stupid objections to cremation, came the imbecile objection that cremation has been advocated on scientific grounds. Said one representative noodle—in my ignorance of party relations in our talking houses, I am unable to say whether he belonged to one or the other of the two chief parties which undertake to get us along by hauling in opposite directions—this system is a crude attempt to foist on us the fancies of men of science. Consider the inane idiocy of such a remark. What is science but the search after knowledge? What is a man of science?—if any claim this name, (for my own part I think no man of sense would ever say more for himself than that he is a student of science)—What is such a man but one who strives to learn what are the laws of nature, and what therefore is best for those whose lives are enforcedly subject to those laws? In the search after knowledge, men have found that every dead body, if left to itself on or in the ground, undergoes processes not merely analogous to those which take place if the body is burned, but actually identical with them; that even if dead bodies are absurdly dealt with so as to delay these processes, the end is the same. The process may last twenty years in a coffin, instead of the three years or so in which mother earth does the work. Or, in a stately mausoleum, the process may last two or three centuries. Or it may be lengthened to thousands of years by elaborate processes of embalming. But always there is the same final result. The study of natural laws has further disclosed the fact that in any populous community mischief of a very serious nature may arise from consigning multitudes of dead bodies to the ground in the midst of human habitations and in the neighbourhood of places (or in the very places themselves) whence the water supply of the community is obtained. That cremation is a method of disposing of the dead which would be free from such objections, has been further shown by scientific researches. Yet these facts, brought before Parliament in connection with a suggestion that this innocuous system already permitted, should be regulated by the State, is described (by one of those who get foisted upon the country as legislators for no merit but the inheritance of certain property) as a crude attempt to foist scientific fancies on the House.

As regards the future wide extension of the system of cremation, there is probably little chance of it. Too many retain the old superstition, natural enough in past times, but a sheer absurdity now, that the collection of various forms of matter which chances to be the "body" of a relative or friend is in some way distinct (beyond the influence of association) from any other equivalent collection of the same forms of matter. It is very natural and proper that after death the body should be associated in our minds with the personality of its former tenant, just as the former home of a Shakespeare or a Newton is associated with the great man who lived in it. If, without harm to the living, the body of a great man could be kept among us unchanged and unchanging, one might see reasons for so keeping it, akin to those which lead men to make statues or portraits of the dead. But when the body *has* to be disposed of, it is as absurd to consider too fancifully the method of disposal, and so be led to reject the best and safest way, as it would be if, were we forced (in some way) to dispose of Shakespeare's former house, we were to insist that the stones and timberwork should only be used for libraries and museums, and not for the homes of men and women.

For my own part, I share with Mr. Labouchere a sense of utter indifference as to what happens—I was going to say with this body of mine, but I rather hope that it will be another body, associated with me as my temporary home, which will be in question when I cease to live—(in other words I hope I may live long enough to have entirely got rid of the material particles which form my present body)—so I will say—with *that* particular collection of sundry elements which may then be my body. I am not quite so well able to rid myself of the influence of association in regard to the body of a dead friend or relative. I can understand the feeling, absurd though it is in itself, that the quick burning of the body is a less loving way of disposing of it than the form of burial which results in a very slow dissolution of whose actual nature we dare not allow ourselves to think. But that there is this natural and unreasonable feeling should be a reason rather for encouraging than opposing cremation, seeing that we have in it the assurance that the system will never be over hastily adopted.

MOVED FROM AFAR.*

I. THE EMOTIONS.

WE will select a few accounts from witnesses not likely to be accused of sentimental exaggeration :—We begin with two closely allied narratives from gentlemen of acknowledged scientific position. And we may remark in passing that men of science—who are not, of course, a large class—contribute, we think, quite their proportional quota to our collection of evidence throughout. The following case was sent to Professor Sidgwick by the Rev. J. M. Wilson, head-master of Clifton College, a senior wrangler and well-known mathematician† :—

"Clifton College, Jan. 5, 1884.

"The facts were these, as clearly as I can remember.

"I was at Cambridge at the end of my second term, in full health, boating, football-playing, and the like, and by no means subject to hallucinations or morbid fancies. One evening I felt extremely ill, trembling, with no apparent cause whatever ; nor did it seem to me at the time to be a physical illness, a chill of any kind. I was frightened. I was totally unable to overcome it. I remember a sort of struggle with myself, resolving that I *would* go on with my mathematics, but it was in vain: I became convinced that I was dying.

"I went down to the rooms of a friend, who was on the same staircase, and I remember that he exclaimed at me before I spoke. He put away his books; pulled out a whisky bottle and a backgammon beard, but I could not face it. We sat over the fire for a bit, and then he fetched some one else to have a look at me. I was in a strange discomfort, but with no symptoms I can recall, except mental discomfort, and the conviction that I should die that night.

"Towards eleven, after some three hours of this, I got better, and went upstairs and got to bed, and after a time to sleep, and next morning was quite well.

"In the afternoon came a letter to say that my twin brother‡ had died the evening before in Lincolnshire. I am quite clear of the fact that I never once thought of him, nor was his presence with me even dimly imagined. He had been long ill of consumption, but I had not heard of him for some days, and there was nothing to make me think that his death was near. It took me altogether by surprise.

"JAMES M. WILSON."

Our next case is also from a scientific witness, who can hardly have been tempted to exaggerate, since the experience which he thus records greatly impairs the force of the main thesis of his book, which is directed *against* the transmission of obscure influences (mesmeric and the like) from one person to another.

[*Translation of pp.* 71-73 *of* "*Der sogenannte Lebens-Magnetismus oder Hypnotismus,*" *by Dr. E. L. Fischer, of Würzburg* (1883).]

"When I was a student at the University I experienced, on waking one morning, a quite extraordinary feeling of sadness. I was not in the slightest degree unwell, and was aware of no reason for distress, and my state of depression consequently made a great impression on me—the more so that I normally enjoy the best spirits. I asked myself what could be the meaning of it, and whether some serious illness must not be impending. I made every effort to banish this deep melancholy, and especially to assume a gay demeanour in the presence of my friends; but all my efforts were unavailing. Before lecture two of them asked me what was the matter; they said I must have something heavy on my heart. During the whole forenoon I remained in this state of dismal wretchedness. All at once a telegram arrived from home, informing me that my grandmother was taken very ill, and that she was earnestly longing for me. There I had the solution of the riddle. Nevertheless from that hour my melancholy gradually decreased, and in spite of the telegram it completely disappeared in the course of the afternoon. In the evening I received a second message to the effect that the danger was over. In this way the second phenomenon, the rapid decrease of my wretchedness—a circumstance which in itself was surprising, inasmuch as the melancholy should naturally rather have *increased* after the receipt of the first news—received its explanation. For the afternoon was just the time when the change in the patient's condition for the better took place; and the danger to her life once over, her yearning for my presence had decreased; while simultaneously my own anxiety was dispelled."

We have space for but one more instance, which is any rate, sufficiently terse and business-like :—

"20, Rankeillor-street, Edinburgh, Dec. 27, 1883.

"In January, 1871, I was living in the West Indies. On the 7th of that month I got up with a strange feeling that there was something happening at my old home in Scotland. At 7 a.m. I mentioned to my sister-in-law my strange dread, and said that even at that hour what I dreaded was taking place.

"By the next mail I got word that at 11 a.m. on the 7th of January my sister died. The island I lived in was St. Kitts, and the death took place in Edinburgh. Please note the hours and allow for difference in time, and you will notice at least a remarkable coincidence. I may add I never knew of her illness. "A. C——N."

In answer to inquiries Mr. C——n adds :—

"I never at any other times had a feeling in any way resembling the particular time I wrote about. At the time I wrote about I was in perfect health, and in every way in comfortable circumstances."

If further proof be needed that we have not to go to weak or hysterical sources for evidence of these vaguer and more emotional sorts of telepathic impression, we may add that our collection includes under this head accounts from two informants who, in very different ways, have obtained the highest reputation as acute and accurate observers—Mr. Henry James and Mr. J. N. Maskelyne.

(To be continued.)

* From a most interesting and suggestive paper on Apparitions, in the *Nineteenth Century*, by Messrs. Edm. Gurney and Fred. W. H. Myers.

† Most of the informants quoted in these articles are privately known to us. But since it happens that many of them bear well-known names, we have thought it better to omit in all cases the statement of our acquaintanceship, rather than to assert it in cases where our personal attestation of confidence would have looked highly superfluous. We may add that the narratives here given are, of course, mere samples from a very large collection, which we hope soon to lay before the public in its entirety.

‡ One or two of the cases quoted by Mr. Galton of consentaneous action or thought on the part of twins seems to us to be probably referable to telepathic impressions.

THE following copy of the passage referred to in Col. Herschel's letter (KNOWLEDGE, No. 131, p. 314) has been kindly forwarded by Mr. R. Barrington :—"I applied the same test to the comet of 1862. There are various modes of making the trial. Mine was by looking at the comet through an achromatised doubly-refracting prism, and turning the prism round in its own plane. I could perceive *no* alternate maxima and minima of brightness in the images. But in this case it is the positive result which is conclusive. Everything depends, in the first instance, on the relative situations of the objects and the eye. And, moreover, the light of the comet of 1862 was far inferior to that of Donati's, rendering the experiment *pro tanto* more delicate, and it is very possible that to septuagenarian eyes, indications of *partial polarisation* might escape observation."

STANDARD TIME.*

By J. K. REES, A.M.

ON November 18, 1883, there was accomplished virtually a reform in the Time Standards of the United States and Canada for which scientific societies and men had long been working. On account of the labour done by the American Meteorological Society, followed by the Society of Civil Engineers and the American Association for the Advancement of Science, and especially because of the prompt and very energetic work of Mr. W. F. Allen,† this reform was quietly begun. There has been but little opposition to the new standards, as their scope has been studied. The newspapers have given such full accounts of the movement that it will be necessary here to make only a short statement. In France the railroad systems are run by Paris time. The greatest difference between the true local time and the railroad time is over forty minutes. In England, Scotland, and Wales but one time is used, and that Greenwich time, while the extreme points, Yarmouth and Land's End, are thirty-two minutes apart. The adoption of one time standard for a large section of the country, and the consequent abolition of the numerous local time standards, is attended with great and obvious benefits.

So far as I know, the first person to publish any system for the United States was Professor Dowd, of Saratoga Springs, who, in 1869, published a pamphlet explaining his "hour system" for the railroads. Professor Dowd proposed to take belts fifteen degrees wide, running north and south through the country, and to have all the railroads in a belt keep the same time. These belts being fifteen degrees of longitude in width, the time standards would thus differ exactly one hour. He first started with the meridian through Washington, D. C., as his initial central meridian, but afterwards changed his system so as to connect with the meridian of Greenwich, by adopting as his initial central meridian the 75th meridian west of Greenwich.

Prof. Dowd met many of the railroad men, and, although influencing a few of the prominent railroad managers, he was not able to bring about any practical result, except, perhaps, to educate a considerable number of railroad men in the advantages of his system. In 1874 the Standard Time Committee of the American Metrological Society took up the subject, and, without being aware of Prof. Dowd's work, elaborated a system of "hour standards," basing their system on Greenwich, and dividing the United States and Canada into north and south belts. This society printed considerable matter on the subject, and through the energetic work of President Barnard, Cleveland Abbe, and E. B. Elliott, succeeded in stirring up a great deal of interest. It was through the influence of this committee that Mr. Allen became a member of the American Metrological Society. Subsequent to the beginning of the work in this society the subject was brought to the attention of the Society of Civil Engineers and also the American Association for the Advancement of Science. Many of the directors of observatories in the United States became thoroughly interested in the proposed reform, and aided materially in educating the people up to a comprehension of the scheme and its advantages.

But while the "hour system" scheme was endorsed by the majority of scientific men, yet there were those who favoured a single standard for the whole country. This standard meridian some proposed to pass through Washington, while others suggested the ninetieth meridian from Greenwich which passes about 50 seconds to the east of St. Louis. The scientists fully appreciated that the key to the solution of the problem lay in the hands of the railroads. If the railroads should adopt any system, the people would very soon follow. Therefore many attempts were made to interest the railroads, without any practical result, until the American Metrological Society interested Mr. Allen in the matter, to whom the railroads had referred the question. Mr. Allen very quickly decided that it would be of no use to press upon the railroads any single-standard system, as the country was too large; and he also decided to recommend any system that should turn out to be practically the best for the railroads without considering the scientific side of the question. In studying the railroads he soon saw that the hour system was the best, and that the roads were arranged in four belts. Roads east of Buffalo, Pittsburgh, Wheeling, &c., ran on various times, of which Philadelphia time was nearly the average. Roads in the belt between Pittsburgh and the western boundary of the State of Kansas had standards of which St. Louis time was nearly the average. Roads between Kansas City and Salt Lake adopted Denver time, and still farther west San Francisco time was used. Noticing how close to the standards suggested by the American Metrological Society his practical standards ran, Mr. Allen proposed to the railroads to adopt that plan. Making a careful study of the whole country, he selected the points at which it was feasible to change from one hour standard to another. After months of writing and incessant labour, the majority of the railroads were persuaded to join in the movement. After the railroads were secured, it became desirable to have the principal cities in the country adopt the new standard time. In our own city (New York) the writer was fortunate enough to meet with General Cochrane, of the Law Committee of the Board of Aldermen. To this gentleman the proposed change was explained, and in a subsequent meeting at the City Hall, Gen. Cochrane, with Mr. Allen and the writer, drew up a preamble and resolution embodying the change. This was passed without trouble, owing to the favourable view taken of the proposition by Gen. Cochrane, and Mayor Edson. In Boston, the action of Prof. Pickering, of the Harvard Observatory, and his assistant, Mr. J. R. Edmands, overcame all opposition, and a change of sixteen minutes was made in their local time. Since the action of these two cities, many of the large cities in the country have adopted the standards of the belts wherein they are situated.

Even after the system shall have been universally adopted in this country, the complete plan of the foremost advocates of the changes in time standards will not be carried out, for they propose encircling the globe with standard meridians fifteen degrees apart. All places seven and one-half degrees east and west of a standard meridian would approximately, governed by limitations of nationality, adopt the time of the central meridian, so that, as one should travel around the earth, he would find the local times differing exactly one hour.

Moreover, it is proposed to have what is called a Cosmic Time. The Cosmic day is to begin when the mean sun is over the meridian 180° from Greenwich. It is further proposed to do away with the A.M. and P.M. designations, and to number the hours from one to twenty-four.

Through the work of the American Metrological Society and the Society of Civil Engineers, the knowledge of this complete plan for the world has been spread abroad. The general plan has had the endorsement of the Geographical

* From the *School of Mines Quarterly*.

† Editor of the Traveller's Official Guide and Secretary to the R. R. Time Conventions.

Convention which met at Venice, the Society for the Reform and Codification of the Laws of Nations at Cologne in 1881, the Imperial Society at St. Petersburgh, the Geodetic Convention at Rome, the International Bureau of Weights and Measures at Paris, the Canadian Institute, &c., besides most of the scientific societies in this country. In order to further discuss this subject in its world-wide application, the President of the United States, acting at the request of a large body of scientific men, has called an international convention to meet at Washington in October.* Judging from the interest shown in the question by all the leading nations of the earth, we may confidently expect to see the world-wide adoption of the hour-standard system in the near future.

Reviews.

SOME BOOKS ON OUR TABLE.

The New Principia. By NEWTON CROSLAND. (London: Trübner & Co. 1884.)—"Whan," said the Scotchman, in the venerable anecdote, "ae mon talks aboot what he disna understan', till anither mon, wha disna understan' hum, *that's* Maytapheesics." *Mutatis mutandis*, this definition might well be applied to the impudent book whose title heads this notice. We use the word "impudent" advisedly, and precisely in the sense in which we should employ it in speaking of anyone who, knowing literally nothing of the distinction between nerve, muscle, and cartilage, should find fault with a surgical operation performed by Lister, Paget, or Skey; or of a man who had never seen a ship in his life, and did not know the port from the starboard side, who should criticise a naval manœuvre of Lord Alcester's. For, sooth to say, the ignorance of Mr. Crosland of the very rudiments of the subjects on which he presumes to dogmatise would be ludicrous—if it were not pitiable. Take, for an example, his wild ideas on the mode of action of a galvanic battery (p. 12); "magnetic action" *simulating* incandescence (p. 14); the consumption and disappearance of metal under this same "action" (p. 15); the conversion of electricity into light and heat by attraction! (p. 17); the *increase* of light and heat with the increase of distance of a planet from the Sun (p. 17); 'electric action" shining "most brilliantly in a vacuum" (*id*); the production of light by the friction of the "magnetic current" emanating from the sun, on our atmosphere (p. 22); as though this "current" flowed like water out of a street plug! and so on. It may suffice to show the pains that our author has taken to acquire his facts from the latest and best sources if we say that his "authorities" for astronomical data are (pp. 38, 39, and 41) "Ferguson's Lectures" (originally published in 1756); Joyce's "Scientific Dialogues," 1825 (a curious mixture of facts and nonsense); Milner's "Gallery of Nature" (a publisher's compilation some forty years old); Lardner's "Astronomy" (published 1855)—Lardner, as is well known, having scarcely ever written a thousand original lines in his life; and Bonnycastle's "Astronomy," a little book compiled by a schoolmaster in 1823. Of any more recent or trustworthy astronomical literature, Mr. Crosland appears to be in the most sublime ignorance. Otherwise he would scarcely have spoken (on p. 8) of the Sun being "a dark globe"; of "centrifugal *force*"; of a "burst planet" (pp. 59 and 60); of the absence of heat on the moon (70), &c., &c. An excellent illustration, too, of his keen appreciation of facts, and of the care he has taken to ascertain them, may be found on p. 43, where he says, "We may be quite sure that a planet which takes thirty years to revolve round the sun moves at too slow a pace to manifest any centrifugal force whatever." Would Mr. Crosland be surprised to hear that Saturn (which, with its period of 29·46 of our years, is obviously the body to which he refers) travels in its orbit with a velocity of 5·95 miles *per second*, or more than twenty-five times as fast as the shot when it is leaving the muzzle of the eighty-ton Armstrong gun? What sort of cord would hold a weight whirled round the hand with the merest fraction of this velocity? But we might fill a page or two of KNOWLEDGE with similar exhibitions of almost childish ignorance; though, in mercy to our readers, we forbear. We differ so utterly from the author, that it is a relief to find a subject on which we can agree. He says on p. 82, "I am quite aware that my style of controversy may be termed conceited, arrogant, contemptuous, and impertinent." So far we are in the most entire accord with him.

A Chapter of Science. By J. STUART, M.A. (London: Society for Promoting Christian Knowledge.)—Opening Mr. Stuart's charming book, after wearily wading through Mr. Crosland's dreary rubbish, is like quenching one's thirst at a crystal spring, after a futile attempt to swallow a few mouthfuls of water from a muddy puddle. It would seem from the preface that Mr. Stuart delivered a course of six lectures, in the year 1868, to the artizans in the London and North-Western Railway Company's works at Crewe, and it is a reprint of them which forms the "Chapter of Science" before us. They appear to us to be the very model of what such lectures should be. Notably is this the case in the exposition of the Laws of Kepler and Newton, which form the subject of Lectures II., III., and IV. For lucidity of explanation we have so far met with nothing to surpass them, and it certainly would seem that our author's original audience must have possessed an amount of intelligence strangely inferior to that of the average British artificer if they failed to apprehend clearly and thoroughly the meaning and nature of the laws which govern not only our own solar system, but the whole visible universe. An excellent popular description of spectrum analysis, too, is given in Lecture V. As a cheap, simple, and absolutely trustworthy introduction to the study of gravitational astronomy, for those ignorant of mathematics, it would be very difficult to find a book to surpass this one of Mr. Stuart's.

The New Atlantis, or Ideals Old and New: A Dialogue, By a DISCIPLE OF BUCKLE. (London: Williams & Norgate. 1884.)—This curious book requires at least a couple of perusals before its full signification can be appreciated. The major part of it takes the form of a series of dialogues between a supposititious "Draper" and an equally imaginary "Lessing." In these conversations the philosophies and theologies both of the ancient and modern world are discussed, and the absolute community of all that is good or worthy in them insisted on. Very curious will it appear to the reader, approaching the consideration of the subject for the first time, to learn, by verbatim extracts from the sacred books of the Chinese, Buddhists, Brahmans, Parsees, Greeks, Romans, and the comparatively more modern Mohammedans, how much that is claimed as essentially distinctive in Christianity is now held, as it has been held in the dimmest ages of the past, by unnumbered millions whom we are but too prone to contemptuously class as "Heathen." The "Disciple of Buckle" looks to America as the home of that greater future civilisation which has yet to come.

* It is to be hoped that this date will be changed to a much earlier one.

Aura Dynamica: Concerning Force, Impulse, and Energy. By JOHN O'TOOLE. (Dublin: Hodges, Figgis, & Co.)—This is an attack on the terminology of modern Kinematics from a metaphysical point of view. So far as we understand Mr. O'Toole, his quarrel is less with the doctrine of energy, as now developed, than with the somewhat slipshod way in which its expositors express themselves; and he certainly has culled a series of examples from admitted authorities, which seem to exhibit them as floundering in a sea of verbiage. Their seeming inconsistencies are doubtless susceptible of explanation, in some cases so readily so as to render Mr. O'Toole amenable to the charge of hypercriticism; but anything which tends to render the use of language more definite and precise, *ipso facto* contributes towards the advance of knowledge. Meanwhile, what is irreverently known as the $(t + t')$ school is quite capable of taking care of itself.

Hints to House-hunters and Householders. By ERNEST TURNER. (London: T. Batsford. 1883.)—A thoroughly practical and very useful book, dealing as it does with the details of construction of every part of a dwelling-house, from the attic to the basement. Nothing is more commonly heard from the lips of house-hunters, after a visit to view some place which they may think of taking, than "Oh! I utterly forgot. . . so and so." With Mr. Turner's handy manual in his possession, no intending occupier can have any excuse for such forgetfulness, inasmuch as everything that can be required (as well as certain things which distinctly are *not* required) in a really healthy, habitable house, will be found set forth in detail. There are but few householders, indeed, who may not learn something from the work before us.

Wholesome Houses. By E. G. BANNER, C.E. (London: Edward Stanford. 1882.)—Mr. Banner is the inventor of a very ingenious and effective method of ventilation, and it is to show its applicability to the rendering houses wholesome that his book is written. Sufferers from sewer-gas and cognate evils of defective drainage may study it with advantage.

Is God Unknown and Unknowable? By the Rev. C. E. BEEBY. (London: Wyman & Sons. 1884.)—This is, in effect, a sermon, neither better nor worse than the average of the 104 preached during the preceding twelve months in the church or chapel which the reader himself attends.

THE installation of the Gülcher Electric Light Company (Limited), at the Crystal Palace, is now working successfully, and effectually lights the central nave. The installation consists of 75 arc lamps of 2,000 candle-power (nominal) each, connected in multiple arc, and fed by the current from three Gülcher dynamos, No. 5 type, running at 500 revolutions per minute. There are four dynamo machines, connected parallel, and so arranged that the difference of potential at the terminals of each machine is kept constant. Provision is also made for the switching-off of any one working machine, the spare machine being substituted without stopping the light. The current generated is led in a pair of main wires to a switch-board, where it is divided into the necessary number of circuits, each circuit at this point being provided with a fusible plug. Arrangements are made at the switch-board for measuring the current without disturbing the light. About 80 horse-power is said to be absorbed. The lamps are suspended by the cable which carries the current, and are provided with counter weights to facilitate the raising and lowering of them. There are 54 lamps in the great nave, 12 in the orchestra, and 8 in the opposite transept. The lamps require 8 ampères each, and a terminal potential of from 65 to 70 volts is maintained at the machines. The lamps burn very steadily, and the illumination of the various objects of interest on view in connection with the International Exhibition now proceeding is effective and complete. We hear, says the *Electrical Review*, that the Crystal Palace Company has it in contemplation to extend the use of the electric light to the whole of the courts and galleries at the Palace, so satisfied are they with their arrangements with the Gülcher Company.

OPTICAL RECREATIONS.

BY A FELLOW OF THE ROYAL ASTRONOMICAL SOCIETY.

(*Continued from p.* 307.)

WE have spoken in the last paper of Galileo's idea of measuring the rate at which light travels, by the extinction of lanterns by a couple of observers a mile or two apart, and of its inevitable and hopeless failure. But the velocity of light has been measured more recently by terrestrial observation, and that with a degree of accuracy little short of marvellous. Fizeau, the French physicist, was the first to devise anything like effective means of doing this. His method may, perhaps, be understood if we say that at a distant station a mirror is placed, to which the light of a lantern is directed from the observer's station. The reflection of this light (which has obviously travelled over double the distance between its source and the mirror) enters the observer's eye through a rectangular slit, corresponding with one in front of the lantern. In front of these slits rotates a wheel with radial teeth, long enough to cover them. Let us suppose that the observer looks between two of the teeth of the wheel at rest. He will see the light of his lantern reflected from the distant mirror. Now let the wheel begin to turn; then, as each tooth passes in front of the lantern, it will cut off its light, so that there will be a succession of flashes between the teeth, which will travel to the mirror and come back again. When we come, by-and-by, to treat of the human eye, we shall find that a luminous impression is retained by it for about the tenth of a second; so that, up to a certain point, the observer will see a continuous image. Evidently he will do so if the light comes back so fast from the mirror that a tooth of the wheel has not had time to get in front of the observer's eye. If, though, we imagine the wheel to turn with such extreme rapidity that the interval between two teeth, through which the light set out on its journey towards the mirror, is replaced by a solid tooth when such light just gets back to it, the observer will simply perceive darkness. The exact rate of rotation of the wheel being measured, and the distance between the light and the mirror known, the velocity of light is obtained by doing a multiplication sum. In practice the wheel rotates with its teeth in the focus of a telescope; the mirror is placed in the focus of a second telescope, so that the rays can be caused to return by the precise route they originally took; and there is an arrangement by which the light passes through the focus of the observer's telescope without reaching his eye, until it has fallen on the distant mirror. The difficulty of determining the exact rate at which the wheel is turning when the light suffers total eclipse (such rate being susceptible of very decided variation without the light reappearing) is a distinct drawback to this form of apparatus. Nevertheless, in a modified form, it has since been employed by Forbes and Young, and by Cornu. Fizeau placed his reflecting mirror 5·383 miles from his source of light and revolving wheel, and found the velocity of light to be 195,114 miles per second. This is now known to be too great. Cornu's subsequent determination with an improved form of Fizeau's apparatus gave 186,663 miles per second. It is, however, to the beautiful apparatus devised by Foucault that we are indebted for our most trustworthy determination of the rate at which light travels. This may be shortly described as a rotating mirror, and a distant fixed concave one, the centre of its concavity falling on the axis of rotation of the moving one. The light falling on the rotating mirror is reflected to the concave one and back again. Here, again, the light comes back accurately by its original route, until the moving

mirror rotates with such velocity that it has shifted its place by the time the light returns to it, when the image of such light will seem to be displaced. Knowing the rate at which the mirror is rotating and the angle subtended by the real and reflected images, a little very easy trigonometry enables us to calculate the velocity of light. Michelsen, of the United States navy, by the aid of this form of apparatus, found that light travels 186,377 miles in a second. Other determinations have been made; but, as a final result, Glazebrook (in his "Physical Optics") adopts as the velocity of white light in vacuo 186,771·4 miles per second.*

But, now, what *is* this invisible agent, traveling in straight lines from the object emitting or reflecting it at this stupendous rate towards the eye? Two answers—and two only—seem possible. The first was that given by Newton, who supposed that actual material elastic particles (or, as he called them, "corpuscles") of inconceivable minuteness were emitted by the luminous body with immeasurable velocity, which corpuscles entered the eye, and, impinging on the retina, produced the sensation of vision. This hypothesis certainly does suffice to explain a very considerable number of observed optical facts; and nothing is more remarkable than the ingenuity with which its author employed it to account for them all. In this he so far succeeded that men like Biot and Sir David Brewster actually held by this theory of emission up to the date of their comparatively recent deaths. We have just said that the emission or corpuscular theory sufficed to explain a very large proportion of observed optical facts; but it failed in others. Very notably did it break down in the explanation of a phenomenon of which we shall subsequently speak—the production of darkness—by two beams of light quenching each other. It was only by a curious straining of probability, too, that Newton accounted for what are now known as "Newton's Rings"—a phenomenon observable at the point of contact between the spherical surface of a lens and a plate of plane glass on which it is pressed, as also in a soap-bubble on the point of bursting. Both Huyghens and Euler conceived that space must be filled by some medium, the production of waves in which produced the sensation of light; but the vast authority of Newton silenced them, and it was reserved for Dr. Thomas Young, at the beginning of the present century, to advance and develop that Undulatory Theory of light which is now universally accepted. If we strike a stretched wire at one end, or shake the free end of a rope, or of a carpet, whereof the other end is fixed, a series of waves, undulations, or vibrations travels along these bodies respectively. There is no movement of translation of their solid particles; it is the form of the wave alone that traverses them. The particles themselves simply move up and down. The manner in which the wave-form may have its origin in such oscillations will be understood from Fig. 3.

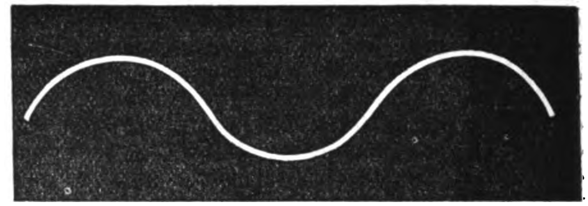

Fig. 3.

The student must cut a narrow slit in a strip of thin, black card, of the size and shape of Fig. 4. Placing now the slit in coincidence with one of the narrow ends of Fig. 3, *i.e.*, square to the length of that figure, and drawing it along such length, while preserving the slit parallel to its original direction, the white spot visible through it will be seen to oscillate in a pendulum-like path; the aggregation of these oscillations obviously making up the waves in our diagram. *What* the medium is in which the light waves are propagated is, in the existing state of our knowledge, a profound mystery. It fills space, it penetrates every body, solid, liquid, and gaseous, existing equally in the most perfect vacuum producible. The phenomena of light point rather to a solid constitution of the so-called "ether" than to a fluid one, and there are mathematical reasons (whose exposition, however, is wholly foreign to the object of the present series of papers) for supposing that the undulations or vibrations in it—which certainly are transverse to the direction of propagation of the rays of light—occur without any interchange of place among the molecules of the ether, which never seem to depart by any measurable distance from their own special localities in space. We shall take occasion, as we go on, to show how beautifully the undulatory theory accounts for the most diverse optical phenomena; and how it has enabled previously wholly unsuspected phenomena to be confidently predicted, with the result that observation has absolutely and accurately justified such prediction.

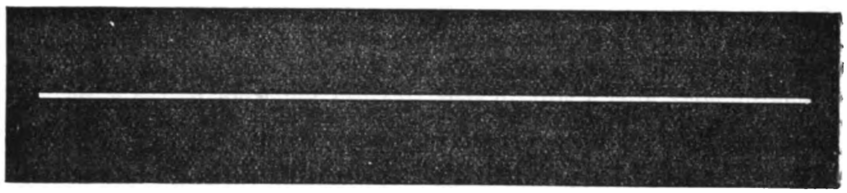

Fig. 4.

(To be continued.)

* Many recent text-books make a cheap display of learning by giving all these quantities in mètres. Written, however, as KNOWLEDGE is, by Englishmen for the English-speaking races, we have preferred to adhere to a standard of length recognised wherever our language is read or understood.

THE AMATEUR ELECTRICIAN.

BATTERIES.—IX.

SOME of our readers ask, Is the current produced by a dynamo machine the same as that generated by Bunsen cells, and how many such cells when used for electric lighting will produce the same candle-power as a dynamo? This question, or another similar to it, rises to the lips of everyone who gives but the briefest consideration to the subject. Our readers will probably remember that in the series of articles on "Electrical Measurement" (see KNOWLEDGE, Nos. 51, 55, and 58), the applicability of batteries

to electric lighting was discussed somewhat fully, and we showed that any desired current could be obtained by calculated combinations. Let us suppose, for example, that the carbon filament in a given incandescent lamp requires, to yield a light of 25 candle-power, a current having an electro-motive force of 50 volts, and a current-strength of 1·25 ampères, the resistance of the hot filament being 37·5 ohms.

Now a Bunsen cell has an E.M.F. (electro-motive force) of 1·9, or say 2 volts, the internal resistance for one of about quart capacity being ·1 ohm. Clearly to produce a current of 50 volts we shall require at least 25 cells, the resistance of which will be, let us say, 2·5 ohms (·1 × 25). By Ohm's law the current flowing through a given circuit is equal to the E.M.F. of the generator divided by the total resistance of the circuit. Whence we get $C = \frac{E}{R} = \frac{50}{2 \cdot 5 + 37 \cdot 5} = \frac{50}{40} = 1 \cdot 25$ ampères. Here, then, we have all that is required, although it would not be by any means an economical arrangement. By doubling the size of the battery plates, or by joining two sets of 25 cells side by side or in parallel circuit (Fig. 1, A B), so as to reduce

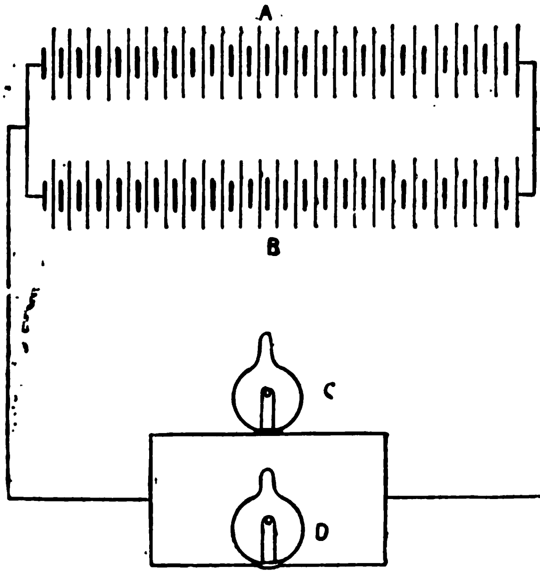

Fig. 1.

the total internal resistance of the battery to 1·25, we can place two lamps (C D) in parallel circuit, and obtain a luminosity of 25 candle-power in each lamp. The joint resistance of the lamps will be 18·75, or exactly half the resistance of one lamp, because we in reality give the current two equal channels of transmission. We then get

$$\frac{50}{1 \cdot 25 + 18 \cdot 75} = \frac{50}{20} = 2 \cdot 5 \text{ ampères.}$$

the E.M.F. remains at 50, because the size of the cell does not affect it. The current of 2·5 ampères divides equally between the lamps, 1·25 going through each. Although the whole current has to encounter in the external circuit but 18·75 ohms, it must not be forgotten that each half of the current has to overcome 37·5 ohms, otherwise, less than 50 volts would suffice, and our purpose would be accomplished by reducing the number of cells.

To feed an arc lamp which requires an E.M.F. of, say, 80 volts, and a current strength of 10 ampères, the resistance of the arc, lamp, coils, and connecting wires being 4 ohms, 40 Bunsen cells will be necessary. The internal resistance of the cells being 0·1 each, we get

$$C = \frac{E}{R} = \frac{80}{4+4} = 10 \text{ ampères.}$$

But 25 Bunsen cells will illuminate two incandescent lamps almost as well as they will one lamp.

Thus $$\frac{50}{2 \cdot 5 + 18 \cdot 75} = 2 \cdot 35 \text{ ampères,}$$

or 1·175 ampères per lamp.

Were we to join two arc lamps in parallel circuit, and feed them with 40 Bunsen cells, we should get

$$\frac{80}{4+2} = 13 \cdot \dot{3} \text{ ampères,}$$

or only 6·6 ampères per lamp, instead of 10. This follows because with incandescent lamps the battery resistance is low when compared with the lamp resistance, when only one or two lamps are in circuit. This simple arithmetical cause does not operate in the case of arc lamps, because the external resistance is also low. To feed two arc lamps in "series" we should obviously require 80 cells to yield an E.M.F. of 160 volts.

To compare more effectually a Bunsen battery with a dynamo, we will choose a Brush machine, which shows up the superiority of the dynamo over the battery for arc lighting. In the course of a series of measurements it was found that the internal resistance of a certain 16-lamp machine was 10 55 ohms, the resistance of the 16 lamps (joined in series), together with their arcs and the wire connecting them with the machine, being 72·96, let us say 73 ohms. The total resistance in the circuit was, therefore, roughly, 83·5 ohms. The E.M.F. produced was 839 volts, and the current in the circuit:—

$$\frac{839}{83.5} = 10 \text{ ampères.}$$

To produce this E.M.F. would require at least 420 cells, which, if they were chosen similar in size to those previously referred to, that is with an internal resistance of ·1 ohm per cell, their total resistance would be 42 ohms. But only 10·55 can be permitted to enable them to give a current equal to the dynamo, consequently the cells will have to be quadrupled in size, or else four sets of 420 cells joined in multiple arc. This number (1,680), at 5s. per cell, would cost £420 at the outset, not to mention the expenditure for acids, which would be very considerable. Any one who has read our remarks on the Bunsen cell (KNOWLEDGE, No. 88), together with the eighth article on Batteries (KNOWLEDGE, No. 114), will see at a glance the practically unsurmountable obstacles to the substitution of Bunsen or any other cells for the dynamo. To compete with a Crompton-Burgin or any other large incandescent dynamo, feeding 500 incandescent lamps, each cell, if we wished to use a single series of, say, 25 cells (to produce 50 volts E.M.F.), would have to be about 83 times the size of the cells offering ·1 ohm resistance, or, if these latter cells were to be joined up in parallel circuit, we should require 2,075 cells.

It is hoped that what has here been said will, in conjunction with the articles referred to, effectually demonstrate the very restricted sphere to which chemical batteries are confined.

To return to the discussion of the various forms of battery useful and available, we may take up the thread with the "Edco," which belongs to the Bichromate class, and was introduced by the proprietors of the Griscom Motor for driving that little piece of apparatus. The battery consists of a wooden box, lined with lead (which is practically unassailable by the constituents of the cells), a leaden partition dividing it into two distinct cells. The size of each cell is

12 in. high by 6 in. long and 8 in. wide. The carbons, of which there are two in each cell, are fixed, the zincs (which dip into porous pots) being attached to a cover, capable of being removed. The plates are 10 in. long by 6 in. wide, and $\frac{1}{4}$ in. thick. The outer solution consists of 3 lb. of sulphuric acid, $1\frac{1}{2}$ lb. of bichromate of potash, dissolved in six pints of water. The porous pot is filled with dilute sulphuric acid. It is claimed that the battery has an E.M.F. of two volts per cell, and that on short circuit it gives a current of 5 ampères, which means that the internal resistance is $\frac{2}{5} = \cdot 4$ ohm per cell. The battery, it is said, may be relied upon to give a steady current for sixty consecutive hours, but a recommendation is added to re-amalgamate the zincs every ten or twelve hours, which rather detracts from its value. This may, we opine, however, be easily overcome by placing a few ounces of mercury at the bottom of each porous cell, and so allowing the zincs to amalgamate themselves as they do in the Fuller cell (see KNOWLEDGE, No. 112).

In passing we may observe that where there is a large area occupied by the zinc, as in the case of the bottom of a cylinder, a very handy method of maintaining the amalgamation is to cut or turn, in a suitable piece of dry wood, a shallow groove a little wider than the thickness of the zinc, and, after well painting it all over with shellac varnish, to run in a little mercury. The zinc may then be stood in the mercury, and the amalgamation will be maintained, at least until the supply of mercury is exhausted.

ERRATA.—The corrected proof of article on the Patent Act of 1883 was unfortunately returned to the editor instead of the publishers, and so arrived too late to be used. The following alterations should be made (one or two others, as *facto* for *facta*, &c., are obvious):—P. 325, first col. l. 19, for "proceeding" read "producing"; l. 38, for "them" read "themselves."

J. B. ROGERS'S ELECTRIC LIGHT AND POWER COMPANY (LIMITED).—Vice-Chancellor Bacon recently made an order for compulsory winding-up on the petition of Messrs. W. T. Glover and Co., creditors of the company. The company's property was in the possession of a receiver. The company did not oppose. This is the company which was to have set the world on fire with the J. B. Rogers's brass ball "accumulator," or, as it has been more properly styled, the "universal binding-screw."

CAPTAIN SHAW expresses a very favourable opinion of the Liquid Fireproof Cyanite. He states that a packing-case coated with two coats of Cyanite (the top inner half also coated with oil paint) offered great resistance to flames, "after which the fire inside died out of its own accord without destroying the case." He afterwards split open the planks with an axe, and found that only the inside surface was charred." A small flight of wooden stairs painted with Cyanite "resisted strong flames well, and practically was not weakened by them." He considers that Cyanite would be found most useful in preventing the spread of fire, and that "wooden stairs coated with it would be much safer than stone."

AN INDUSTRIAL USE FOR ELECTRICITY.—M. Henri de Parville calls the attention of wine-growers to the services electricity might render them in protecting their vines from the disastrous effects caused by a sudden fall in temperature. For several years past vines in France have been protected from the effects of cold winds or sudden changes in the temperature by artificial clouds of smoke, produced by the burning of tarred straw and other combustibles. Vine-growers have learned to appreciate the benefits of this system, but to light the fires at the right moment in the night-time, it is indispensable to have a thoroughly reliable watcher. By the aid of electricity the safety of the vines would be ensured. A vine-grower has only to place in his vineyard one or more electric batteries similar to those used to fire mines, the wires being connected with the fires prepared at regular distances. When the thermometer falls towards zero, by means of a very simple arrangement it causes the electric current to pass along the wires, the fires are lighted, the air being filled with the protecting smoke-clouds. M. de Parville cites one or two instances in which his system has been most successfully employed, with very little outlay to the vine-grower.—*Electrical Review.*

Editorial Gossip.

SCARCELY ever has a more appalling disaster at sea been recorded than the loss of the *State of Florida* and the *Ponema*. The details indeed will not bear to be dwelt upon, scarcely even to be thought of. Turning from them to the collision itself, could there have been a more striking illustration of the inadequacy of the method of lighting adopted for our ocean steamers? One half of the strength of each side light is carefully taken off by the use of coloured glass, as though with no other object but to increase the chances of collision! Not a sign of the actual course of the swiftly moving steamer is afforded by her lights! Because theoretically a certain method of lighting ought to make steamers nearly safe, the method is adhered to though over and over again it has been proved inadequate for safety. The finest ocean-going steamer in the world is not safe against such collision as sank the *State of Florida*. Yet at an expense which would be a mere nothing compared with the value of the steamer itself, to say nothing of the lives entrusted to her, collisions might be rendered all but impossible.

IN an article commenced this week, I indicate one plan (among several available) by which ocean steamers could be thus ensured against the danger of collision, by a method of lighting which would in the first place show a steamer at least twice as far off as the present system, and in the second—which is of far more importance—would indicate her distance, course, and every change of either, as clearly on the darkest night as if it were broad day.

I WAS astounded to find in the table of contents that "Our Night Sun" was included; for though KNOWLEDGE aims at originality of thought and variety of subject, yet a night sun appeared rather too original, and suggested a quite unexpected departure from all that has heretofore been observed. I was relieved to find that the heading itself was correctly given. Doubtless it was transcribed rather hurriedly by whomsoever it may be to whom the preparation of the table of contents is entrusted, and so the marvellous phenomenon of a "night sun" appeared among the contributions to KNOWLEDGE.

I HAVE received quite a number of communications, asking whether the signs ♎ opposite the earth's place in spring and in autumn, at p. 310, should not have been interchanged. They are, however, correctly placed. The notion seems to be that ♈ is necessarily the spring sign and ♎ the autumn sign; but this is only the case when we are considering the sun. He enters the sign Aries at the vernal equinox, and the sign Libra at the autumn equinox; but the diagrams at pp. 310, 311, like all plans of the planetary orbits, are heliocentric,—that is the planetary positions are referred to the sun as centre. Now from the sun as centre the earth enters Libra at the time of the vernal equinox and Aries at the time of the autumn equinox. Therefore in any heliocentric chart ♎ is the sign for spring and ♈ for autumn.

I MAY remark that the charts at pp. 310, 311, are reduced from some which I published as far back as 1867. I used the same method of giving two charts—the only satisfactory way of presenting the solar system—in the

illustrations to my article on "Astronomy" in the edition of the "Encyclopædia Britannica" now issuing.

SEVERAL suggestions have been made as to plans for showing the positions of the planets usefully. But the difficulty of the problem has not been fully recognised. It will be obvious that in the case of the diagram on p. 310 the chief difficulty resides in the circumstance that the positions of Mercury and Venus vary rapidly; whereas, in the case of the diagram on p. 311, the outer planets change so little in a month, or even a year, that it is hardly worth while to give diagrams at short intervals. I think my only course is to deal separately and often with Mercury, Venus, and Mars; and to give one diagram for the next ten or twelve years in the case of the outer planets. Even so the drawing of lines of sight, &c., must be left to the student, as otherwise most confused diagrams would result,—at least if all such lines as *might* by any chance be wanted were drawn in.

I FIND it is the general feeling, as certainly it is my own, that Mr. Ossipoff Wolfson has done as much as is desirable in the way of making it abundantly clear to all why the Polyonymous founder of the Zetetic Astronomy started that amazingly childish system and described the absurdly impossible experiments in which his few astronomical followers believe. Any astronomical absurdity may be safely trusted to impress the foolish,—and the flat earth has probably served its inventor well. But now we know why it was invented we care to know no more. We do not want a disproof of the flat earth from any one, seeing that no one with an average allowance of brains believes in it; and no amount of proof would avail with one who has not that average. Certainly we were not expecting any disproof from Mr. Wolfson, who as a former believer in it, is *ipso facto* disqualified. If Mr. Hampden should ever lose faith in Zetetic Astronomy we should not ask him—of all men—to point out its absurdity. A man who at sea had taken land for a cloud long after every one else on board had seen houses and trees on it, would not be exactly the person to whom one would turn for disproof of the cloud fancy.

MR. IRVING has made some excellent comments on the effect of an appreciative audience upon an actor. He might have gone so far as to say that an actor's real power is never known by those who have only seen him when before a cold or dull assembly. It is much the same with the actor as with the lecturer. I know that, in my own case, my audiences have very much to do with the lectures I give. Who can be enthusiastic before a thin and unappreciative audience? One might as well try to warm oneself at a wet blanket.

MR. A. C. RANYARD, an authority, as our readers know, on the subject of meteors and meteorites, would be obliged if General Norgate would forward to him (to the office of KNOWLEDGE, marked "for Mr. Ranyard") the piece of stone whose fall is recorded at p. 336, under the heading "A Strange Incident."

I LOVE a joke, even when it tells against myself,—and I have a very good one now. A kind friend, of the theologically good-natured sort (he uses a disguised hand, and has apparently taken my address from the list of Fellows of the Royal Astronomical Society, at any rate he uses an address six years out of date), sends me a cutting from the *Cheltenham Examiner*,—a letter, to wit, probably written by himself. Now I thought I had pleased and hoped I had instructed my charming Cheltenham audiences. I had done my very best, and if they had most anxiously desired to make me think they were content they could hardly have done more than they did, by close attention and apparently quick appreciation of explanations, descriptions, and illustrations. But "*surgit amari aliquid.*" Here comes "J." (whom, but for the admission of his letter into the *Cheltenham Examiner* I should have inferred to be the local lunatic at large), who thinks that I must have been laughing in my sleeve, and saying "How very easily that Cheltenham audience was satisfied."

THIS "J." literally expends himself or herself, in his or her wrath,—query whether feminine, and a blue jay? ("There's more *to* a blue jay," says Mark Twain, "than any other creature. He has more moods and more different kinds of feelings than any other creature; and whatever he feels he can put into language; and no mere commonplace language either but rattling out-and-out book talk—and bristling with profundity—just bristling." My "J." *must* be a blue jay.) He or she gives the following as a stock sentence from my lectures. Observe not a word about the utter absurdity of the statements; it is only the too frequent repetition of these "truths" to which the blue "J." objects:—

"By the help of scientific analysis we are able to detect the existence of vapour of hydrogen in the solar corona, but we are not able by the same means to detect the presence of sodium in Rigel, nor are we able to detect the presence of sodium in Betelgeux, nor can we detect it in Aldebaran: if we had been able by the help of scientific analysis to detect it or the vapour of it in Aldebaran, we might justly expect the same result would attend our observation of Rigel, and so of Betelgeux; but failing in this latter, neither can we any more indicate its presence in the two former."

THIS is the sort of thing "J." recalled "on a careful retrospect of the lectures." And apparently neither he (or she) nor the editor of the *Cheltenham Examiner* recognises how gross an insult to the Cheltenham audiences is implied by the suggestion that they could listen to such inane nonsense for five minutes.

THIS "J." had formed his own idea of what my lectures should be. He expected to be told the names of the stars, and why the sun rises (as "many of us are able to see") at different parts of the Cotswold Hills in different parts of the year, and a number of other things which an intelligent child can learn from the ordinary text-book, though to "J." they may seem worth discussing before an audience of educated persons.

THAT one who "on a careful retrospect" got such nonsense out of my lectures as I have quoted above, should imagine there was a good deal of repetition in them, is natural. But to myself whose real trouble is that each lecture of the six ranges over at least three times as much ground as I can properly cover in the time allotted to it, the charge of vain repetition is simply absurd. I had or rather gave a singular illustration of this wideness of subject only last week. A lecture had been arranged at Llanelly by Sir Arthur Stepney, who expressed a wish—after hearing it—to hear my lecture at Swansea on the

same subject. I therefore determined, that, though the syllabus and most of the pictures were necessarily the same, I would give him an entirely different lecture; and I did: I doubt if one statement out of fifty was the same even in subject matter as on the previous night. Sixteen out of forty pictures were different. Yet my subject was in both cases The Life of Worlds, and every sentence of each lecture could have been woven into one lecture elucidating that subject. I believe I could quite easily fill six hours with matter relating to the Life of Worlds alone. My wife has heard the lecture (*how* she could, I know not) many times, and not once, I venture to say, without hearing new arguments and learning new evidence.

OUR "J." grows serious over my comet lecture. I gave this at Cheltenham very much as I did at Exeter; and the Bishop of Exeter in remarks which those who advertise my lectures have made rather more public than I should myself have desired, spoke of that lecture in terms which assuredly encouraged me marvellously. This lecture, the local "at large" found most empty of all real information (meaning doubtless the sort of information children need) and "containing a grave offence against the religious feelings of the audience." Dr. Temple did not find anything irreverent in the suggestion that the angel seen by David and the elders of Israel with drawn sword over Jerusalem, who subsequently returned his sword into the sheath, "was only a comet." The idea was at any rate not brought forward for the special benefit of my audience at Cheltenham, but will be found fully dealt with in my article on Dangers from Comets ("Mysteries of Time and Space," p. 207). There is no doubt that the men of those old times found signs in natural phenomena. For them the sun, moon, and stars *were* "for signs." The "stars in their courses" fought for and against men. Is there irreverence in saying that the "bow" seen by Noah and his family, in the heavens, and by them regarded as the token of a covenant, was *only* a rainbow? The account in both cases correctly presents the feeling with which men in those days regarded all that they saw in the heavens. That the comet should be regarded as an angel of the Lord is no stranger than that a rainbow should be regarded as a token of God's promise. It would indeed be absurd for us to view either phenomenon in this way; but it was a natural and even beautiful thought in old times.

IT would be strange if comets, the most impressive and awe-inspiring of all the celestial phenomena, had been left entirely unnoticed by men so impressionable as clearly were those writers to whom we owe the older books of the scant but precious Hebrew literature which has reached our time. I take the true explanation to be that which I have suggested, that comets appeared to the children of Israel even more impressive than to other ancient nations, and that as they found in the rainbow a message of mercy from the Almighty, so in a comet they saw His message of wrath, with a sword which, as the comet moved, turned every way, or waved threateningly over Jerusalem. In later ages we know that men recognised in the slow retreat of a comet the sheathing of a mighty sword. What more natural than that the people of David's time should have thought that the angel of the Lord "put up his sword again into the sheath thereof." That these ideas were natural in those days, and even much later, is shown by what Josephus says of those who "having no understanding rightly to use their eyes" did not heed celestial messengers of wrath—"as for example that comet in the form of a sword which hung over Jerusalem for a whole year."

"Let Knowledge grow from more to more."—ALFRED TENNYSON.

Only a small proportion of Letters received can possibly be inserted. Correspondents must not be offended, therefore, should their letters not appear.

All Editorial communications should be addressed to the EDITOR OF KNOWLEDGE; *all Business communications to the* PUBLISHERS, *at the Office, 74, Great Queen-street, W.C.* IF THIS IS NOT ATTENDED TO DELAYS ARISE FOR WHICH THE EDITOR IS NOT RESPONSIBLE.

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A STRANGE COINCIDENCE.

[1237]—As you are just now open to correspondence upon the subject of coincidences and the like, you may not think the following too long for, or unworthy of, insertion in your columns:—

On Sunday, May 6, 1866, I was sitting in our drawing-room with my father, about 9.30 p.m., when my mother, who had been upstairs, came down hurriedly, and in great excitement stated that, whilst speaking to one of the children in bed, she had heard footsteps ascending the stairs; the door of the room she was in had been pushed open, that the same footsteps had gone up another flight of stairs and into a room overhead, where her eldest son, then absent in Australia, had formerly slept.

Father and I immediately took a light and went upstairs, and indeed all over the house, without finding anything to afford us the notion that any human being had caused the noise which had so agitated my mother.

On returning to the drawing-room we found mother in tears, and, in despite of our assurances, she remained impressed with the belief that it was a supernatural visit, and that she would never see her absent son again.

The circumstance passed from my mind, and in the July or August of that year (1866) we received a letter from my brother, stating that he should sail from Melbourne in the *General Grant*, and that (D.V.) we might expect to see him almost as soon as his letter reached us.

After anxiously watching the papers for some weeks afterwards, no tidings of the *General Grant* came to hand, and ultimately she was numbered amongst the "missing"; and although we still hoped against hope, nearly eighteen months had passed when one morning a paragraph appeared in the daily papers announcing that a telegram had been received to the effect that the *General Grant* had been wrecked on the Auckland Isles, and that a few of her survivors had subsisted there for a period of thirteen months. Later news brought a list of the survivors and details of the wreck, but my brother's name was not included in the list.

The details (I am quoting from memory, as I have not read them since 1868) stated that the vessel had been found to be out of her course (it seems, however, that the Auckland Isles had been wrongly charted), and before they were able to adopt any means to prevent it, she had been dragged by currents towards one of the islands and into a cave, in the recesses of which she was rapidly dashed to pieces, but not, however, until all the boats had been filled with passengers and despatched with a view of landing upon the islands. One of the boats, containing upwards of a hundred of the passengers, &c., was unhappily beaten against the rocks, and all on board drowned.

The survivors stated that as they rowed away from the mouth of the cave the captain and one or more men, who could not be accommodated in the available boats, were seen clinging to the rigging of the vessel, and that when some of them subsequently returned to the cave, all traces of the *General Grant* had disappeared.

According to the narrative it was in the evening of May 5 that they found themselves in the vicinity of the islands which proved so disastrous to the vessel and its occupants, but no particulars were given as to what hour the long-boat was lost, nor what time it was when the boat containing the survivors lost sight of the captain and his companions.

Now it will be seen from what I have stated that my brother

must have been lost *about* May 6, the date when the strange noises at home impressed my mother with the belief that she would never see her son again; but I have never been able to obtain any information which would enable me to make, what I have ever since looked upon as a coincidence, more complete. Perhaps this may meet the eye of one or more of the survivors, who might be able and disposed to furnish the necessary information, which would not fail to be of interest generally, and to myself in particular.

As far as I have been able to learn, 9.30 p.m. May 6 in London would be equivalent to 8.30 a.m. *May* 7 at the Auckland Isles, so that the only date given by the survivors which I have seen affords little or no evidence even as to the coincidence of the matter, as the evening of May 5 at the Auckland Islands—supposing the wreck occurred in the evening—would have been the morning of the same date here, or eighteen hours before the noise my mother heard.

It may be, however, that the actual loss of the vessel, or, at any rate, of the life of my brother, may not have happened until the hour coincident with 9.30 p.m. here of May 6, and I shall therefore be glad indeed to learn through your columns some positive information on the subject.

Personally, I do not believe in apparitions, nor in anything akin thereto; but coincidences such as you record from week to week must have happened to most of us, and obtuse indeed must the individual be who does not think that there is something supernatural sometimes even in coincidences. J. J. COLLINS.

VISION.

[1238]—The writer of letter 1174, April 4, is, I think, wrong in stating that visual impulses are caused by the "vibrations" of the optic nerve fibres. The presence of the blind spot, and the phenomenon of Parkinge's figures, prove that the optic nerve fibres are insensible to light. It is probably in the layer of rods and cones that the impulses originate, though how rays of light stimulate those bodies is not definitely known. There is no absolute proof that visual sensations become perfected into visual perceptions in the frontal lobes.

It is manifestly incorrect to speak of effects produced in the "optic ganglia" by an intrinsic stimulation, as "optic *nerve* vibrations." J. C.

THE MYSTERY OF GRAVITY.

[1239]—I perceive from your answer to my question, Vol. V., No. 127, p. 232, that my illustration of the difficulty of reconciling the law of gravitation with that of the conservation of energy was not the best I could have chosen; and, with your permission, I will again try to make my meaning plain, in the sincere hope that you, or some one else, will point out the fallacy, if there be one, in my argument.

I am travelling beyond the boundaries of civilisation, and have no books of reference with me to give an accepted formula of the second of these laws; which is, however, generally supposed to teach that energy is an entity, the amount of which in the universe can never become greater or less, but that there are various forms of it, among which are commonly enumerated heat, light, electricity, momentum and latent energy, such as that in fuel, all of which are convertible into one another in definite proportions, so that the amount of work which can be done by each of them under fixed conditions can be exactly measured; the energy in a pound weight of coal can, for instance, be converted into a measurable amount of heat, which, again, can raise a certain weight a definite height, and after this has been done all the energy in the coal has passed away.

In addition to these and other closely allied forms of energy there is a more mysterious one, known as the attraction of gravitation, which is at least in so far of the same nature that it can generate either directly or indirectly momentum, heat, &c., but of which it cannot be proved empirically, as it can of the other forms, that it becomes less in proportion as work has been done by it.

Thus, if the energy in falling water is traced backwards, the source of it will be found in the attraction of the earth. It has been hastily assumed that its source is the sun, whose rays are said, not by poets only, to draw up water into the sky. It is obvious, however, that though the sun's heat vapourises the water, and thus renders it lighter than the air, it is the gravitation of the earth which forces it upwards, like a cork is forced to the surface of water by the superior weight of the latter, and that after the vapour has gradually radiated into space, or otherwise lost the energy it had derived from the sun, it again condenses, and, becoming heavier than air, is drawn downwards by the gravitation of the earth; nor is there any connection or commensurability between the energy of falling water upon the earth and that energy of the sun which had previously vapourised it, and which had been absorbed by the vapour—according to Professor Tait, to the amount of one-horse-power for half-an-hour to every quarter of an inch of water, but all this energy had departed before the vapour was again condensed. The energy expended on the surface of the earth by falling water has been great beyond conception. We know the effects of a portion of it in continents carried down into oceans, and this vast molar motion was, it is presumed, converted gradually into molecular and dissipated into space; now it was all derived in the first instance from the attraction of the earth, but no one will venture to say this work was done at the expense of this attraction, and that the energy of it is being gradually used up and passing into other forms. You lately explained that gravitation is a mystery. I offer what seems to me a fresh proof of this, for, though the conservation of energy is a postulate of reason, neither observation nor experiment can bring this law into harmony with the phenomena of gravitation. J. C. MURRAY AYNSLEY.

AN UNSOLVED TRICYCLE PROBLEM.

[1240]—Simply our old friend the "Sucker." The tyre being round the weight on the *centre* of the track only is great enough to enable the tyre to draw up a ridge of dust after it; *mud* sticks to the whole of the bottom surface of the tyre, which we all know only too well. R. P. G.

CATS AND DOGS.

[1241]—I notice the last paragraph of the article, p. 243 of last week's KNOWLEDGE, taken from a paper by Mr. C. A. White. What he there states is doubtless generally correct, but I have a brindle terrier and a tabby cat which quite reversed what seems to be the usual state of things.

About a year ago I moved a short distance; the dog was many times afterwards found at and brought from the old house, but the cat never. The moving made no difference to him. Each is an affectionate animal to us all. E. B. WILLIAMS.

SCHMIDT'S LUNAR MAP.

[1242]—In reply to "G. Williamson," permit me to say that Mr. Wesley, Essex-street, Strand, London, could very likely supply Schmidt's Great Map of the Moon. I know it was in his catalogue the other day. Mr. Wesley has served me well on many occasions. "Let Knowledge grow from more to more." ARTHUR MEE.

South Wales Press, Llanelly.

EARTHQUAKES FROM THE DISTURBANCE OF THE SUBTERRANEAN WATER DISTRIBUTION.

[1243]—The recent earthquake has suggested to me the possibility of earth movements from the disturbance of the supply of water to the subterranean reservoirs by reduction, and also by hydraulic pressure; in the first case by producing a vacuity under a large district, causing a subsidence of the land; and in the second by a lofty head of water, of small diameter, acting upon a large area of pent-up, confined fluid without other outlet, causing an elevation of the strata above it. It has been reported that at Colchester, previous to the earthquake, there was a deficiency in the supply to the waterworks, and that alterations in the engine-pumps were contemplated, and that after the earthquake the water rose in the wells 75 ft., so that the proposed alterations in the pumping-gear were abandoned. This extraordinary supply may have been caused by a subsidence of the land, pressing upon the water in the internal reservoirs, and thus forcing it into all the outlets, into the supply wells. If this were really the case, it is probable that the extraordinary supply to the waterworks will be of short duration—nay, that in a short time it may altogether cease.

W. CAVE THOMAS.

CAN THE SEVERED HEAD THINK? FALSE PERSPECTIVE, &c.

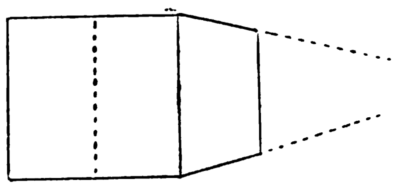
[1244]—Thought seems to act with almost infinite rapidity. For example, an apparently long and circumstantial dream is often suggested by some sound that appears to be actually connected with an incident that comes in at its conclusion and causes us to wake with an unpleasant start. It is reasonable, therefore, to suppose that there is ample time for much intensely horrible thought during the instant the guillotine's sharp knife is passing through the hapless culprit's neck. Cerebration may perhaps continue as long as there remains any blood in the brain; if so, what ghastly and phantastic processions may pass before one of Mr. Binn's slowly-strangled victims!

Dr. Lamprey, then of the 67th Regiment, in his lecture delivered at the United Service Institution some years ago (the subject was

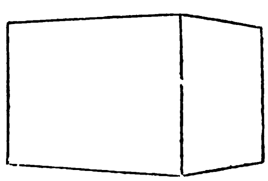
the Economy of the Chinese Army) relates the following curious anecdote:—

"I was passing through the narrow main street (of Taitsan) when my attention was drawn by a crowd of people looking at what proved to be the head of a Chinese soldier who had just suffered decapitation. . . . While looking at this head I was much struck by observing the quality of saliva that continued to flow from the mouth, and on examining the appearance of the wound, a very jagged one, indicating that it was not done by one clean cut, I observed that there was a remarkable spasmodic action of the incised muscles of the back of the pharynx and tongue, and continuing to notice this for a little time, I found that the action of the contraction and relaxation occurred at irregular intervals, and that all the muscles appeared to act together. That this was or was not an evidence of consciousness or thought passing in the mind of the individual, may be a difficult problem to solve; but, be this as it may, I could not help recording the exclamation of one of our soldiers—an Irishman, of course—who was standing by, and observed me watching the muscular movements which he also noticed. 'Arrah sure! sir,' said he, 'I think he is striving to spake.'" After a lapse of ten minutes or so, the doctor remarked that the muscular movements continued to be nearly as active as at first. Here, certainly, is food for most curious speculation.

In many elementary books on perspective, I find the followin obvious mistake, which, I think, few teachers trouble themselves correct. We are supposed to view the front face of a cube from



point exactly opposite its centre, and from this position to see one of its sides, as shown in Fig. 1. This I take to be clearly impossible. In order that we may be enabled to see a side, we must



stand to the right or left of a perpendicular passing through the centre of the face, and in this case both the face and the side would appear to retire, as shown in Fig. 2.

Many readers of KNOWLEDGE consider your excellent Lessons on Co-ordinate Geometry fully worth the cost of the entire journal, and I for one trust they may be continued for a long time to come.

R. JONES.

THUMB WEAKNESS.

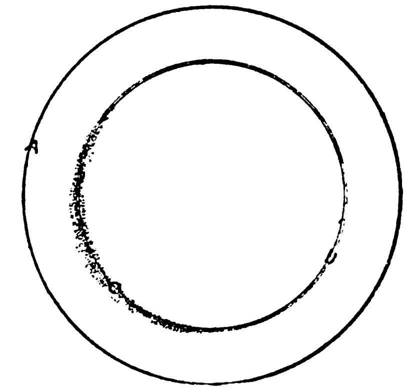
[1245]—Is it too much on my part to ask if any of your correspondents can explain how it is that the power of using the thumb for holding the pen in writing should have been (practically) absolutely taken away, whilst the power of writing solely by holding the pen in the first and second fingers has developed? There are in this town two cases I know of (myself and another.)

The ability to use the thumb continues for all ordinary purposes except that of holding small, slender bodies so as to move them at will between the thumb and fore fingers.

Can they suggest any remedy for restoring the use of the thumb, for it seems highly probable that continuing to write with the thumb packed in the palm must weaken it? "PHONO."

DUST.

[1246]—With reference to Professor Langley's article on "Our Earth's Dust Envelope," I venture to add two notes as illustration. 1. The singular, and perhaps oft-noted, appearance of dust within binoculars out here. If you leave a pair, after cleaning, on your table for a fortnight, untouched, as surely will you find a layer of dust on the inner and upper face of the object-glass. How did it get there? Of course, the ready reply is, it is merely the dust, already in the interior, deposited on the object-glass. *That it must be*, but the question is, how did it get there, and why is it not deposited equably on the inner surface of the object-glass, but invariably in a circle corresponding in size to the draw-tube holding the eye-pieces? This diagram will illustrate my meaning. A is the object-glass, B the proportionate size of the draw-tube, and C the circle of dust on the object-glass. Often and often have I most



carefully dusted and blown out the interiors, and then rapidly screwed on the object-glasses, but the result is the same. If such comparatively small interiors cause sensibly and quickly-deposited dust, what must be the deposition of the earth at large?

2. Years ago, when returning from India, and voyaging to the Cape as a boy, the sight of a particular part of the voyage was to see the sails reddened by sand blown from, the Captain said, "the Desert of Sahara!" This sand was sent to Ehrenberg, and he declared it to be made up entirely of fossil organisms found only in South America. Fancy the gigantic proportions of that whirl, which, licking up the South American infusorial deposit, bore it aloft eastwards, dropping it in Africa and on our passing ship, miles from its coast, in its westward sweep!

Morai, Bengal, 1884. R. F. H.

JAPANESE FIGURES.

[1247]—I have two Japanese carvings. One is apparently a woman with long hair, and the lower part of the body is in the form of a fish or snake, coiled round an object like a thimble, which I think is a bell. In the right hand is a hammer. The other figure here represented seems to be a dog-headed dwarf, holding between

its feet a drum. Inside its mouth, which is open, is a human face, with eyes inlaid with glass or some shining substance. I should be greatly obliged to any of your readers who would state to which of the Japanese religions these figures belong, their names (*i.e.*, the figures') and attributes, and any information about them.

E. C. R.

SCIENTIFIC MORALITY.

[1248]—Your correspondent "T. Common," in the letter referred to above, says:—"For those who have independent means, erroneous theories as to duty are not so serious." If the word duty is here intended to mean, as it would imply, the *whole* duty of those who have independent means, this is surely a great mistake, and is inculcating the principle that in ethics there is one law for the rich and another for the poor. It is, indeed, a question whether the disregard of duty on the part of the rich is not the cause of all the Socialistic agitation that is disturbing society at the present moment.

With the spread of education, men who beforetimes were mere machines are learning to think for themselves, and, by the aid of a cheap and free press, to compare facts, and reason upon them. While the lower classes were steeped in ignorance, a deficiency in sense of duty among those above them was likely to pass unnoticed; but all that is changed, and if the leaders of society would retain their pre-eminence they must not only possess theories of duty at once exalted and free from error, but they must manifest them by their lives and conduct, and so exemplify that high standard of morals, the universal adoption of which is a people's surest road to happiness and prosperity.

It appears to me that we are approaching a critical stage in our national life; a point from which a new departure must be made. What our future course may be will depend upon the standard of morals that is allowed to influence us now. If high, and embracing the rights of all, the new departure may be made peaceably, and will lead us to contentment and greater prosperity; if low and cramped by selfishness, it may be disastrous. But since all peaceable reforms must emanate from the legislating class (the wealthy), it is of all times the moment when the possession by this class of erroneous ideas on the question of duty would be most serious.

A. McD.

[It appears to me that Mr. T. Common and Mr. A. McD. are very much at one. Due consideration for the rights of others as well as self seems to be the rule each would lay down.—R. P.]

THE DIASCOPE.

[1249]—Several years since I projected the construction of a diascope—of an instrument for cross vision, *i.e.*, the right eye seeing the objects on the left, and the left eye the objects on the right. My first experiments were made with two paper tubes that could be crossed, and with these I found that the objects seen with the right eye *appeared* to be seen with the left, and *vice versâ*. More recently I have tried two stereoscopic photos, and these appear to combine equally well when the photo on the right is seen by the left eye, and that on the left by the right. On this point, however, I should like to have confirmation. I find, too, by means of the diascope that the field of each eye appears to be of a different tint.

W. CAVE THOMAS.

A PHENOMENAL VOICE.

[1250]—Could any of your musical or physiological correspondents kindly give me a proof of the assertion quoted from Dr. Carpenter in an article by Professor Jeffrey Bell in "Science for All" (Vol. V. p. 45)—"that the celebrated Madame Mara was able to sound one hundred different intervals between each tone. The compass of her voice being at least three octaves, or twenty-one tones, the total number of intervals was 2,100, all comprised within an extreme variation of one-eighth of an inch; so that it might be said that she was able to determine the contractions of her vocal muscles to nearly the $\frac{1}{17000}$th of an inch."

I have a musical friend who altogether denies its possibility, and though I myself am perfectly convinced of its validity, I am unable to remove his doubts.

E. R. COX.

A SPORTING PROBLEM.

[1251]—A sporting paper recently offered a sum of money as a prize to the individual who succeeded in placing the first three horses, as placed by the judge, in each of two races. In the first race there were 81 entries; and the second, 63. Would you kindly calculate, and answer in an early number, the odds against one individual doing this, and the number of papers he would have to purchase to make his winning the prize an absolute certainty?

NED W. WITHERS.

[The number of possible first threes, in definite order, among 81 horses, is $81 \times 80 \times 79$ or 511,920. The number of possible first threes, in definite order, among 63 horses, is $63 \times 62 \times 61$ or 238,266. The total number therefore of possible events is obtained by multiplying together 511,920 and 238,266. Roughly, about 123,000,000,000 copies of the paper would have to be bought to insure winning the prize, which ought to be a rather large one,—say about £500,000,000 if the paper is a penny one.—R. P.]

LETTERS RECEIVED.

T. K. B. I fear that no change in the aperture of eye-piece would be of any use. The eye-glass is made just large enough for all the emergent beams to pass out, and you would find the same effect of looking through a small hole, even though the eye-glass were an inch in diameter. The only way in which, in your case, high powers can be used, is by increasing the size of object-glass. A refracting telescope, having a large O.-G. and short focal length, would suit you. A reflecting telescope would be better still. But whatever telescope you use, you will not be able to use the highest power it will bear with ordinary eye-sight.—JAS. WHITELAW. The earth enters Libra, not Aries, in spring. It is the *sun*, viewed from the earth, which enters Aries at the vernal equinox.—J. BINDON CARTER. I sincerely hope, in the interests of the Martial snail, that the Martial cabbages *would* be on the enlarged scale you mention. But I have no direct information to offer. Very likely there are no creatures on Mars akin to men, elephants, or snails.—J. MURRAY. I am sorry, but hasten to repair the omission by noting that the P.D. system *does* seem to lead you entirely astray.—M. X. Y. It seems to me that in whatever degree the roof impeded the rush of air, in deflecting it, in the same degree would the chimney be protected.—F. H. WOOD. Should have been delighted to examine your geometric ruler; but shall not be at home on a Monday, Wednesday, or Friday (see Lecture List) for weeks.—W. F. CURTIS. The sun and moon cannot be magnified in the way you suggest; moreover, they are not so magnified. They do not look larger near the horizon. We only fancy they do. A halfpenny at the end of a nine-foot rod (square to its length, the eye being at the other end) will just hide the mid-day sun: well you will find that it will also hide the setting sun. The sun and moon both *look* almost exactly as large as a one-inch globe nine feet away, whether they be high up or low down. The moon looks a little *smaller* on the horizon, being then a little farther away than when high up.—SIMPLEX. There is no fallacy whatever about your plan for trisecting an angle. Roll the arc that way, and you can trisect it, and the angle. Roll the whole circumference, and you can rectify, and in effect square the circle. But I have not yet heard of any postulate in "the geometry of the line and circle" running thus:—Let it be granted that the arc of a circle may be rolled along a straight line, so that the points of contact with given points on the line may be marked off on the circumference of the circle.—P. M. C. K. Thanks for account of two large meteors on May 5, at 7. I would insert, but the real paths of the bodies in the air could not be ascertained from positions so "general."—T. H. CHRISTY. Telephone wire sent to electrician.—W. W. SAWTELL. The planchette story is curious; but probably not more than a coincidence.—H. A. VINCENT. Which *is* the great sympathetic nerve? Your theory does not correspond with the history of *all* great poets: and I rather doubt your statement that the born poet has "a peculiar longing and desire for spirits." If adopted that theory would encourage many who are not born poets to imagine such a longing. As a born poet (no doubt) remarked, "A man carn't make hisself a poat, no mor'n a sheep can make itself a goat," but a man *can* make himself a spirit-swiller.—GEO. ELLIS.—You will find me giving the same explanation in *Nature* somewhere about the year 1870. But it never occurred to me that the explanation was a new one. Naturally I agreed with your view, and inserted your letter (KNOWLEDGE, Oct., 1882) as expressing well the theory. Why *should* I "be vexed with you"?—PROTEA. 1. Professor Ledger's Gresham Lectures are thoroughly trustworthy. 2. "Romance of Astronomy" rather weak. 3. A red star was suspected by Trouvelot during last eclipse; but it was certainly not Vulcan (probably for the excellent reason that there is no Vulcan).—JOHN HAMPDEN. We have had enough of the Zetetic astronomy, and cannot allow our readers to be further troubled with such utter rubbish. Who is there who can possibly profit by anything on the subject? Can you imagine that it would be of the least moment if the ten or a dozen, or it may be twenty, persons who have been deceived by Parallax's pretended experiments could be convinced of their folly? The subject has served its purpose in showing what foolish folk may manage to remain at large. The recent letters have brought out—what might have been expected—the fact that the folly of these persons is not limited to one particular subject, but shows itself in whatever they say or do, and is in fact practically unlimited. If it has not made clear to *every* reader of KNOWLEDGE the reason why Parallax started the Zetetic absurdity, there must be one or two foolish readers still remaining,—and we cannot alter our ways on their account. Seek some other paper as a channel for Zetetic nonsense: we will have no more of it.

Our Mathematical Column.

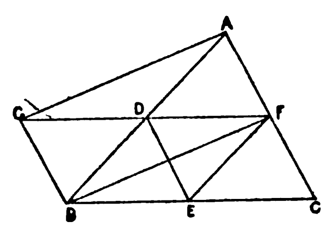
NOTES ON EUCLID'S FIRST BOOK.

BY RICHARD A. PROCTOR.

(*Continued from page* 318.)

WE shall now proceed to some problems on the subject of the First Book, which are of great utility and importance.

PROP. XX.—*If the three sides* A B, B C, C A *of the triangle* A B C *be bisected in the points* D, E, *and* F, *the three lines* D E, E F, *and* F D *are respectively parallel to the sides* C A, A B, *and* B C *of the triangle,* A B C, *and equal to the halves of these lines, respectively.*

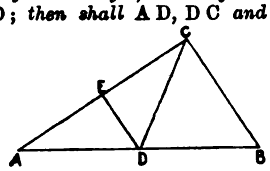


For, produce F D to G making G D equal to D F and join B G, A G. Then by Prop. XVIII. *conv.*, since A B, G F, are bisected in D, A G B F is a parallelogram. Therefore B G is equal and parallel to A F;

that is, to F C. Therefore (Euc. I. 33) G F is equal and parallel to B C. But G F is double of D F (*const.*) and B C of E C (*hyp.*); therefore D F is equal to E C or B E. And in like manner it may be shown that D E is parallel to A C and equal to A F or F C; and that E F is parallel to A B and equal to A D or D B.

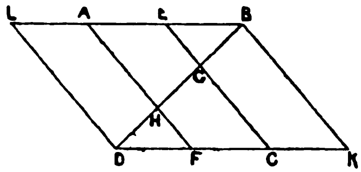
COR.—The four triangles A D F, E F D, B D E, and F E C are equal in all respects (Euc. I. 8 and 4), and equiangular to the triangle A B C.

PROP. XXI.—*Let* A C B *be a right-angled triangle,* C *being the right angle, and let* A B *be bisected in* D; *then shall* A D, D C *and* D B *be all equal.*



Bisect A C in E and join D E; then D E is parallel to B C (Prop. XX.). Therefore the angle A E D is equal to the interior A C B (Euc. I., 2); that is A E D is a right angle. Thus the line D E is at right angles to and also bisects the side A C of the triangle A D C; therefore (Prop. XIII.) A D is equal to D C. Similarly D B is equal to D C.

PROP. XXII.—*If the two equal and parallel straight lines* A B, D C *be bisected in the points* E *and* F, *then* C E *and* A F *trisect the line* D B *in the points* G *and* H.



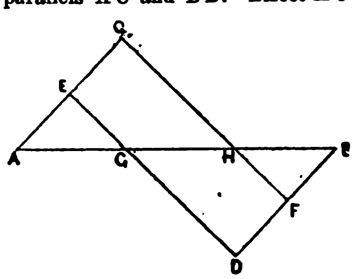
Produce B A to L making A L equal to A E or E B; and produce D C to K making C K equal to D F or F C. Join L D and B K. Then since the lines L A, A E, and E B, are equal to each other and also to the three lines D F, F C, and C K; therefore (Euc. I., 33), the lines L D, A F, E C, and B K are parallels. But L B is trisected in A and E where it meets the parallels. Therefore B D is trisected in G and H. (Prop. XVII.).

COR.—If A B, C D were divided into any number n of equal parts, and lines drawn from C to the division-point nearest B, from the division-point nearest C to the second division-point from B, from the second division-point from C to the third division-point from B, and so on, these lines would divide the diagonal B D into $(n + 1)$ equal parts.

The proof of the corollary would be similar to that of the proposition, C K and A L being taken equal to any one of the n equal parts of B A and C D.

PROP. XXIII.—*To trisect a given straight line.*

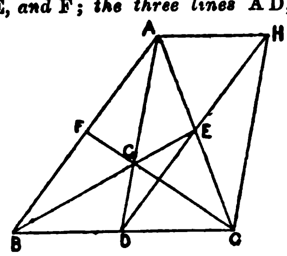
Let A B be the given straight line. From A and B draw, in opposite directions, the equal parallels A C and B D. Bisect A C in E and B D in F. Join D E, and C F; intersecting A B in G and H. Then A B is trisected in G and H. (Prop. XXII.)



COR.—In like manner we can divide a straight line into any number, n, of equal parts. For we have only to draw two unlimited parallels in opposite directions from the points A and B, and to take off from A and B $(n-1)$ equal divisions (of any length) along these lines. Then, joining the points of division in the manner indicated in Prop. XXII. Cor., A B will be divided into n equal parts.

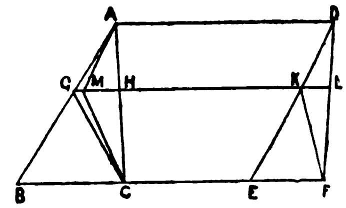
Another method of trisecting a line is usually given. This will be presented as a problem farther on.

PROP. XXIV.—*If the three sides* B C, C A, *and* A B *of the triangle* A B C *be bisected in the points* D, E, *and* F; *the three lines* A D, B E, *and* C F, *pass through one point, which is a point of trisection of each of the three lines.*



Let B E intersect A D in G; join D E and produce to H, making E H equal to E D. Join A H, H C. Then since A C and D H bisect each other, A H C D is a parallelogram (Prop. XVIII. *conv.*). Therefore A H is equal and parallel to D C; that is, to B D (*hyp.*). Hence H D is equal and parallel to A B, Euc. I., 33, and therefore, since D E is equal to E H, D G is a third part of A D (Prop. XXII.). Similarly it may be shown that C F cuts off from A D a third part, towards D. That is C F passes through the point G. And as G D has been shown to be a third part of A D, so may G E be shown to be a third part of B E, and F G to be a third part of F C.

PROP. XXV.—*If the triangles* A B C, D E F *be on the equal bases* B C, E F, *and between the same parallels* A D *and* B F, *and* G H K L *be drawn parallel to* B F, *meeting* A B, A C, D E, *and* D F *in the points* G, H, K, *and* L *respectively,* G H *shall be equal to* K L.



For if not one of these lines must be greater than the other. Let G H be the greater, and from H G cut off H M equal to K L. Join G C, K F, A M, and M C.

Then, since M H is equal to K L, the triangles A M H and D K L are equal (Euc. I. 38), and so are the triangles M H C and K F L. Therefore the triangle A M C is equal to the triangle D K F. But the triangle B G C is equal to the triangle K E F (Euc. I. 38). Therefore the triangles G B C, A M C are together equal to the triangle D E F; that is, to the triangle A B C. But this is absurd. Therefore G H and K L are not unequal. Therefore they are equal.

(To be continued.)

EASY RIDERS ON EUCLID'S FIRST BOOK.

WITH SUGGESTIONS.

PROP. 26.

71. A E B, C E D, are two straight lines intersecting in C; A E is taken equal to E B and lines A D, B C are drawn in such a way that the angles E A D, E B C are equal. Show that E C is equal to E D.

72. If from any point in a line bisecting a given angle perpendiculars be drawn on the lines containing the angle, these perpendiculars shall be equal, and shall cut off equal parts from those lines.

73. In a given straight line find a point such that the perpendiculars from it on two given straight lines shall be equal.

74. Through a given point draw a straight line so as to cut off equal parts from two straight lines which meet in a point.

75. If the straight line which bisects the vertical angle of a triangle is perpendicular to the base the triangle is isosceles.

76. Through a given point draw a straight line such that the perpendiculars on it from two given points may be on opposite sides of it and equal to one another.

PROPS. 27 and 28.

77. Two straight lines A E B, C E D bisect each other in E; show that A C is parallel to B D.

PROP. 29.

78. From the centres A and B of two circles parallel radii A P, B Q are drawn; P Q meets the circumferences again at P and S show that A R is parallel to B S.

79. If a straight line be drawn parallel to one of the sides of an equilateral triangle, it will form with the other sides, produced if necessary, another equilateral triangle.

80. If a straight line be drawn parallel to one of the sides of a triangle, it will form with the other sides, produced if necessary, a triangle equiangular to the first.

81. Two straight lines, A E B, C E D intersect in E; two other straight lines A F and C G are parallel respectively to C D and A B; show that the angle A is equal to the angle C.

82. The point P lies between two parallel lines. Show that if any straight line through P terminated by the parallels is bisected in P, every straight line so drawn will be bisected in P.

83. The intersecting straight lines A E B, C E D, terminated by parallel lines A C, and B D, bisect each other in E; show that A C is equal to B D.

84. The line drawn through the vertex parallel to the base of an isosceles triangle is perpendicular to the line bisecting the vertical angle.

85. If the line bisecting the exterior angle of a triangle be parallel to the opposite side the triangle is isosceles.

86. If from any point in the bisector of a given angle lines be drawn parallel to and terminated by the lines containing the given angle, the lines thus drawn shall be equal and shall cut off equal parts from the others.

87. A B C is a triangle right angled at B, and D is the middle point of A C. Show that if the line E B F is parallel to A C, then the angle E B A is equal to the angle A B D, and the angle D B C to the angle C B F.

(To be continued.)

Our Chess Column.

By MEPHISTO.

THE following two games were played between Dr. Zukertort and Mr. Charles A. Maurian, at the rooms of the new Orleans Chess, Checkers, and Whist Club, April 16, 1884. — *Times Democrat*).

GIUOCO PIANO.

White. Mr. C. A. Maurian.	Black. Dr. Zukertort.	White. Mr. C. A. Maurian.	Black. Dr. Zukertort.
1. P to K4	P to K4	21. Q to R5	Castles
2. Kt to KB3	Kt to QB3	22. P takes P (*d*)	P takes P
3. B to B4	Kt to B3	23. Kt to B3	B to K3
4. P to Q3	B to B4	24. R takes R (ch)	Q takes R
5. B to K3	B to Kt3	25. Kt takes P	P to B6 (*e*)
6. QKt to Q2	P to Q3	26. B takes B (ch)	R takes B
7. Kt to B sq	Kt to K2	27. P takes P	Q to Q7
8. Kt to Kt3	P to KR3	28. Kt to Kt4 (*f*)	Q takes KtP
9. Q to Q2	P to B3	29. Q takes BP	Q takes RP
10. P to B3	P to Kt4	30. K to Kt2	Q to Kt6 (*g*)
11. P to Q4	Q to B2	31. Kt to B6	P to QR4
12. R to Q sq	Kt to Kt5	32. Kt to Q5	P takes Kt (*h*)
13. P to KR3	Kt takes B	33. Q takes R (ch)	K to Kt sq
14. Q takes Kt (*a*)	Kt to Kt3	34. Q takes QP	Q to Kt7
15. Kt to R5	Kt to B5	35. R to KB sq	P to R5
16. Kt takes Kt	KtP takes Kt	36. Q to Q6 (ch)	K to R2
17. Q to Q2	R to Kt sq (*b*)	37. Q to QKt4	Q to R7 (*i*)
18. K to B sq	R to Kt3	38. P to QB4	B to Q5
19. Kt to R4	R to B3 (*c*)	39. Q to R5 (ch)	
20. Q to K2	B to Q2		

And White wins.

NOTES.

(*a*) If 14. P takes Kt, Black replies with 14. * * P takes P, and white cannot take without losing a piece by 15. * * P to Q4.

(*b*) 17. * * B to Q2, preparing for immediate castling, would be better, and leave black with a slight superiority.

(*c*) If 19. * * R to Kt2, or to Kt sq, White would probably gain an advantage by 20. P takes P, followed by Q to Q6.

(*d*) Had White captured the BP, he would have lost his QP in return, as Black would have replied with 22. * * P to QB4, threatening to win a piece by B to QKt4.

(*e*) Unsound. 25. * * B takes B; 26. Kt takes B, Q to Q6 (ch); 27. Q to K2, &c., would draw.

(*f*) The only move. If 28. R to R2, then 28. * * B to B2, &c.

(*g*) 30. * * P to KR4 would have extricated the Rook, and is, therefore, better.

(*h*) Forced. If 32. * * R to Q3; 33. Kt to K7 (ch), winning the Queen.

(*i*) 37. * * Q takes Q appears better, but White could still win, we think, as follows:—38. P takes Q, B to Q5; 39. R to Q sq, B to Kt7; 40. R to Q5, P to Kt3; 41. P to K5, P to R6; 42. P to K6, P to R7; 43. P to K7, P queens; 44. P to K8, queens, and wins.

(The second game played on the same occasion.)

EVANS' GAMBIT (compromised).

White. Dr. Zukertort.	Black. Mr. C. A. Maurian.	White. Dr. Zukertort.	Black. Mr. C. A. Maurian.
1. P to K4	P to K4	13. B to Q3	Q to R4
2. Kt to KB3	Kt to QB3	14. Kt to K4 (*b*)	R to K sq
3. B to B4	B to B4	15. Kt to Kt3	Q to Kt 5
4. P to QKt4	B takes KtP	16. KR to K sq	P to Q4 (*c*)
5. P to B3	B to R4	17. P tks P *en pass.*	P takes P
6. P to Q4	P takes P	18. QB takes P	B to K3
7. Castles	P takes P	19. Q to Kt sq	B to Q4 (*d*)
8. Q to Kt3	Q to B3	20. B takes P (ch)	K to R sq
9. P to K5	Q to Kt3	21. B to K4	Q to Q2 (*e*)
10. Kt takes P	KKt to K2	22. B takes B	Kt takes B
11. B to R3	Castles	23. R takes Kt	R takes R (*h*)
12. QR to Q sq	B to Kt3 (*a*)	24. Kt takes R	

And White wins.

NOTES.

(*a*) 12. * * P to QKt4 is acknowledged to be best. The text move prevents the development of the Queen's Bishop, and permits the adverse King's Rook to occupy the all important King's file.

(*b*) 14. KR to K sq would be stronger still.

(*c*) Black's position is indeed unenviable, and this move is certainly not calculated to benefit it.

(*d*) It was necessary to prevent R to K4 at all hazards.

(*e*) Which loses a piece; but Black's game was past redemption, as the entry of the hostile Rooks into his game must prove fatal.

ENDING p. 299.

1. Kt × P (ch)	1. Kt × Kt
2. B to R5	2. Kt to Kt2
3. B × P	3. Kt to Q sq.
4. B to K8 (ch)	4. K to B2.
5. P to Kt6, and wins.*	

White's second move forms the key-move to the position. But it cannot be played without 1. Kt × P preceding it; the object of this move being to prevent the Black Knight from coming to the assistance of his Pawns. If White at once plays 1. B to R5, then Black would play Kt to Q sq., and White would lose if he took the Pawn, *i.e.*, 2. B × P, P × B. 3. P to R7, Kt to B2. Of course if Black moves his K in reply to Kt × P (ch), then White takes the Kt and plays B to R5. White could not play 4. B × P, to which Black would reply with Kt to B2, and however White may play Black must gain a P, and by giving up his Kt for the other P draw the game.

After 1. Kt × P (ch), Kt × Kt. 2. B to R5, B to Kt3, White wins by 3. B × P, B to Q sq. 4. B × P, P to K3. 5. P to Kt6, &c.

This ending is a good example of the danger attending Pawns when not on the same colour of square as the Bishop.

Three of our correspondents (Clarence, W. H. L. M., and A. H. E.) have pointed out to us one variation in which, although White is successful in his design, yet he will not be able to force a win—*i.e.*:—

1. Kt takes P (ch)	1. Kt takes Kt
2. B to R5	2. P takes B
3. P to Kt6	3. P takes P
4. P to R7	4. K to Q2
5. P to R8 (Q)	5. Kt to QB3

Now, we must confess that we took it for granted—too readily, perhaps—that with a Queen White could win. On examining the position, we have, however, come to the conclusion that a win cannot be demonstrated for White. We do this in spite of the fact that White has yet some winning chances left if, after capturing the two passed Pawns, he can work his King round the Queen's or King's side so as to press Black to abandon the defence of his Pawns. If feasible, this, at best, is a very tedious process, and beyond the range of that practical analysis within which such examples ought to be confined.

BLACK.

WHITE.

Our award of the prize lay between the three above-named competitors, and, after examination of their respective solutions, we have decided to make the award in favour of Brockelbank. We acknowledge, among others, a specially neat solution from H. Doyle.

SOLUTIONS.

PROBLEM 117, by H. W. SHERRARD.

1. B to Q6	P × B (ch)	or	P to K3
2. K to Q5	any	2. R to Kt3 (ch)	K to K5
3. R to Kt3	mate	3. B to B2	mate

PROBLEM 118, by F. J. LEE.

1. R to Q8	1. K to Q6
2. Kt to B5	Mate

ANSWERS TO CORRESPONDENTS.

**** Please address Chess Editor.*

Correct solutions received from W. Hamahan, Arthur S. Rutter, Clement Fawcett, H. T. Hester, S. Osborne, Stettin.

An opponent wanted for correspondence game.

* An instructive end game.—R. P.

Our Whist Column.

BY FIVE OF CLUBS.

WE have been asked by several readers of KNOWLEDGE to quote our synopsis of the various leads, in such a form that they can be readily studied at a glance and easily remembered. We would invite those who have tried to retain in their recollection the multitudinous leads given in the books heretofore published, to note how simple the Whist leads are when viewed as we have presented them. We venture to say—indeed, we *know*, having tested the matter—that a more perfect knowledge of the leads at Whist can be gained in a week by considering when to lead Ace, King, Queen, and so forth, than in two months at least by the usua method of considering what card to lead from each of the numerous combinations which the cards may present. Moreover it is found in practice that a learner who has followed our method at once picks up the habit of interpreting the leads of others, whereas one who followed the other method is often a long time in passing from a knowledge of what he should lead to the ready recognition (instant recognition, it should be, after a little practice) of the meaning of any given lead.

SYNOPSIS OF THE LEADS IN PLAIN SUITS.

Lead Ace, from Ace, with four or more others, not including King; from Ace, Queen, Knave, with or without others; from Ace, two others (not including King), if you have reason to believe that your partner has strength in the suit; and from Ace one other, whatever this other may be. The last two cases are, of course, forced leads.

After leading Ace, from Ace four or more, follow* with lowest, (unless you adopt Drayson's plan of playing lowest but one if there are more than four others). After leading Ace from Ace, Queen, Knave, follow with Queen if you have not more than one small one of the suit, otherwise follow with Knave. When you lead Ace from Ace two others (forced lead, follow with highest. Lead Ace from Ace, King, and others, when you have trumped another suit, lest your partner should trump your King, to establish a cross ruff.

Lead King, from Ace, King, and others; from King, Queen, and others (unless these others, being more than two, include the Knave); from King two others (forced lead), if you have reason to believe that your partner has strength in the suit; and from King one other (forced lead), whatever that other may be.

After leading King from Ace, King, and others, follow with Ace, unless you hold Knave, in which case you may sometimes—if the state of the score seems to render it advisable—change suit, that you may be led up to and finesse the Knave. After leading King from King, Queen, and others, if King makes, follow with small one, unless you hold Knave also, when follow with Queen (not with small one, because Ace may have been held up). When you lead King from King two others (forced lead) follow with highest.

Lead Queen from Queen, Knave, Ten, with or without others; from Queen, Knave, and one small one (forced lead); from Queen two others, not including Knave (forced lead), if you have reason to believe that your partner has strength in the suit; and from Queen one other (forced lead) whatever that other may be.

After leading Queen from Queen, Knave, ten, follow with Knave, unless you have five or more, when follow with lowest of the Queen, Knave, Ten sequence. After forced lead from Queen two others, if Queen makes, follow with highest.

Lead Knave from King, Queen, Knave, and not less than two others (not including ten); from Knave, ten, nine, with or without others; from Knave and two others (forced lead), and from Knave one other (forced lead).

After leading Knave from King, Queen, Knave, &c., follow with King if you have two small one, with Queen if you have more. After leading Knave from Knave, ten, nine, lead ten if there is only one card below the nine, the nine if there are more. After leading Knave from Knave two others, whatever they may be, follow with highest.

Lead ten from King, Queen, Knave, ten, with or without others; from King, Knave, ten, with or without others; from ten two others, or ten one other (forced leads). After leading ten from King, Queen, Knave, ten, follow with King if you have no small cards, otherwise with Knave. After ten from King, Knave, ten, play a small one. After forced lead of ten play your highest.

Lead nine from King, Knave, ten, nine; and in case of forced lead, from nine two others.

Lead a small card from all suits not considered in the above synopsis. Lead the lowest from four cards, the lowest but one from five or more (the lowest but two from six or more, if you care to adopt Drayson's rule); the highest from three or two small cards.

Note that it can scarcely ever happen that playing the lowest but one or two for the purpose of indicating length, can be mistaken by your partner for a forced lead from two or three small cards, or *vice versâ*.

We have already considered concisely, yet fully, the distinction between trump leads and leads from plain suits (see No. 12).

Observe that, short as the above synopsis seems, considering the multiplicity of Whist leads as usually presented, it would be very much shorter if it dealt only with original leads. For these one may say that all the beginner need learn is summed up in the following:—

Lead Ace from Ace and four others, following with small one; and from Ace, Queen, Knave, with or without others, following with Queen, if you have not more than one small one, otherwise with Knave. Lead King from Ace, King and others following with Ace; and from King, Queen and others, following with small one. Lead Queen from Queen, Knave, ten, following with Knave, unless you have five or more, when play lowest of head sequence. Lead Knave from King, Queen, Knave, and two or more, from Knave, ten, nine, with or without small ones. Lead ten from King, Queen, Knave, ten, and from King, Knave, ten, with or without others. Lead nine from King, Knave, ten, nine. In other cases lead a low one, the lowest if you have only four cards, the lowest but one if you have more.

Let the learner combine with this the general rule, that if he is obliged to lead from a weak suit, he always plays the best card of it, unless he has either Ace, King, or Queen, with two small ones. He now knows nearly all that he need know about leading from plain suits. All that he need at first notice about leading from trumps, is, that he can more safely play a waiting game in that suit, as his good cards in it cannot be lost by trumping; also that he must consider the trump card. The play in trumps is also apt to be modified by considerations depending on the state of the score, the position of the cards in other hands, and so forth.

PEMBRIDGE.—We should be much obliged if you would send us some of your experiences of stupidity (or malignity?) We have still the third of your games in the *Westminster Papers* to quote.

* When we thus speak of second round, we do not wish the reader to forget that the first round may show it to be unadvisable to continue the suit; it may seem better to leave your own suit and ead your partner's, or to lead trumps, &c.

CONTENTS OF No. 132.

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LONDON: FRIDAY, MAY 23, 1884.

CONTENTS OF No. 134.

SENT TO THE BOTTOM.

By RICHARD A. PROCTOR.

(*Continued from page* 342.)

THE most serious defect, however, in our present system of steamship-lighting, does not reside in the obvious weakness of the colour-lights in space-penetrating power, but in their want of significance as signals. As we have said, they scarcely say anything; and a steamship's ordinary fixed signals at night ought to say a great deal.

Yet it is the simplest thing in the world to adopt a method of lighting a ship at night by which not only her true position and bearing but even her distance can be at once known so soon as her signal lights are seen.

I long since suggested a method which seemed excellent to many landsmen, as it did to myself (who had not then crossed the ocean), but which was at once seen by seamen to be inconvenient if not impracticable. It involved the employment of signal lights amidships, which would probably have been ill-seen under average conditions. The plan I am about to suggest is open to no such objection, and I believe to no objection at all except that it requires three signal lights on each side and a good bow light and a good stern light, or if preferred two bow lights and two stern lights one on each side.

What I propose is simply that every steamship should carry three starboard lights, well placed amidships, in this position—

*

*

*

three port lights, similarly placed amidships, but in this position—

*

*

*

all six lights being white; that she should further carry a strong white light in her bows visible only from in front, that is from any point in front of an imaginary horizontal line taken through her amidships line at right angles to her length; and a strong red light astern visible only from points behind the imaginary line just mentioned.

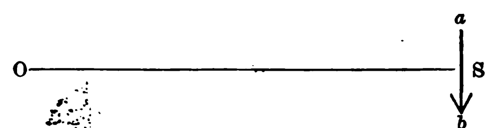
It is easily seen that with this arrangement a steamer's lights would at once indicate her true relative position and bearing. The position of the light-triangle on the side (point to the bows) would show whether her port side or her starboard side were in view; the shape of the triangle would show what angle her length made with the direction line (or line of sight) towards her; and the visibility of the white bow light or the coloured stern light would show whether she were approaching or receding, at that angle with the line of sight.

Suppose for instance that when first any light of a distant ship was seen, only a single white light could be perceived. It would then be known that that ship's course was at the moment directly towards the observer. If on the other hand only a single coloured light could be seen it would be known that her course was taking her directly away. (In such a case, however, the ship from which she was observed would probably be overhauling her, as it would seldom happen that a stern light would be seen *first* in any other way.)

But in general of course the side-light triangle would be visible either when first a ship was sighted or very soon after.

Suppose now the following cases:—

—I.—



1. The stranger is standing directly across the line of sight as shown, O the observer, S the strange ship, and passing from left to right, so that her starboard lights are seen, and both her bow light and her stern light. The lights then would be thus seen—

* Coloured. * * White.

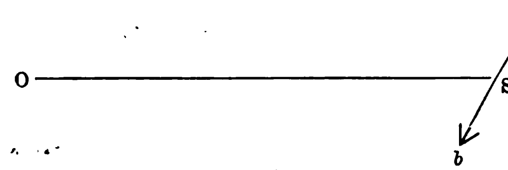
*

*

1. STRANGER TRAVELLING DIRECTLY ACROSS THE LINE OF SIGHT (STARBOARD LIGHTS).

—II.—

2. The stranger is crossing the line of sight thus, where



O is the place of the observer, S the strange ship, *a b* her course. Then her bow light would be visible, not her stern light, and the lights seen would appear thus,—

* * White.

*

*

2. STRANGER STANDING ACROSS THE LINE OF SIGHT, AT AN ANGLE OF 60°, APPROACHING (STARBOARD LIGHTS).

—III.—

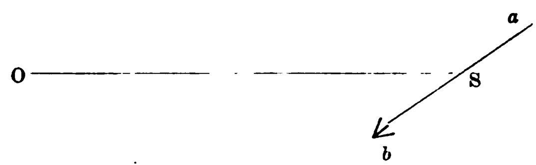
3. The stranger is crossing the line of sight thus (O and S as before),—

Her stern light would be seen, not her bow light, and her lights would appear thus,—

3. Stranger standing across the line of sight at an angle of 60° receding (starboard lights.)

—IV.—

4. The stranger is crossing the line of sight thus,—

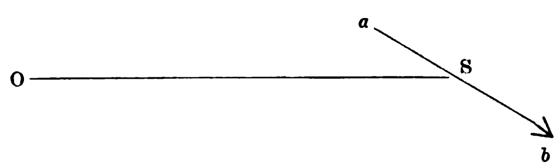


Her bow light would be seen, not her stern light, her side lights thus,—

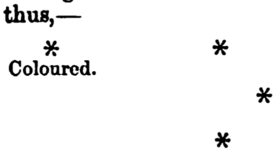
4. Stranger standing across the line of sight at an angle of 30° approaching (starboard lights).

—V.—

5. The stranger is crossing the line of sight thus,—



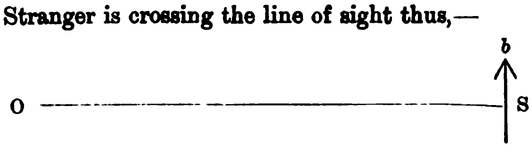
Her stern light would be seen, not her bow light; her side lights thus,—



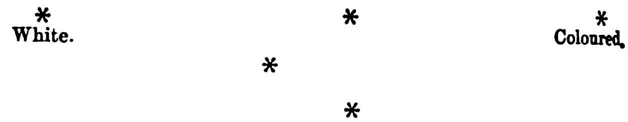
5. Stranger standing across the line of sight at an angle of 30° receding (starboard lights).

—VI.—

6. Stranger is crossing the line of sight thus,—



at a right angle, and from right to left. Both bow light and stern light would be in view, side lights thus,—



6. Stranger standing directly across the line of sight (port lights).

—VII.—

7. Stranger crossing the line of sight thus,—

Her bow light would be visible, not her stern light; her side lights thus,—

7. Stranger standing across the line of sight at an angle of 60° approaching (port lights.)

—VIII.—

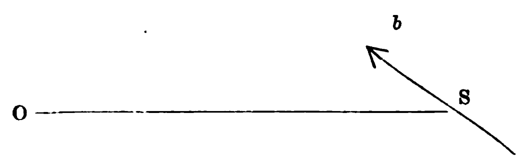
8. The stranger is crossing the line of sight thus,—

The stern light would be seen, not the bow light; her side lights thus,—

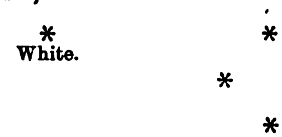
8. Stranger standing across the line of sight at an angle of 60° receding (port lights).

—IX—

9. The stranger is crossing the line of sight thus,—



The bow light would be seen, not the stern light; the side lights thus,—

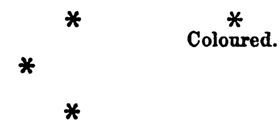


9. Stranger standing across the line of sight at an angle of 30° approaching (port lights).

—x.—

10. The stranger is crossing the line of sight thus,—

Her stern light would be seen, not her bow light; her side lights thus,—



10. Stranger standing across the line of sight at an angle of 30° receding (port lights).

In any position whatever in which the approaching ship might be, her course would be at once recognised by the apparent shape and position of the amidships triangle, combined with the visibility of the bow light or the stern light as the case may be. There would be no question of nice or delicate observation, or of signalling,—though suitable signalling (by means of a powerful foghorn) of the course which each ship was about to keep, would be most useful later. So soon as the lights were clearly seen the veriest landlubber could see how the stranger's hull was situate, as readily as the best seaman on board.

(*To be continued.*)

ELECTRO-PLATING.

V.

By W. Slingo.

Electro-typing, that species of electrical deposition to which brief reference was made in the previous paper, is an art which has been largely and successfully cultivated. No conception can be formed of the quantity of copper so deposited year by year, or of the more than thousand and one objects for which the system is adopted. Notwithstanding, however, the large experience and practice of the past, recent demands are calling forth further advancements. Recently an electrolytic bath was constructed to hold some 70,000 gallons of solution, and capable of taking a type or copy of a statue of the most colossal dimensions. Obviously the amateur will not commence by attempting large or difficult works. His best course would be to start with small and simple articles. Coins and medals in good condition and in deep relief afford excellent practice. As it is manifestly impossible to take a copy of both sides in one piece, it is better to attempt one at a time than to try copies of the two sides simultaneously. There are several methods of copying the above-mentioned articles available. One is to obtain a deposit direct from the metal. This is accomplished by attaching a copper wire to the coin, either by soldering on the back or twisting firmly and securely round the rim. The side which is not to be taken, and which has been called the back, is well coated with an insoluble insulating material, such as wax or gutta-percha. The former may be applied by dipping the back of the coin two or three times in a shallow vessel containing the melted wax. The application of a percha coating is, perhaps, a trifle more troublesome. The copper wire having been fixed, the coin is fitted, face downwards, in a hole previously cut in a piece of wood or a stout piece of cardboard. The exposed surface of the metal is then either heated or coated with an indiarubber or gutta-percha solution, made by dissolving the insulating substance in bisulphide of carbon, which evaporates very quickly, and which may be obtained at any druggist's. It is to be observed that the bisulphide is a volatile and highly inflammable liquid, and should, therefore, be kept at a respectable distance from a flame.* A piece of gutta-percha is then warmed and rolled into a ball, which is placed on the centre of the coin, and worked out till the whole coin is covered. A weight is then placed on the percha and kept there till it is thoroughly set. On raising the coin the face to be copied is coated with plumbago, brushing it into the crevices with a soft brush, and rubbing the smooth surfaces with the finger. Thus prepared, the coin is placed in the bath, and a coating of copper deposited on the plumbagoed surface. When the coating is sufficiently thick, it is separated from the coin and exhibits a reverse representation of the picture. Should there be any imperfection, it arises in all probability from a faulty plumbago surface. When the bath is small, the surface of the coin should be parallel with the anode; but of this more anon. To obtain a real or direct representation of the coin, the coating or mould is insulated on the back, plumbagoed on the face, and attached to a copper wire immersed in the solution.

This method affords instructive practice; but a more expeditious one is to obtain a reverse mould composed of wax, gutta-percha, plaster of Paris, fusible alloy, or some other available material.

To obtain a wax mould, a little oil is rubbed over the surface of the coin to prevent the wax sticking, a strip of stout paper about an inch wide bent round the edge and fastened with a little sealing-wax, the coin is warmed, and then into the receptacle made by the paper rim some white wax which has been heated to a trifle above the melting point is poured. The coin and wax are then put in a cool place until the latter is quite set, which generally takes several hours. When cool the wax is removed, and a piece of copper wire bound round the edge for purposes of connection. The face of the mould is then carefully and thoroughly plumbagoed, the plumbago making contact with the binding wire. It is then placed in the bath. As the plumbago is the conducting material, care is taken that it is not placed anywhere where the copper is not required. Should it get on the back, some difficulty may be experienced in separating the mould from the electrotype. In the event of the plumbago getting where it is not wanted, it should be covered with a little insulating varnish. Copper which has found its way to the back of the mould may, however, be broken off, or, if that is impracticable, as is frequently the case, a file may be used to effect the necessary separation.

Plaster-of-Paris makes very good moulds if it is of the finest description. After slightly oiling the coin, a paper rim is bent round it, as in the process of making a wax mould. Plaster is mixed with water to about the consistency of treacle, and a little of it brushed carefully over the surface of the coin, so as to ensure a perfect representation. The receptacle is then speedily filled with the plaster, which soon dries, and forms a good solid mould. It is, however, porous, and should be saturated with wax, which

* A little bisulphide of carbon on being burnt in a saucer on the hob readily extinguishes a chimney fire by forming, in combination with the oxygen of the air, carbonic and sulphurous anhydrides, gases which do not support combustion.

is accomplished by placing the mould in melted wax, back downwards. The ivory-like surface assumed by the plaster and wax shows plainly when the wax has done its duty. The mould is then bound with wire, and plumbagoed as before, and afterwards immersed in the bath.

A good mould is obtained by using a mixture composed of two parts of gutta-percha to one of marine glue, which is cut up into very small pieces and then heated gently. The compound is well stirred to ensure thorough mixing. A thin brass or copper rim having been fixed round the edge of the coin, a warm, soft ball of the compound is placed on the centre and worked outwards to facilitate the exclusion of air bubbles. A gradually increasing pressure is then applied, and after about two hours, the mould, being cool, may be removed with a gimblet. The slight contraction due to the cooling of the compound renders its removal easy. This mould, after being wired, requires, like the others, to be plumbagoed.

Of fusible alloys there are several, but mention need only be made of one, which is composed of eight parts by weight of bismuth, five of lead, and three of tin. The metals should be well mixed and made as homogeneous as possible, the fusing point of the alloy being about 212° F. To obtain a mould a little of the mixture is poured on a stone slab, the surface skimmed with a card, and the coin dropped on it. When cold, the coin is removed, the back of the mould varnished to prevent the deposition of copper there, and the face plumbagoed, a wire being attached for the purpose of connection. Ordinary coins are never "undercut," but when such a surface is to be copied, special elastic moulds are made, unless it is preferred to make the electrotype in two or more pieces. A serviceable elastic mould is made by breaking a quantity of the best fine glue into small pieces, and soaking them in just enough cold water to cover them. When thoroughly dissolved, any superfluous water that may be present is decanted. The gelatinous mixture remaining is then heated with one-fourth the quantity of dissolved glue to nearly the boiling-point of water. The function of the treacle is to prevent drying and consequent shrinking. To obtain a mould, the surface of the medallion is oiled and a paper rim fastened round the edge. The mixture is poured in while hot, and stood aside for some hours to dry. The paper is then removed, and the mould very carefully detached. Its face is plumbagoed and its back varnished with the india-rubber solution. To prevent as far as possible the absorption of water by the mould (and its consequent swelling) a concentrated copper solution must be employed.

HOW TO GET STRONG.

(*Continued from p.* 323.)

FROM KNEE TO TOES.

HOWEVER absurd some of the common notions about shapeliness may be—as for instance that a small hand or foot is to be admired merely because small, though possibly too small and altogether ill-shaped—there is good reason for the common prejudice in favour of a well developed calf (or preferably a pair). Although footmen and ballet-dancers shame most of us as regards this particular development, and yet are not the most esteemed products of civilisation, there can be no doubt that the shapely calf indicates racial advance. The lower races of savage man are calf-less. The higher savage races have very queer calves, to say the least of it. In civilised races the lower types have the worst calf development. It is only in the highest civilised races, and in the best specimens of these races, that we find the shapely calf shown in Greek sculptures.* I take back indeed what I said just now, or seemed to say, about footmen and ballet-girls. Their development of calf is great, but to the artistic eye unsatisfactory. There is something manifestly wanting in intellectuality about this portion of their understandings. Their limbs below the knee are as out of proportion as a blacksmith's arm,—and though as I wander through galleries of modern sculpture I am sometimes led to suspect that artists have occasionally taken such calves as footmen and ballet-dancers possess for their models, I am sure no sensible sculptor would take the legs of John Thomas or of Signorina Toanheli as samples of masculine or feminine perfection of form, any more than he would copy the arm of a blacksmith in fashioning the statue of an Apollo.

I do not advise a series of energetic exercises for the calf; but I do strongly advise that so much exercise should be systematically given to it that it may be developed in due proportion. Where the calf is small and weak, it may with advantage receive a larger share of attention.

The first exercise I would suggest is a very simple one, but instructive as showing what is the work for which the calf muscles are intended. But the exercise is also good for the knees. (I may remark in passing that so many of the exercises I have suggested for the thighs and body, and many of those which I am about to suggest for the leg below the knee, are good for making the knees supple, lissome, and strong, so that I do not think it necessary to describe special exercises for this purpose.) Walk a hundred steps (in your bedroom or sitting-room, if you like), on the toes, letting the body sink well at each step as the heel draws near the ground, and be springily raised as the heel rises. There is no exercise in this, you probably think, as you take the first ten or twelve steps; but by the time you have taken a hundred steps you will find your knees beginning to be rather tired (if you have kept up properly the alternate rise and fall of the body). The calves have not apparently been very much tried. Yet as you rest after the exercise, a certain something suggests that the calves have been "got at."

It is the rising and sinking of the heels which has given the calves work. Therefore, attend now especially to this particular movement. Standing with the head well up, chest out, and shoulders thrown back, the knees set as if you were about to begin the toe-touching exercise already described, and the feet pretty close together but not touching, and *the toes turned out*, steadily rise on the toes, after the ballet-dancer's fashion, as high as you can. Then slowly lower your heels till they touch the ground,—keeping the knees all the time well back. Steadily rise again till you are standing on tip-toe. Sink again. As you next rise, slacken your knees a bit and lower the body till you can feel the calf-muscles, and note how they tauten as the body is being raised. Resume the straight-knee condition, and steadily rise and sink from the toes as before. Repeat as long as convenient,—forty to fifty times the first day, but later you may take several hundreds. There is no exercise which more directly affects the calves than this. "A gentleman of our acquaintance," says Blaikie, "of magnificent muscular and vital developments, was not satisfied with the girth of his calves, which was $14\frac{1}{4}$ inches. At our

* We may recognise the significance attached to the calf as one of the most characteristically human developments in the old custom referred to in Tennyson's "Princess" in words which many readers have found perplexing—"Married to a booted calf." Obviously the calf, well booted, was regarded as aptly symbolising the man himself.

suggestion he began practising this simple raising and lowering of the heels. In less than four months he had increased the girth of each calf one whole inch. When asked how many strokes a day he averaged, he said, 'from fifteen hundred to two thousand,' varied some days by his holding in each hand during the process a 12 lb. dumb-bell, and then only doing one thousand or thereabouts. The time he found most convenient was in the morning on rising, and just before retiring at night. Instead of the work taking much time, seventy a minute was found a good ordinary rate, so that fifteen minutes at each end of the day was all he needed. But this was a great and very rapid increase, especially for a man of thirty-five; far more than most persons would naturally be contented with, yet suggestive of the stuff and perseverance of the man who accomplished it."

However the exercise is a rather slow one, though effective enough. Many others are available which if they do not act so directly on the calf are better for the general development of the leg.

Quick walking with hard pressure of toes and soles against the ground will be found very capital work for the feet and calves. Walking up hill in the same way is even better. Blaikie strongly recommends running on the fore part of the feet. But as running on this part of the feet only, is not good running, and it is a pity to spoil the style, I would simply recommend the careful avoidance of flat-footed running. It is neither well to run with too much foot action nor with too little as when running flat-foot. By aiming at a style between the two, you get the most effective form, and at the same time develop well the muscles of the calf and of the upper thigh. Note that in the actual drive back with the lower limb by which forward propulsion is accomplished, the heel should come first to the ground (whatever runners may say, and doubtless believe, about the heel not touching the ground at all), then the mid-sole, and finally the ball of the foot and the toes, the last forward effort coming from the front of the foot. In this part of the leg stroke the knee should be well back. If special attention is directed to this part of each stride, the calves will get an extra allowance of work; while the running style will be perceptibly improved. But if you care to get a quick style, avoid over-springiness. The spring of the gait should only suffice to reduce the vertical impact to a minimum, taking off the jar which is perceptible in flat-footed running; but any springiness by which an unnecessary rise and fall of the body are produced is a fault of style, and involves more or less waste of strength or loss of velocity or both.

Blaikie further recommends hopping, which is no doubt the most effective of all exercises for enlarging and hardening the calf. But skill in hopping is not worth very much in ordinary life. The accomplishment is one which most persons would prefer to avail themselves of in private,—and even in a bedroom persistent hopping might be regarded as a rather objectionable practice. The only advantage of hopping is that it tells very quickly and tires very soon.

Jumping is far more satisfactory, being in the first place much more useful, tending to strengthen and improve more limbs and muscles than hopping, and having a saner aspect in public. You can often get the chance of taking a good run across a heath or common presenting abundant opportunities for light leaping.

GAS IN PARIS.—The dividend of the Parisian Company for Lighting and Heating by Gas for 1883 is at the extremely liberal rate of 27½ per cent. per annum. Although this was an extremely satisfactory result, it was scarcely so good as that obtained for 1882.

COINCIDENCES AND SUPERSTITIONS.

BY R. A. PROCTOR.

(*Continued from page* 327.)

I HAD intended to pass to the consideration of those appearances which have been regarded as ghosts of departed persons, and to the study of some other matters which either are or may be referred to coincidences and superstitions. But my space is exhausted. Perhaps I may hereafter have an opportunity of returning to the subject—not to dogmatise upon it, nor to undertake to explain away the difficulties which surround it, but to indicate the considerations which, as it appears to me, should be applied to the investigation of such matters by those who wish to give a reason for the belief that is in them.

At present I must be content with indicating the general interpretation of coincidences which appear very remarkable, but which nevertheless cannot be reasonably referred to special interpositions of Providence. The fact really is that occasions are continually occurring where coincidences of the sort are *possible*, though improbable. Now the improbability in any particular case would be a reasonable ground for expecting that in that case no coincidence would occur. But the matter is reversed when a great multitude of cases are in question. The probable result then is that there *will* be coincidences. This may easily be illustrated by reference to a question of ordinary probabilities. Suppose there is a lottery with a thousand tickets and but one prize. Then it is exceedingly unlikely that any particular ticket-holder will obtain the prize—the odds are, in fact, 999 to 1 against him. But suppose he had one ticket in each of a million different lotteries all giving the same chance of success. Then it would not be surprising for him to draw a prize; on the contrary, it would be a most remarkable coincidence if he did not draw one. The same event—the drawing of a prize—which in one case must be regarded as highly improbable, becomes in the other case highly probable. So it is with coincidences which appear utterly improbable. It would be a most wonderful thing if such coincidences did not occur, and occur pretty frequently, in the experience of every man, since the opportunities for their occurrence enormously outnumber the chances against the occurrence of any particular instance.

We may reason in like manner as to superstitions. Or rather, it is to be noted that the coincidences on which superstitions are commonly based are in many instances not even remarkable. Misfortunes are not so uncommon, for instance, that the occurrence of a disaster of some sort after the spilling of salt at table can be regarded as surprising. If three or four persons, who are discussing the particular superstition relating to salt-cellars, can cite instances of an apparent connection between a misfortune and the contact of salt with a tablecloth, the circumstance is in no sense to be wondered at; it would be much more remarkable if the contrary were the case. There is scarcely a superstition of the commoner sort which is not in like manner based, *not* on some remarkable coincidence, but on the occasional coincidence of quite common events. It may be said, indeed, of the facts on which nearly all the vulgar superstitions have been based, that it would have amounted to little less than a miracle if such facts were not common in the experience of every person. Any other superstitions could be just as readily started, and be very quickly supported by as convincing evidence. If I were to announce to-morrow in all the papers and on every wall that misfortune is sure to

follow when any person is ill-advised enough to pare a finger-nail between ten and eleven o'clock on any Friday morning, that announcement would be supported within a week by evidence of the most striking kind. In less than a month it would be an established superstition. If this appears absurd and incredible, let the reader consider merely the absurdity of ordinary superstitions. Take, for instance, fortune-telling, by means of cards. If our police reports did not assure us that such vaticination is believed in by many, would it be credible that reasoning beings could hope to learn anything of the future from the order in which a few pieces of painted paper happened to fall when shuffled? Yet it is easy to see why this or any way of telling fortunes is believed in. Many persons believe in the predictions of fortune-tellers for the seemingly excellent reason that such predictions are repeatedly fulfilled. They do not notice that (setting apart happy guesses based on known facts) there would have been as many fulfilments if every prediction had been precisely reversed. It is the same with other common superstitions. Reverse them, and they are as trustworthy as before. Let the superstition be that to every one spilling salt at dinner some great piece of good luck will occur before the day is over; let seven years of good fortune be promised to the person who breaks a mirror; and so on. These new superstitions would be before long supported by as good evidence as those now in existence; and they would be worth as much, since neither would be worth anything.

THE PATENT ACT OF 1883.

By Polyglot.

(*Continued from page* 326.)

YOU will next have to resolve whether you will file what is called a "provisional specification" first, and a "complete" subsequently, or a "complete" in the first instance. I will describe both procedures and advise the first. The why will appear by-and-by.

The provisional specification (to be filed in duplicate) must be a clear, concise, and fair statement, sufficient to enable any person skilled in the art or trade to which the invention relates to identify the invention, but not necessarily to practise it. If the invention relates to a machine or the like, drawings may be furnished, but are not essential; if to a chemical compound, proportions need not always be given, nor is it always necessary, when several similar substances may be used, to specify them all; a generic name, such as "an alkali" or "a resinous material," will often be sufficient *unless* the invention consists just in the use of *one* such substance where *others* have previously been used. Supposing, for instance, that you employ a chloride of sodium where chloride of calcium has been used before, and that this is the gist of your invention, then it will not do merely to say "an alkaline chloride," as this is too vague to meet the case. In doubtful cases it is preferable to make your statement needlessly full to making it unduly meagre. An important consideration is, that nothing that does not appear in the provisional specification can ultimately be claimed, though you need not afterwards claim the whole subject-matter of your description.

Above all, do not endeavour, either in the provisional or in the complete specification, to hide the nature of your invention by dubious or ambiguous wording. A fair and honest description in plain words is requisite, nay, essential. If you acquire a piece of ground, and have a map annexed to the title-deed, you will insist upon it that the map shall clearly define the territory that is to be yours; and so will your neighbours, lest quarrelling should ensue. The specification is the map which defines the ground to be covered by your patent. Lenient as courts as a rule are, and almost tenderly careful of the rights of an honest inventor, they have a way of most mercilessly "coming down" upon a man who shows a disposition to trickery in the matter.

When your declaration is ready and declared to before a magistrate, a justice of the peace or a commissioner to administer oaths (a solicitor), or, if you are not in England, before an English consular officer, you will send it and your specification to the patent office. The declaration must be stamped with a £1 stamp, and it is of no use to send money instead. If your documents are informal or otherwise wrong, the patent office will call upon you to amend them. If they are all right, the patent office will notify you of their acceptance. The office, however, takes no responsibility upon itself, the acceptance only means that the papers seem in order, but, nevertheless, your invention may be old, bad, futile, or irrevelant, or your specification insufficient to protect you, without your having any redress against the office. If, however, at the time at which your application is before the office another similar invention is brought in, or is embodied in an earlier specification, the office will notify both applicants, but will not tell either in what the other's inventions consists. This can only be found out at the opposition stage, of which more anon.

When the provisional specification has been accepted, you may, if you chose, at once file your complete specification, but are not bound to do so until nine calendar months from the date of application—that is, from the day on which your application was filed.

The complete, like the provisional, must be in duplicate; whilst, however, the provisional bears no stamp, one copy of the complete specification must bear a £3 impressed stamp.

The complete specification must fully set forth what the invention consists in, and in what manner it may be carried into practice. It must be accompanied by drawings—if these are required—to render the invention fully intelligible, and it must in every respect be a fair, true, and ample statement, enabling any one who is conversant with the trade or art to which the invention relates to practise it without any guide beyond ordinary skill, plus the specification. This does not mean that in a machine, for instance, all the strains must be calculated and the dimensions of the several parts laid down; or, for instance, in a patent for making paper it would be sufficient (if in accordance with fact) to say that the pulp is to pass through a "knotter," as every papermaker is supposed to know what a "knotter" is. The main object must be not to omit any fact that is material to the proper understanding of what the inventor really does for the purpose of attaining that result which he seeks to accomplish. To make elaborate statements about how much profit can be made by using the invention, about the beauty of its effect, or, as one or two over-fervent enthusiasts have done, to describe how wife and children "took it," is worse than useless. If you want to give a cook a recipe for a pudding, you do not begin with a lecture upon the chemical changes produced in wheaten flour by panification. Likewise, a specification must not be a treatise, only a full and honest description.

It is wisest to make the complete specification an amplication of the provisional, adding such slight amendments in the invention as practice may have proved desirable. And this possibility of adding or amending is the reason why it is best at first to file a provisional only. Because, though no essential feature must be altered or added, yet trifles go so far in making or marring new inventions, that

in the period intervening between the filing of provisional and complete, details may be devised which are likely greatly to increase the utility of the invention; and, if the provisional be well drawn, much can be introduced and validly covered by the same patent that would otherwise require a new patent; or sometimes even, if omitted would render the whole nugatory.

To make that which is new intelligible, it is in the majority of cases necessary to describe much that is old. Photography being an art well known to many, I will draw upon it for an illustration. We may assume that some one invents yet another new focussing arrangement. It is obviously desirable to describe the whole camera, and to show how the new gearing is combined with the old parts. There is no need to expatiate on the focal length of the lenses or on the size of the tripod, but probably the bellows-body, the back, and other parts will have to be mentioned and shown by drawings, and each piece of which the new motion consists must be described. To enable any one to render himself an account of what is supposed to be new, one or more "claims" are appended to the description.

Claims are a specific statement indicating precisely what is new. Say the focussing gear above spoken of consists of a rack, a slotted guide, and a toothed wheel, with a thumb-screw, then the claim might read somewhat as follows:—"I claim the focussing gear for a photographic camera, consisting of a rack, a slotted guide, and a toothed wheel with a thumb-screw, all substantially as described and shown in the drawings."

(To be continued.)

THE ROTATION PERIOD OF MARS.

WE quote the following letter from *Nature* for May 15, chiefly for the purpose of replying to it, and correcting statements based on incomplete information:—

"Notwithstanding his comparatively small diameter and slow axial motion, the planet Mars affords special facilities for the exact determination of the rotation period. Indeed no other planet appears to be so favourably circumstanced in this respect, for the chief markings on Mars have been perceptible with the same definiteness of outline and characteristics of form through many succeeding generations, whereas the features such as we discern on the other planets are either temporary atmospheric phenomena or rendered so indistinct by unfavourable conditions as to defy lengthened observation. Moreover, it may be taken for granted that the features of Mars are permanent objects on the actual surface of the planet, whereas the markings displayed by our telescopes on some of the other planetary members of our system are mere effects of atmospheric changes which, though visible for several years, and showing well-defined periods of rotation, cannot be accepted as affording the true periods. The behaviour of the red spot on Jupiter may closely intimate the actual motion of the sphere of that planet, but markings of such variable, unstable character can hardly exhibit an exact conformity of motion with the surface upon which they are seen to be projected. With respect to Mars the case is entirely different. No substantial changes in the most conspicuous features have been detected since they were first confronted with telescopic power, and we do not anticipate that in future ages there will be any material difference in their general configurations. The same markings which were indistinctly revealed to the eyes of Fontana and Huyghens in 1636 and 1659, will continue to be displayed to the astronomers of succeeding generations, though with greater fulness and perspicuity owing to improved means. True there may possibly be variations in progress as regards some of the minor features, for it has been suggested that the visibility of certain spots has varied in a manner which cannot be satisfactorily accounted for on ordinary grounds. These may possibly be due to atmospheric effects on the planet itself, but in many cases the alleged variations have doubtless been more imaginary than real. The changes in our own climate are so rapid and striking, and occasion such abnormal appearances in celestial objects that we are frequently led to infer actual changes where none have taken place; in fact, observers cannot be too careful to consider the origin of such differences and to look nearer home for some of the discordances which may have become apparent in their results.

"The rotation period of Mars has been already given with so much precision that it may seem superfluous to rediscuss the point, but it is very advisable to see whether recent observations confirm the values derived from former results. The 'Hour-glass' or 'Kaiser Sea,' which is admittedly the most prominent mark on the planet, is a very suitable one for comparisons to find the intervals of rotation. Early in 1869 I saw it with a 4½-inch refractor as it passed the central part of the disk. On Feb. 2, 1869, it was central at 10h., on Feb. 4 at 11h., and on Feb. 5 at 11h. 30m.

"I observed the same object in February of the present year with a 10-inch reflector (power 252), and noted it crossing the planet's central region at the following times:—

1884	h. m.
Feb. 14	5 55
„ 15	6 35
„ 19	9 5
„ 22	11 4

I have combined my observation of February 4, 1869, with that of February 14, 1884 (as I regard this pair as the best obtained), to ascertain the rotation period. The interval includes 5,487d. 18h. 55m. = 474,144,900 seconds. Correcting this for the difference in longitude between Mars and the earth at the two epochs and for defect of illumination (there is no necessity to apply any correction for equation of light, as the apparent diameter of the planet on the dates selected for comparison was about 16", and hence the distances were nearly the same), I find the time of rotation resulting from the discussion of these observations to be

h. m. s.
24 37 22·34 (5,349 rotations),

which is in satisfactory agreement with the periods computed by Kaiser, Schmidt, and Proctor from a much longer series of observations. In order to exhibit the small difference between the period now computed and those resulting from some of the best modern determinations, I give the following summary:—

		h. m. s.	
	J. H. Mädler ...	24 37 23·8	'Ast. Nach.' 349.
1864,	F. Kaiser	24 37 22·62	'Ast. Nach.' 1468.
1866,	R. Wolf	24 37 22·9	'Ast. Nach.' 1623.
1869,	R. A. Proctor...	24 37 22·735	'Mont. Not.' vol. xxix., p. 232.
1873,	F. Kaiser	24 37 22·591	'Annalen der Leidene Sternwarte,' vol. iii., p. 80.
1873,	J. F. J. Schmidt	24 37 22·57	'Ast. Nach.' 1965.
1884,	W. F. Denning	24 37 22·34	

It is obvious that Mädler's period of 24h. 37m. 23·8s. is about one second too great. If we take a mean of the other six values (all within 0·6s. of each other) we get

h. m. s.
24 37 22·626

which may be fairly regarded as a very near approximation to the true sidereal rotation period of Mars.

"The computations of Kaiser, Schmidt, and Proctor are severally based on very long periods, the comparisons being modern observations with those of either Huyghens or Hooke during the last half of the seventeenth century. It is unfortunate, however, that there is some question as to the correct identification of the spots depicted in some of the ancient drawings. The representations by Hooke on March 2, 1666 (old style), at 12h. 20m. and 12h. 30m., also those by Huyghens in 1659, 1672, and 1683 give a large irregular spot, extending in a north and south direction, which can only be identified as the 'Hourglass' or 'Kaiser Sea.' It would appear, however, that this interpretation is incorrect in certain cases, for the several drawings do not only show disagreements with each other, but also when compared with modern observations originate discordances of period, small it is true, but still too large to be attributed to simple errors of observation. No doubt the period which approaches nearest to the truth will become apparent from future observations, though it can hardly admit of definite settlement for many years, inasmuch as the differences between the several times of rotation as above deduced are very insignificant, and must so closely accord with the real period of the planet that the errors such as exist must be allowed to accumulate over a lengthened interval before they will become distinctly manifested. A comparison extending over fifteen years is insufficient for the purpose, for a computed time of rotation, erroneous to the extent of one-tenth of a second, will still, at the termination of such a period, answer to the positions of the markings to within nine minutes of time. It

is to be remarked that Mr. Marth, whose opinion is entitled to great weight, has, for some time, adopted the period of 24h. 37m. 22·626s. for the rotation of Mars. This corresponds to a daily rate of 350°·8922, and forms the basis of his computations on his 'Ephemerides for Physical Observations of Mars,' annually published in the 'Monthly Notices.' W. F. DENNING."

[Mr. Denning omits to notice that in a paper contributed to the proceedings of the Astronomical Society in 1873 I indicated errors if computation in Prof. Kaiser's treatment of the long period between Huyghens in 1659 and Kaiser's own observations in 1864 and later. Kaiser appears to have counted the years 1700 and 1800 as leap-years, and to have made a further mistake as to the effect of change of style. When correction is made for these errors —as to the reality of which no question can exist (for Kaiser indicates the number of days he counted in the long period, and they are three too many) his value of the rotation-period is changed into a value practically identical with mine. For each day counted in error caused an extra Martian rotation to be counted, giving in all three rotations of Mars over the right number, and the difference, between three Martian days and three terrestrial ones,—or three times 37m. 23s.,—has to be divided among nearly 89,000 rotations —giving ·076 sec. to be added (obviously) to Kaiser's value. This gives 24h. 37m. 22·7s. (I have long since given up the idea that the second decimal figure can be given.) Prof. Newcomb, of Washington, told me in 1874 he had checked my examination of Kaiser's computation, and found it correct.

Mädler's period, a second too long, shows how little reliance can be placed on observations covering only ten or twelve years or so. Mr. Denning's value is a marvellously good shot for so short a range in time as he employed. But it cannot (from the nature of the case) enter into competition with any of the long-distance ones. The mean of the other long-distance values (Wolf's and Schmidt's) is, oddly enough, 24h. 37m. 22·735, exactly that which I gave in 1869. But as I have said, all I would now maintain is that the value is 24h. 37m. 22·7 sec.,—that is, that the seconds lie between 22·65 and 22·75.

Mr. Marth probably adopted his fellow-countryman's estimate (Kaiser's in 1864), and not having followed later inquiries, has failed to notice the errors discovered in Kaiser's computation. I am not aware that he has independently examined the matter at any time.

Oxford, May 17. RICHARD A. PROCTOR.]

THE FACE OF THE SKY.

FROM MAY 23 TO JUNE 6.

BY F.R.A.S.

MALGRÉ the passage of (what we may call) the official spot-maximum, beautiful groups continue to appear upon the surface of the Sun. The aspect of the night sky may be gathered from Map VI. of "The Stars in their Seasons." There is, however, no real night at all now in the British Islands. Mercury is a morning star, and about the time that these notes terminate may be caught above a point of the hozizon between E. by N. and E.N.E., just before sunrise. Venus is now by far the most brilliant and conspicuous object in the sky, and her crescent forms an object of daily-increasing size and beauty in the telescope. Mars, Jupiter, Saturn, and Neptune have left us for the season; but Uranus may be picked up to the west and north of β Virginis (Zodiacal Map, p. 165). He must, though, be looked for the moment the twilight deepens sufficiently. One occultation of a star only is visible during the next fortnight. It occurs on May 30, when 16 Sextantis, a 6th magnitude star, will disappear at the Moon's dark limb at 10h. 19m. p.m. at an angle of 112° from her vertex. It will reappear from behind her bright limb at 11h. 17m. p.m. at a vertical angle of 285°. At noon to-day the Moon is in Aries, out of which constellation she passes into Taurus about half-past two o'clock to-morrow morning. It is not until 10 a.m. on the 26th that she enters the northern strip of Orion, which is conterminous with Taurus and Gemini; travelling through it in some 11 hours, and about 9 p.m., crossing its eastern boundary into Gemini. She remains in Gemini until 10 a.m. on the 28th, at which hour she enters Cancer. She quits Cancer for Leo at 11 o'clock on the night of the 29th, through which latter constellation she travels until at 1 a.m. on the 31st, when she descends into Sextans, re-emerging in Leo at 5 o'clock the same afternoon. She crosses from Leo into Virgo at 7 o'clock in the evening on June 1st, and her passage across this great constellation occupies until 5 a.m. on the 5th. At that hour she quits Virgo for Libra, where she remains until 6 a.m. on the 7th.

Reviews.

SOME BOOKS ON OUR TABLE.

Biogen. A Speculation on the Origin and Nature of Life. By Professor ELLIOTT COUES. (London: Trübner & Co. 1884.)—Here is the old scholastic figment of a "vital principle" warmed up again, after we fondly imagined that it was still and cold for ever. Why a vital principle should be more needed to account for the phenomena of life than, say, a "watch principle" for the movement of the seconds-hand of a watch, Mr. Coues does not condescend to inform us. He also is good enough to tell us that he *feels* that so-and-so is true; ergo it must exist. But Mr. John Hampden feels that the earth is as flat as a pancake, and a visit to Hanwell or Colney Hatch would reveal the existence of numerous gentlemen who each feels that he is the prophet Habakkuk, or a glass bottle, or the Rumtifoozle, as the case may be. While appreciating the spirit and feeling which have prompted the utterances of Professor Coues, we cannot congratulate him on his contribution to science.

A Lump of Iron. From the Mine to the Magnet. By ALEXANDER WATT. (London: A. Johnston. 1884.)—A popular and well-written account of the most important of the metals in all its aspects—mineralogical, metallurgical, and mechanical. The processes of smelting, ironfounding, steel-making, &c., up to the conversion of the steel into a magnet, are described in the clearest possible way; there is an interesting chapter on meteorites, as the source of meteoric iron; and a kind of appendix of interesting notes on matters connected with the subject of the book. Mr. Watt's little volume is well worth buying. It is news to us that a piece of wrought-iron was taken out of an inner joint of the Great Pyramid forty-seven years ago. This carries its use back to a very remote age indeed.

Experimental Proofs of Chemical Theory for Beginners. By WM. RAMSAY, PH.D. (London: MacMillan & Co. 1884.)—This really excellent little manual contains a series of exercises in quantitative analysis calculated to familiarise the beginner with the methods and results of the most recent developments of chemical science. Illustrations of every one of the pieces of simple apparatus required are given, together with explicit directions for their manufacture by the student himself. The twelve chapters of which the work consists treat successively of the Measurement of Temperature Pressure and Weight, the Relation of the Volumes of Gases to Temperature and Pressure, Air, Hydrogen Chloride, Water, Ammonia, Atoms and Molecules, Quantivalence, Equivalents of Metals, Specific Heat, "Replacement," and the Periodic Law. Dr. Ramsay's small volume will, doubtless, become a popular text-book in the laboratory—as it deserves to be. We note by the way a little slip or omission on p. 4, where it is stated that, in reading a thermometer, "if the eye is too high the temperature registered will be in excess of the real one," and *vice versâ;* the fact being that this depends upon whether the graduations are between the mercury and the eye—or (as is generally the case) behind the quicksilver. In the latter case an elevation of the eye seemingly *depresses* the mercury and makes the instrument read too low.

The Periodic Law. By JOHN A. R. NEWLANDS. (London: E. & F. N. Spon. 1884.)—"If," says Dr. Ramsay, "the elements be arranged in the order of their atomic weights in eight vertical rows, they fall into natural classes, each class containing elements resembling each other in the

nature of their compounds, and in their usual valency." As both Mendelejeff and Lothar Meyer have received more or less credit for this discovery, Mr. Newlands makes a reclamation on his personal behalf in the work now before us, and shows incontestably, by reference to dates, that his own detection of the so-called "Periodic Law" was made in 1864. This and his researches on the atomic weights generally make up a volume, which is sure to be soon found in every chemical library.

About Photography and Photographers, &c. By H. BADEN PRITCHARD, F.C.S. (London: Piper & Carter. 1883.)—This chatty and readable series of short papers and essays seems eminently calculated to develop a taste for photography in any one and every one who may come across it. It may be opened at random, with the certainty of coming across something amusing: for the amateur, the professional photographer, the artist, the sitter, and the tourist will one and all find matter to interest him.

A Popular Treatise on Modern Photography. By GEORGE DAWSON, M.A. (Glasgow: George Mason & Co.)—The reader whom Mr. Pritchard may have filled with a burning desire to commence practical photography forthwith, could scarcely do better than lay out a shilling in Mr. Dawson's pamphlet. It contains the plainest and most explicit directions how to operate both by the wet and dry processes; explaining to the student in the minutest detail not only what to do, but—which is often of equal importance—what *not* to do. It is a capital little book.

The Early Days of the Human Race. By T. FREDERICK J. BLAKER, M.R.C.S. (Brighton: H. & C. Treacher.)—Mr. Blaker tells the story of ancient man in a simple and agreeable way, and his little book may be read, not without profit, as an introduction to larger and more pretentious works on the subject.

Science in the Nursery, or Children's Toys and what they Teach. (London: Griffith & Farran.)—What Dr. Paris did in the days of our grandfathers in his "Philosophy in Sport made Science in Earnest," in connection with toys in vogue during the earlier part of the century, Mr. Erle has essayed to do with regard to those with which the present generation is amused. He tells us in his introduction that his work is a reproduction of a series of lectures addressed "to a rural audience." If this be so, we can only fear that our author's expositions must have gone clean over the heads of his hearers. His science is perfectly sound, and his descriptions of the mechanical and optical principles upon which many of our common toys are constructed leave nothing to be desired—except that the language in which they are conveyed should be a trifle less "fine." Opening the book absolutely at random, we read (p. 288), apropos of "dynamical friction," "It is either shifted over to the particles of the medium, or surfaces, which environs or touches the line of march, producing among them a certain amount of dislocation of arrangement, or, if displacement be more or less successfully resisted by them, then motion to the same amount reappears as its *alter ego*, that is to say, in its other phase of heat." It would be curious to speculate how much of this sort of thing would be assimilated by a gentleman of the agricultural persuasion who had spent his day turnip-hoeing or tan-flaying.

The House of Lords. By SIR JOHN BENNETT.—Save in its sociological aspect, the subject of this funny little tract is rather beyond our scope. Certainly the ex-sheriff does not spare the hereditary Chamber of Legislators, and his *exposé* of that most pitiful and contemptible race the Stuarts, and the contrast he draws between them and Oliver Cromwell is not without historic interest. Somehow, though, we imagine that the House of Lords will last for our time.

PLEASANT HOURS WITH THE MICROSCOPE.

By HENRY J. SLACK, F.G.S., F.R.M.S.

THE wings of insects do not correspond with the anterior extremities of any of the vertebrate animals. Their function of beating against the air to produce the motion of flight is, indeed, the same as that of the wings of the bird, but they are not, like those organs, modifications of fore-limbs. If, as is probable, the earliest insects were inhabitants of the water, some of them would be supplied with external gills, and by successive changes these organs would be modified so as to suit terrestrial life, and, finally, serve for flight. External breathing organs are common in the larval forms of many water insects. They are well furnished with tracheal tubes, and this is the case with the wings of flies, bees, &c., which have been called aërial gills. These tracheal tubes are made firm and hard by deposits of a chitinous material like that of the hard skin and elytra of beetles, and their use as strong supports of the delicate wing membranes has led to their being named *Pterigostia*, or "wing bones." Mr. Newport remarked that there appears no part of the body in vertebrata analogous to the wings of insects, except, perhaps, in the single instance of one of the Saurian reptiles, *Draco volans*, in which a pair of supernumerary organs to assist in locomotion are developed from the sides of the body, and which are formed by the ribs, directed horizontally outwards, and covered over by the skin. In insects that are good fliers the supply of nerves to the wings is very considerable, and so arranged as to produce a perfect co-ordination of their motions.

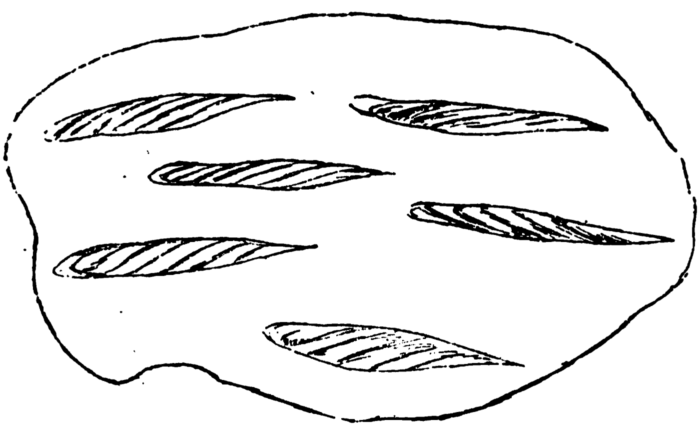
Having thus reached the conclusion that the wings receive many nerve fibres and many tracheal tubes, we may expect that some part of their structure is likely to be connected with sensations, and in the various kinds of flies, bees, wasps, &c., it is probable that some of their peculiar hairs or bristles may receive and transmit vibrations useful to the creatures in avoiding obstacles in their flight.

Between spring and autumn many two-winged insects of grey and blackish hue frequent our houses, and settle on the windows. Few persons notice much difference, except in their sizes, but they comprise several species. All, however, that I have examined have wings covered with a multitude of minute hairs, of simple structure, and springing from a little bulb. The outer rim of the wings exhibits another kind of hair or bristle, and near their base there are long porcupine-quill-like bristles, which, viewed as opaque objects, with a magnification of 200 or 300 linear, are seen to be beautifully fluted. Further down the wing towards the tip, the long bristles cease, and stumpy conical ones appear, also multiple fluted. Each one of these, both long and short, seems capable of motion, as the way they spring out of the bulb bears some resemblance to a ball and socket joint. Near the tip of the wings these stout bristles cease, and on the inner side of the wing there are none, but a fringe of fine delicate hairs, longer than those thickly scattered over the wing membrane. The short bristles may possibly transmit vibrations like the whiskers of a cat. The multitude of small hairs scattered all over the wings must, as they stand out, give the organ a grip upon the air as they strike it slantingly. Their bulbs must also tend to prevent any accidental rent or rift in the thin membrane from extending, and thus they add to its strength.

I am not aware that any one has made a study of these hairs in different orders and genera of insects, but it might repay the trouble. The most curious difference I have

noticed occurs in wasps. The wings of this insect are dotted over with hairs, which a lady to whom they were shown likened to a shoal of little fishes. They are of various sizes, some thin and some stout; but all agree in having a number of ribs making a diagonal pattern, as shown in the annexed sketch. These hairs are not arranged as regularly as those on the wings of a house-fly, and do not show their structure without careful illumination. The bristles on the outer edge of the house-fly's wing have their ribs or grooves arranged lengthwise, and as nearly parallel to each other as their tapering form permits. The wasp wing hairs have their ribs starting from about equal distances all down their sides, which suggests a spiral arrangement. The membrane of this wing is so transparent as to be scarcely visible between the hairs and nervures.

Is the structure of these hairs a modification tending towards butterfly scales? No answer can be given to this question without extensive observations. The wasps are, structurally, in advance of common flies, and Haeckel regards the Lepidoptera—butterflies and moths—as the most perfect class of flies, and the last to develop. Their fossil remains are not traced beyond the tertiary epoch, while Dragon Flies are found in coal. The wings of hive bees



Hairs on Wasp's Wing, ×870.

and hornets do not possess these peculiar hairs. Some of the broadest of the wasps' might almost be called scales; but they have not the quill-like beginnings of the butterflies' plumes, nor are they inserted into similar cavities or sheaths. As all sorts of flying insects will abound in the coming summer, there will be good opportunities for noticing what modification of hairs their wings can show, and if any of my readers become possessed of a new fact, I shall be obliged by an early notice of it.

With reference to the probability that wings were developed from external tracheæ, Sir John Lubbock observes, "That wings may be of use to insects under water, as proved by the very interesting case of *Polynema natans*, which uses its wings for swimming. This, however, is a rare case; and it is possible that the principal use of the wings was, primordially, to enable the mature forms to pass from pond to pond, thus securing fresh habitats, and avoiding in-and-in breeding."

THE PANAMA CANAL.—It is affirmed that of 90,000,000 cubic mètres of earth which have to be excavated from the Panama Canal, only 2,500,000 cubic mètres had been removed up to October, 1883. In that month more than 10,000 men were employed on the work. It is now proposed to increase the working force to 15,000 men, and it is expected that with better weather the extraction will be materially increased. It is still hoped that the canal will be inaugurated in 1889.

THE ENTOMOLOGY OF A POND.

By E. A. BUTLER.

OUR aquatic insect fauna is both extensive and interesting. The habits are varied and the forms peculiar, in consequence of the structural modifications rendered necessary for their adaptation to an aquatic mode of life. They can, moreover, be easily studied, even in the home, by help of suitable aquaria, and, hence, we hope that a few papers devoted to their consideration may be not unacceptable. The insect inhabitants of a pond constitute tolerably well-defined groups, differing according to the area of their distribution. You find one set almost exclusively on the surface, which they rarely leave either for excursions into the depths below or the air above; another in the middle depths, where they disport themselves in all directions, occasionally also visiting both top and bottom, and even escaping upwards into the rarer element; another on the bottom, where they grovel amongst the mud; another, again, round the margin, where, like children at the seaside, they dabble about in the wettest parts, and even let the tiny ripples play on their very feet; and yet another, gracing with their presence the air above the pond, scudding about in search of the two great desiderata of an insect's life, food and mates. We will first turn our attention to

THE SURFACE.

The fauna here is almost exclusively Hemipterous, consisting of bugs belonging to the remarkable section *Hydrodromica*, or Water-measurers. These curious beings will have attracted the attention of even the most unobservant. Blackish spider-like creatures floating on the surface, and jerking themselves rapidly along by vigorous strokes of their long thin legs, leaving little rippling eddies behind them, they will have excited wonder by the apparent impossibility of their submersion, and by the confidence with which, therefore, they trust themselves to what is, to most creatures, the treacherous element. It is not easy to catch them: they are wary and shy, and can calculate with considerable exactitude the area of pond surface that can be covered by the water-net of the expectant biped on the bank, whom they seem to take a delight in tantalisingly watching from just outside the charmed circle. Let him hide behind a bush and wait till they appear on the other side, and then come round with a dash and a swoop of the net—they are equal to the emergency, and before the weapon can reach the surface, a few bold strokes of those long slender legs have carried them in an instant out of harm's way.

Cautious attempts, however, after a time result in the enclosure in the net of some stray individuals less wary than their fellows; but even then their ultimate capture is not a foregone conclusion—those same spindleshanks come to their assistance again, and, unless their would-be captor is vigilant, with a few bold leaps they will be out of the net, and hopping off in all haste through the grass to the water, which, once reached, they will sail gaily away. Suppose, however, we have managed to secure a specimen of the commonest species, *Gerris lacustris* (Fig. 1). Let us proceed to examine it. It is a blackish creature, with an orange edge to its narrow body, and a little over ⅓ inch in length. The head is prolonged into the customary beak, characteristic of the Hemiptera, bent back as usual underneath the body. The wings lie so closely along the back as almost to escape observation, but if we can manage to open them, we find that the upper pair are opaque and tolerably stout, but the under pair thin, membranous, and semi-transparent. They are very neatly packed away, and the upper pair overlap at their

tips. Turning the creature over on its back, we notice that underneath it is closely covered with tiny hairs, which in certain lights shine like polished silver, but in others appear of a dull grey. The legs are six in number, but the antennæ, lying close to the front pair, and almost equalling them in size, give the insect the appearance of having eight legs, like a spider; the front pair are short and rest upon the water at their tips, being extended beyond the head, where they are extremely useful in securing prey; the second pair are much the longest and constitute the rowing organs—they are slender, and look like stiff bristles bent twice at an angle; the third pair are similarly constructed, but, being shorter, do not in any way interfere with the powerful strokes of the others, and are used as rudders.

The attachment of the rowing legs to the body, instead of being placed underneath, as is almost universally the case with insects, is thrown well out at the sides, a peculiarity which enables the little rower to use its muscular power to the best advantage. The general appearance of the creature is not particularly attractive; in addition to the dinginess of its colour, the various modifications of its limbs give it, when off the water, an ungainly aspect, which seems to suggest that the owner of such slender appendages must have an anxious time of it to guard them from fracture; but Nature is always prepared to sacrifice elegance and symmetry for the sake of utility. A close inspection, however, reveals many points of beauty besides the silvery hairs, notably some coppery scales, dotted here and there over the upper surface. The eyes are prominent, and no doubt give their possessor a wide range of vision, which it greatly needs, for, living as it does at the junction of two media, it is exposed to the attacks of foes in the air above and in the water beneath.

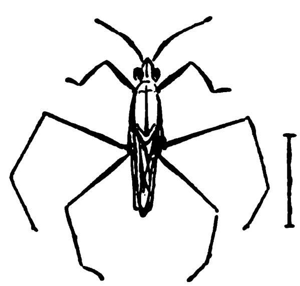


Fig. 1. Gerris lacustris.

The Gerridæ live by sucking the blood of other insects, which they can catch by pursuing and leaping upon them. Even on the water they possess considerable saltatorial power, and when themselves fleeing from their persecutors, if the ordinary rowing does not effect their escape with sufficient rapidity, they will expedite their flight by a few wild leaps.

Ten species of the genus inhabit the fresh waters of the British Isles, two of them occurring only in Scotland. The largest kinds can with fully outstretched rowing legs cover a width of 2¼ in. of water, and are gifted with proportionately rapid powers of locomotion. Like most of the Hydrodromica, all the genus are gregarious, scores of the smaller kinds being often seen dotting the surface of a suitable corner of a pond. Insects somewhat similar are known to exist on the surface of the sea, out in mid-ocean, where, hundreds of miles from land, they spend their whole lives. It is curious how very few insects proper are associated with salt water, though the fresh-water fauna is abundant.

Closely allied to the Gerris group, but differing considerably in appearance and method of locomotion, is the strange insect named *Hydrometra stagnorum* (Fig. 2). This is one of the narrowest of all British insects, and reminds one of the exotic "walking-stick insects" on a small scale: its legs are as fine as hairs, and even its body, with a length of half-an-inch, does not exceed, at its widest part, one-twenty-fourth of an inch in diameter. It does not jerk itself along after the manner of a Gerris, but actually walks or runs upon the surface of the water; it is most frequently found close to the margin of the pond, where it alternates between land and water, equally at home on both. In consequence of their extreme slenderness, they easily escape detection, and half-a-dozen may be walking on the water, just under one's eyes, without being noticed at all.

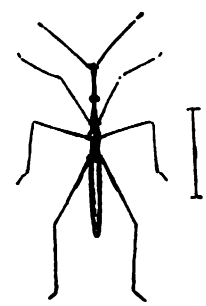


Fig. 2. Hydrometra stagnorum.

This insect exemplifies a remarkable peculiarity often met with amongst the Hemiptera. It will be remembered that the progress of development in bugs is such that no quiescent pupa stage intervenes between the active larval form and the adult insect; the pupa differs from the perfect form principally in the absence of wings, and from the larva in faint indications which form a suggestion or promise of those organs. Occasionally, however, the ultimate form does not acquire wings, but remains "undeveloped," thus greatly resembling a pupa, so much so, indeed, as to have deceived entomologists again and again, until it was discovered that these apparently immature forms were sexually mature, a condition that may usually be accepted as proof that an animal has reached its ultimate state. In all orders of insects there are apterous forms, but the Hemiptera are specially remarkable in two respects, viz., that there are various degrees of imperfect development in different species, ranging from an entire absence of wings to their perfection in all but some minute part, and that these conditions prevail in a large proportion of species. Out of a total of 420 species of British bugs, about 60 occur more or less imperfectly developed. Species thus imperfect when mature, occasionally, from causes at present undetermined, assume in certain individuals the completely winged form, but such instances are, as a rule, rare. The present insect possesses only the merest rudiments of wings.

(To be continued.)

Subterranean Fish.—A fact of much interest to students of natural history is vouched for by Cavalier Moerath, a civil engineer, formerly of Rome, and now visiting this country. This gentleman has devoted much labour and attention to the improvement of water supplies in Italy. In prospecting for water with one of "Norton's Abyssinian" tube wells, he tapped a spring from which was pumped a tiny living fish. This fish had passed into the tube well through the ordinary perforations of about ⅛ inch. Examination proved it to have no eyes, clearly indicating that it belonged to an order intended to inhabit subterranean waters. The occurrence was certified to by two other gentlemen who were present when the fish was pumped up.

Editorial Gossip.

M. PASTEUR promises us safety in future from hydrophobia,—though as yet it does not appear clear that he has established the validity of his preventive system. It appears that after being bitten by a mad dog a man or any other animal may be saved from an attack of hydrophobia by being inoculated with the virus in different degrees of strength. It is unnecessary to be inoculated before being bitten. The case is akin to that of small-pox as dealt with by an American physician, whose experiences we quoted in one of the early volumes of KNOWLEDGE. He found that the signs of an impending attack of small-pox could be recognised by the pulse; and that even after such signs had been noted it was not too late to secure safety by vaccinating the patient.

PASTEUR hopes to see hydrophobia eradicated, through the application of his system to every case in which an animal has been bitten by a rabid dog. For whatever the actual origin of the disease, it can now only be imparted by actual biting.

How great is the "glorious uncertainty" of cricket! The Australian eleven defeats in one innings an eleven including many of the finest players in England, and is presently defeated by the Oxford eleven by seven wickets. The Oxford eleven meets an eleven of the gentlemen of England regarded as only moderately strong,—and though at this writing the match is not over, it looks "all Lombard-street to a China orange" against Oxford. (A few minutes before writing these lines I saw the fifth Oxford wicket fall for 73, more than 200 runs being wanted to save the game.)

ON one occasion, when the same elevens of Eton, Winchester, and Harrow encountered each other, Eton beat Harrow in one innings; Harrow beat Winchester in one innings; and Winchester thus shown (one would have thought) to be far weaker than Harrow, which had been proved to be far weaker than Eton, beat Eton in one innings. What are the odds against such a result to the three matches? It would not be easy to say. But the nature of the problem may be thus presented. The games showed the three schools to be pretty nearly equal. The chances may be some ten or twelve to one against one of two nearly equal elevens beating the other in a single innings. Say eleven to one. Then the chance of each match ending in a single innings defeat is $1/12 \times 1/12 \times 1/12$, or $1/1728$; but the chance that the beaten eleven in the first match would be the winning eleven in the second, is only half the chance that either one or the other of the elevens in the second match will win in one innings: a similar relation holding in the third match. Hence, if the chance of a single innings' defeat in a match between two equal elevens is $1/12$, the actual chance of the observed event was $1/12 \times 1/2 \times 1/12 \times 1/2 \times 1/12 = 1/6912$, or the odds were 6911 against the observed event. The coming-off of the observed event was undoubtedly a very singular coincidence.

IT may be a mere coincidence, a singular chance, that Cambridge has been singularly successful against the Australians. Thrice have Australians met Cambridge, and thrice have they been soundly beaten by the younger men —once in a single innings. Oxford now has had a turn; and let the match with Cambridge go how it will, the Universities will be one win ahead on the seven games. In the first Cambridge victory, Cambridge played without Lucas, her safest bat. Speaking of cricket chances, what are the odds against every man in an eleven making double figures? Undeterminable of course. But, has such a result ever been recorded? Last year it nearly came off. (I have long been looking out for a case.) An eleven made all double figures but one,—even double figures in the extras, and no triple figures. But one man reached nine only! Is there any recorded case where every man in an eleven made over nine and under 100, the extras being also between those figures?

MR. YATES, editor of *The World*, thinks to show his profound acumen by ridiculing the account given by Mr. Wilson, senior wrangler in 1859, and head-master of Clifton College, and to make his ridicule more effective calls Mr. Wilson "a senior wrangler, now a schoolmaster." This sort of wit is akin to that shown by the rival editors of *Truth* and *The World* in calling each other respectively Edmund and Henry, as if the editors of *Nature* and KNOWLEDGE should deem it clever to address each other in those papers as Richard and Joseph. Mr. Yates would not find his standing much shaken were anyone to speak of him as "a novelist, now an editor," though others might regard it as no compliment to speak of him as author of Mr. Yates's novels and editor of *The World*. One is led to ask whether Mr. Yates's bludgeon was broken when he brought it down so clumsily on Buchanan, or the edge of his dagger turned when it glanced off Lord L.'s backbone. Mr. Wilson would have survived even though Mr. Yates had said of him he was only a mathematician,—for Mr. Wilson was something more than a fine mathematician even at the time to which his story relates. He was an excellent classic, a more thorough student of literature than Mr. Yates, to judge from his books, has ever been, and outside his study a good all-round man at many manly sports, as cricket, football, and rowing,—"a virtue which was never seen in" Y. Why any statement of his should not be trusted merely because he has not written (at least he has not yet published) a fifth-rate novel, or edited a paper which has been described of late as fit reading only for fools and flunkeys, is not obvious.

THE PHOTOGRAPHIC SOCIETY OF LONDON.—Last the monthly meeting of the Photographic Society in Pall Mall, Mr. James Glaisher, F.R.S., presiding, Mr. John Spiller, F.C.S., read a paper on "The Fading of Paper Photographs." He ascribes much of the fading to the presence of traces of free hyposulphite of soda in the cards and papers ordinarily sold by manufacturers, and on which the photographs are mounted. The presence of hyposulphite of soda therein, he said, is almost universal, especially in the black and highly-coloured tablets. The salt is used by manufacturers to get rid of the chlorine used in bleaching the materials. Although thirty years have elapsed since photographers made an outcry against hyposulphite of soda in ordinary papers, it is only of late that much chance of the disuse of the salt has appeared; out sulphite of soda will answer the same purpose, and at present this and several other sulphites are in course of manufacture on a large scale at Stratford for paper manufacturers. He had soaked paper photographs in sulphite of soda for forty-eight hours without their being the worse for the treatment, which was not the case when hyposulphite was substituted. In the discussion which followed, Mr. Dunmore said that he admitted that solutions of hyposulphite of soda would cause a print to fade, but in a dry state it would keep all right; he and others had tried it by experiment. Captain Abney said that the yellowing of the white and not the fading of the dark parts of photographs was the greater trouble; the organic oxides of silver did the damage, he thought. He could corroborate Mr. Dunmore's statements. Mr. Werge stated that very thorough washing of the prints after fixing, such washing as they did not always receive, had much to

do with their permanency. Another speaker said that twenty-three years ago, when he was abroad, he sent some photographs to the London International Exhibition, and was surprised to receive no award or notice; he went to Dr. Diamond about it, who asked him to go and look at his pictures; he did so, and found they had all faded. Those kept by himself had not faded, so the effect was due to something in the mounts used by the London operator to whom he sent them to be mounted for the Exhibition. Mr. W. England stated that he remembered the photographs mentioned by the last speaker; they had been suspended in a damp place at the Exhibition. Mr. Spiller, in the course of his reply, said that sodium sulphite has little power in dissolving chloride of silver, but it absorbs free chlorine. He denies that prints with or without hyposulphite of soda in them can be kept dry, because paper is hygroscopic; a sheet of paper will, therefore, gain several grains in weight in wet weather. He was only dealing with one portion of the subject that evening, and not the one mentioned by Captain Abney. Mr. W. E. Debenham then read a paper on the "Illumination of the Developing Room," and the proceedings closed.

Telegraph Wires in New York.—The New York *Electrician* states that the winter just past has been unusually severe upon overhead telegraph wires, although no single storm has wrought the extensive damage that has been caused by the sleet and snow of previous years. The Metropolitan Telephone and Telegraph Company of New York was the most seriously affected, by reason of its immense system of light wires. A single storm caused about nine hundred interruptions, while on other occasions the derangement of the service was such that the superintendent would be willing to see the wires placed almost anywhere, even underground, if the result was otherwise satisfactory.

Travelling in Abyssinia.—The special correspondent of the *Daily News* with Admiral Hewett's expedition gives the following account of the country:—The next morning we entered on the most serious part of our journey, a very steep ascent of 5,000 feet. The first few miles of our route lay through very fine mountain scenery, reminding me a little of our own Highlands and a good deal of the Balkans. Birch, cedars, and acacia trees, box, and orchids covered the sides of the gorges, flowers in profusion, maiden-hair ferns and lichens brushed us as we toiled up the mountain. A few of the Abyssinian guard in front of the Admiral played upon pipes roughly made out of the bark of the trees, and the notes, very mellow and sweet, seemed to start all the birds along our route into song. Skirting for some time the side of a rocky precipice, we suddenly emerged into a valley, the aspect of which was unlike any we had yet seen. The whole foliage of the mountain seemed to change as if by magic, so unexpected a transformation from European delicacy to African crudeness was the sight of the *Euphorbia candelabra gigantica*, bursting into bloom with its clusters of red and yellow blossom, gigantic aloes in flower, and cactus parasites clinging to the rocks and trailing in great luxuriance from the trees. The sun, which had been shaded from us by the forest below, now burst out in all its fierceness, flooding the valley with a glare of light, making this Arabian Nights sort of scene, though novel, most distressing for weary travellers in search of a camping-ground. At last, where the valley narrows under an avenue of *Candelabra gigantica*, we pitched our tents and started our fires. The following day was the most trying and difficult of all our marches. The Maiensi Pass is one of the steepest routes for the passage of human beings to be found on the globe. It is absolutely impossible to ride any kind of horse up it; so we all took to mules. My horse, being led by my servant, dropped down dead at the foot of the mountain. Whether he died from sheer fright at the terrible journey before him, or by poisoned herbs he had picked up the night before, I never shall know, for my groom only brought his saddle and tail in when we arrived in camp. Poor brute! He was the only animal we lost on that rough journey. Nearing the top of the Maiensi the *Candelabra* began to disappear, and only wild olive and the box were left to crown the heights, with the exception here and there of a bush of dog-roses and some wild lavender. A horseman now came scampering down from the mouth of the pass above us, and saluting the Admiral, told him that the Ras, his master, had seen us coming, and thus early sent his greeting. Presently the route narrowed into a rocky defile, and we suddenly emerged on to the Abyssinian plateau. Immediately her Majesty's representative was sighted, the slight eminence on our right and the plateau to our left became alive with horsemen galloping towards us, and when we were well in the open more than 1,500 cavalry charged straight at our group, throwing up their spears and waving their shields. Curbing their horses a few paces in our front they careered round our flanks, bowing to the Admiral, and then formed up in an irregular line in our rear.

"Let Knowledge grow from more to more."—Alfred Tennyson.

Only a small proportion of Letters received can possibly be inserted. Correspondents must not be offended, therefore, should their letters not appear.

All Editorial communications should be addressed to the Editor of Knowledge; *all Business communications to the* Publishers, *at the Office, 74, Great Queen-street, W.C.* If this is not attended to delays arise for which the Editor is not responsible.

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The Editor is not responsible for the opinions of correspondents.

No communications are answered by post, even though stamped and directed envelope be enclosed.

FALSIFYING HISTORY.

[1252]—It may appear a comparatively trivial matter with which to occupy the columns of a scientific journal, but I should like to enter my protest against the persistent falsification of history which goes on in our coinage and postage-stamps, by the way in which the Queen's head is reproduced upon them. The same girl's head and face, which did duty on money coined in 1840, reappears now forty-four years afterwards without the slightest alteration in any detail. I am, I trust, too loyal a subject to presume to believe that it is merely conceit on the part of the Queen which is at the bottom of this ridiculous travestying of her features. Her age (65) may be read in every penny almanack that is sold about the streets, and hence to represent her as still about 18 is simply to insult her by the implication that she is pleased by such barefaced and fulsome flattery. Her subjects are not wholly unfamiliar with her present aspect, and know perfectly well that what purports to be her portrait upon the shilling and the postage-stamp bears much about the amount of resemblance to her at present that it does to Miss Victoria Vokes. How differently these things were managed in the days of the Queen's immediate predecessor, a glance at coinage still in circulation will suffice to show. George IV. was about as vain and conceited a man as ever lived, but he did not go to the length of being represented as a young man of two-and-twenty upon the huge five-shilling pieces which characterised the mintage of his reign. Nor did his Majesty's immediate predecessor, William IV., indulge in such empty nonsense either. The value of coins and medals to the numismatist, the archæologist, and the historian can scarcely be overestimated—always assuming that they present accurate contemporary representations of the persons whose effigies they bear; but if such effigies are not to be actual *portraits*, they become a mere delusion, mockery, and snare. The recent issue of a fresh set of postage-stamps and the proximate one of the new half-sovereign might well have been taken advantage of to give the nation a real and authentic portrait of the Queen. Is it mere red tape, or is the meanest and paltriest toadyism, which has stood in the way of this most desirable consummation?

Esse quam videri malim.

["Esse" (he will excuse my shortening his name) omits to notice that the coins and stamps he refers to will be hereafter most instructive in one important respect, though they may not show (which I venture to regard as of less importance) the appearance of the venerable old lady whom they are supposed to symbolise. Will it matter greatly if lads and lasses five hundred years hence, looking at a shilling of this day in a numismatists' collection, should fall into the mistake of imagining that the nominal sovereign of England in 1884 was a mild-looking young lady of some eighteen summers? If at that time men have so far progressed that they no longer put as an important part of education the dates of accessions and deaths of kings and queens, but consider rather those events by which the progress of the nation was really influenced, I should imagine they would hold that "the value of coins and medals to the *historian* could scarcely be *under*-estimated," so far as the particular point touched on by "Esse" is concerned. But the lesson our young-lady pictures of a lady who if I mistake not has been for several years a great-grandmother, will teach about the ways of certain classes in these our times, will be instructive, and, I should imagine, rather impressive five hundred years hence. Some Herbert Spencer of the future will therein find

evidence, I expect, of the way in which men may rise "on stepping-stones of their dead selves to higher things" (for this we trust is as true of the race as of the individual). No one I should suppose can doubt what this silly bit of flattery really means. Nor can any one who notes the development of sense and manliness among us question that a few generations hence such nonsense will be impossible. I venture to suggest some slight doubts as to "Esse's" chronology. I labour under the impression that the immediate predecessor of the queen was the exemplary William IV., who left no son to succeed him, though a dozen sons and daughters were born to him—the genial "sailor-king" of 1830–1837, who after 1837 is pictured in the narrative of those who had bowed before him and flattered him during life, as a half-crazed boor of most ungenial nature. His immediate predecessor was the still more exemplary George IV., of whom Englishmen did not hesitate to call themselves "loyal subjects," degrading the noble word "loyal" to a most ignoble use, and describing themselves by a word which for my own part I would no more apply to myself (let the law call me what it pleases) than I would call myself a slave when I knew I was none.—R. P.]

THE GRAND CAÑON.

[1253]—I believe Sir C. Lyell has given 30,000 years as the time it has taken the Falls of Niagara to recede about six miles from their original position. I have some grounds for supposing the same period has been occupied by the Colorado river in cutting the channel or ravine called the Grand Cañon. As you are doubtless aware, this gorge is cut through 4,500 feet of sedimentary deposit, and then 1,000 feet into the granite. I should like to ascertain whether any estimate of the time it has taken the Colorado river and its tributaries to cut these deep gorges has been given by any geologist. THOMAS AYERS.

CANTILEVER BRIDGES.

[1254]—A friend has lately put into my hands a bundle of past numbers of your publication, KNOWLEDGE, the perusal of which has afforded me much pleasure. Your journal seems to me to supply a want.

In No. 127 I observe a description of a Cantilever bridge lately erected in America. This article was of great interest to me, as I have long advocated this system of construction. In the year 1858 I patented a "Bracket bridge." A large cardboard model of this bridge, made with my own hands, stands in the Patent Museum at South Kensington. This system was defective, and in 1864 or 1865 (I forget which) I patented an improvement, which consisted in a compound Cantilever and truss superstructure, with hinged joints at the junction of Cantilever and truss. This is the system described in your No. 127.

For several years I tried to get this plan tried by engineers, and very nearly succeeded. In one case the design was approved, working drawings were made, and the erection of piers begun. This was in 1866 for a bridge in Russia. A commercial crisis coming on, the works were stopped, and the bridge never erected after all. The bridge now building over the Forth is on this system.

In 1868 I patented a further improvement, which consisted in substituting a suspension bridge for the central truss, thus introducing further economy. I made numerous designs and estimates for bridges on this plan, but the conservatism of our English engineers was too strong for me, and consequently I have spent the last thirteen years of my life in ordinary railway engineering work in the Colonies.

I think it is a very happy idea of yours to gild your pill of knowledge by a Chess and Whist Column. You might go further and notice another scientific game—billiards, as regards which I have never come across any *scientific* treatise. The manuals which I have seen are anything but scientific. For example: The authors start by assuming that the angle at which a ball rebounds from the cushion is the same as the angle of incidence, which can scarcely be the case, as the angle of reflection is affected by the rotation in a vertical plane acquired by the ball in its journey to the cushion.

Your answers to correspondents are often very amusing reading.

E. W. YOUNG.

PUIR DOGGIE.

[1255]—My dog having a weak leg, I wished to see if magnetism would benefit him. I held one of his paws and my brother the other, each of us holding a handle, but to my surprise the current would not pass through the dog, although it had just before passed through a friend in the same position. Can you explain this fact? IDA.

COINCIDENCES AND SUPERSTITION.

[1256]—It seems to be very generally admitted that the most reliable evidence of supernatural appearances is that bearing on cases occurring at the time of death of friends and relatives of those who have been the victims of the apparition.

In other cases, without the appearance of anything supernatural, the fact of death having taken place has been forced upon the mind of distant relatives in a most unaccountable manner immediately after, or perhaps just at the time of decease. Whatever truth there may be in these two classes of evidence, I have no doubt much is due to mere coincidence. My own experience is limited to two instances. A few years since I attended an evening concert in a building well known to the Editor of KNOWLEDGE—the Dome at Brighton. At that time I knew my father was seriously ill. After the concert had made a good start, and when I was thoroughly enjoying the singing, a sudden conviction forced itself on my mind that my father was dead. I reasoned with myself that it was mere nonsense to allow such an idea to exist in my mind, but reason utterly failed, and I was quite prepared to hear of my father's death. A letter arrived a day or two after, and I was informed that there had been no change in my father's condition. My own health at the time was not good, and I should therefore, if "Cosmopolitan's" theory be correct (1180) be in the best condition to receive truth by the bodily forces being in subjection to the spiritual part of us.

My second experience was very similar. My wife was with her father, who was very ill, and she wrote me that the doctors had stated he could not live long. A day or two after the receipt of this news a sudden idea, which amounted to a strong conviction, took possession of my mind that death had taken place at a particular hour. It was a false conviction; my father-in-law died several months after. My health on this occasion was good.

Had death taken place in either of these cases about the time of my unreasonable convictions, the coincidence would have been accepted as matter of fact.

The next case tends to the same conclusion as that of Mr. Corbett (1166):—

In a portion of Dorset there still exists a belief, though now dying out, that when a person dies, a noise is heard at the window like the tapping of a bird. A death occurred in the house of a friend of my own about midnight, and a little later the widow and her son were in a room alone. The son lay on a sofa, tired and sleepy. Suddenly a peculiar noise was heard at the window, and what made it more than usually unaccountable was the fact that a stream of water ran between the window and the road. The gentleman was instantly wide awake, and, although a disbeliever in "ghosts," the tales he had heard in childhood came strongly to mind. After a short interval the tapping was repeated; and, again, the orthodox third time. The gentleman rushed out of the house, and opposite the window he found, not a "ghost," but a carter, who, having lost his way, had left his horses to make inquiries. He could not find the door, and had struck the window lightly with his whip across the stream.

G. HANN.

COINCIDENCES.

4, Bellevue-crescent, Edinburgh, May 5, 1884.

[1257]—The following is from the *Dundee Advertiser* of May 2:—"On Saturday last the four-masted ship *Glencairn* arrived from Chittagong, with a cargo of jute. She hails from Glasgow, registers 1,564 tons, and has made the passage in 116 days. On Monday the ship *Trafalgar* arrived in the river. It is notable that this vessel is also a four-masted ship, belongs to Glasgow, has made the passage in 116 days, comes from Chittagong, and brings a cargo of jute, being in these five circumstances identical with the *Glencairn*."

Hardly any but "those who go down to the sea in ships" can appreciate the countless conditions which affected the progress of these two ships, so that their equal passages are quite remarkable as a coincidence, even if this were not coupled with four others of greater probable chance of recurrence. J. P. S.

[1258]—Whilst you are dealing with coincidences in KNOWLEDGE perhaps you will allow me to submit to you the following one:—

Some years ago I left my home in Scotland, and took up my residence in London. Prior to my departure I had met, on separate occasions and only for a few hours in each case, two gentlemen of nearly equal age, and bearing the same patronymic (which I shall denote by "Blanke"). They were, however, quite unknown to each other; one resided in the north of Scotland, the other in one of the English midland counties.

Well, after an interval of about four years, during which I had quite lost sight of the Messrs. Blanke, a letter from home

announced to me that (the Scotch) Mr. Blanke was proceeding to the metropolis on a visit, and that he would call upon me at my office *within the next few days*. In a few hours after the receipt of this letter "Mr. Blanke" was announced. The appearance of my visitor was slightly different from what my recollection of him led me to expect; however, I put that down to the difference which a few years would naturally produce, and his visit, coinciding completely with the information I had just received in the letter, seemed to me an indubitable proof of his identity. As for the other "Mr. B.," I had quite forgotten his existence for the time.

During our conversation I inquired after a person with whom I had reason to think that my visitor was acquainted, but the reply somewhat surprised me: "Don't know him," said Mr. Blanke. A few other remarks of my visitor also seemed to me rather incomprehensible, and we got on much better when we came to discuss topics of general interest. Not until the interview was finished, when I was reflecting as to what the causes of our misunderstanding could have been, did I think about the other Mr. Blanke; and then the coincidence was at once revealed. My visitor had been the English Mr. Blanke, who, also chancing to visit London at the same time as his namesake, had called upon me without previous notice. About a week afterwards Blanke *secundus* appeared on the scene, and was considerably surprised and amused at the coincidence I had to narrate to him. J. W. S.

[1259]—As you seem to be rather "going in for" curious coincidences just now, I send you one in case you may think it sufficiently curious to insert. Some years ago I went to stay at B. in company with a party of friends. On the first Sunday we went to church, where the sermon was preached by a visitor, the vicar reading prayers. The sermon was fairly good, and I do not doubt the excellence of the personal character of the preacher; but in consequence of something about the tone of the discourse, one of our party gave to this clergyman the sobriquet of the "Pharisee." This title, not a very kind one, though not unkindly meant, stuck to him amongst ourselves. The good vicar, who had a comfortable, genial aspect, was forthwith dubbed the "Publican." That name stuck to him also, but strictly among ourselves. His countenance was, I think, ruddy and pleasant to behold, but there was nothing about him suggestive of the tax-gatherer of the New Testament. Three years afterwards, my wife (one of the original party) and I again visited B., and went to church on the Sunday morning. A friend—Mrs. V.—was with us. We had told her the story of the Pharisee and the Publican, and had made the remark, "How curious if they should both take part in the service." We had no reason to suppose that the stranger was staying at B. at the time, and we were not aware whether there had been any change of incumbent. Service began, the original stranger saying prayers and reading the lessons. He read from the old lectionary, though the new had by that time come into general use. The second lesson was from Acts xxiii. When he came to the words in the sixth verse, "Men and brethren, I am a Pharisee, the son of a Pharisee," our feelings were not quite what they ought to have been in church; but when the incumbent—the pleasant, genial-looking vicar—"ascended the pulpit," and gave out as his text Luke xviii., 13, "And the publican standing afar off," &c., and preached an eloquent sermon on the subject, our feelings gave us a great deal of trouble indeed, and our friend, Mrs. V., who has a strong sense of the ridiculous, underwent a martyrdom it would be difficult to describe. C. H.

[1260]—A number of years ago my mother was residing in the north of Scotland, in a country district, and, while there, had a curious dream, immediately followed by what may fairly be called a "coincidence," allowing for the bull.

She dreamt she was calling at the house of her cousin, Captain F——, and on arriving found the front door open, and no one in attendance. She walked into the dining-room—to the right on entering—and to her horror found a coffin lying on the bare mahogany table. She particularly noticed that the lid was lying diagonally across the coffin, and on looking into it, her horror was still greater to find the dead body of her cousin, Captain F——, dressed in full Highland costume. She then awoke.

Now it happened that on that very night, and unknown to my mother, Captain F—— was attending a county dinner at the town of B——, and had intended to remain at an hotel for the night, but on its becoming known that this was his intention, several gentlemen in his immediate neighbourhood at the table chaffed him unmercifully, alleging that an easy tumble into a bed close at hand was much more to his liking than a nine miles' drive, and perhaps an uneasy tumble into the bed of some mountain stream. This proved sufficient to make F—— drive home, and on his way, his horse went over the low parapet of an old-fashioned bridge, and precipitated his groom and himself into the ravine below. The groom was instantaneously killed, and F——, who was found a few hours afterwards, did not see the day out. Now, for the coincidence, as I have called it. My mother called the following day, and found the *door open*, with *no one in sight*, and on going into the dining-room found the coffin on the bare table with the lid as I have already described, and inside it, the corpse of her cousin, *dressed in Highland costume*. The last coincidence was the strangest of all, as, whatever eccentricities the Gaels may be capable of, it is not customary with them to lay out their dead in any other but the orthodox manner. Nor was my mother aware that it had been her cousin's wish that he should be so dressed after death.

T. W. R.

[1261]—Without attaching any importance to the following, it may, I think, fairly be considered a "curious coincidence," and as such, perhaps not beneath your notice. One evening, some years ago, I was the spectator of two or three lengthy rubbers of Whist, during which the entire run of luck was in the direction of one of the players, *viz.*, my father. Being what I may call a *family* game, his continued success gave rise to no small amount of good-natured triumph on his part, and banter on the part of his opponents. At length I—the youngest of the party, and knowing really nothing of the game beyond one or two very broad rules—challenged my father with, "I will take 'dummy' and win every trick the first round just to punish you," and, amid general incredulity, and never having really meant my thoughtless, rash assertion, I nevertheless turned up four honours, and, with ease, took all the tricks, according to promise, and this against old and, by comparison, skilled players, neither of whom were more amazed at my good luck than was I myself.

At that time I would be about sixteen years old, then, as now, very stupid at cards—taking, usually, no interest in games of any kind. B. H.

SINGULAR AND SAD COINCIDENCE.

[1262]—We have received the following story from the naval officer who conducted the inquiry referred to in it:—

"During the war between Buenos Ayres and the Banda Oriental, in 1844, an officer commanding one of our vessels of war was sent by the senior officer, at the request of the British Minister, to inquire into the truth of a report of Col. Gomez and five hundred of his cavalry force having been taken prisoners and put to death by General Urquiza, who commanded a body of Buenos Ayrian cavalry.

"At Maldonado, near the battle-field, he obtained clear evidence that the prisoners—officers and men—had been drawn up in line with their arms tied behind, and a dismounted lancer behind each, who on the word of command from Urquiza lanced them all to death. This he reported on his return to Monte Video.

"In 1864 the same officer was crossing to France in the steamer from Newhaven, and met on board a fine-looking young Spaniard, who, he found, came from Monte Video, and who said he recollected the siege, as he was ten years old at the time. In speaking of the barbarous character of the war the officer mentioned the facts connected with the murder of Col. Gomez and his men. When he finished the tears were on the stranger's cheeks as he said, 'Col. Gomez was my father.'

"That he should have heard the particulars of his father's death, *for the first time*, from a stranger in Europe twenty years after, was an extraordinary coincidence."

ANCIENT STORY AND ODD COINCIDENCE.

[1263]—Apropos of your article on coincidences, perhaps it may be worth your while to insert the following recent experience of mine. I was reading an article on schoolboys' examination blunders. One boy had said that the venerable Bede was, owing to his antiquity, called *Adam* Bede. Not remembering at the moment who the latter was, in a little perplexity as to the exact meaning of what I had read, I laid the paper aside, and, in reaching for something else, glanced over my sister's shoulder. She was reading George Eliot's "Adam Bede"! F. J. N.

COINCIDENCES,—PHENOMENAL VOICE.

[1264]—We seem to be getting a little mixed as to what constitutes a "coincidence," when it can be said that we must admit "something supernatural sometimes even in coincidences." I am rather afraid to attempt a definition, but it would surely have to run something like this:—"A 'coincidence' is the concurrence of events or phenomena due to independent* causes, though appearing to be due to a common cause." It is a contradiction in terms to

* No doubt one of the causes might be thought supernatural; but, *then*, could it *also* be independent?

call that a co-"incidence" which is conceived to have a "supernatural" origin or explanation, except in so far as *all* events may be so conceived. Again, it is essential to a "coincidence" (in our present connection) that, if it be a *re*currence, all the essentials shall be unperiodic. If I go the same walk, at the same hour, on successive days, and meet the same people, there is no "coincidence," unless I can establish that those people were as little in the habit of taking that walk at that hour as I myself. In short, "coincidences" must be *accidental* in every aspect.

May I be allowed to give another instance, which, I believe, will satisfy this condition. I have a craze for logarithm tables, and on one occasion bought a copy of Grunert's 5-place Logs, with which I was very much pleased; and took an early opportunity to use it. To my disgust, my result was wrong—or *would have been*—I am uncertain now—owing to a misprint, which I detected. Naturally, I threw the tables aside, resolved never to be taken in by *them* again—albeit it was a pity, because they are the best arranged tables (of the kind) I know. It was only some considerable time afterwards that, happening to look into the volume again, I saw a short notice headed "Nachträgliche Verbesserungen"—meaning "supplementary corrections"—and on closer examination I found six mentioned of which one only was in the body of the page—as any one may see who comes across these tables—and, of course, it was the one I had found! Now, that is what I call a *true* coincidence, without a trace of the supernatural to spoil it. I have many others, nearly as good, duly noted. Only yesterday I opened Newcomb's "Astronomy," a volume of 560 pages, at p. 354, the *very page* to which the index had just referred me. But that is a trifle to opening the Post-Office Directory *at random*, in the desperate hope of finding something in the *Official* part, and gradually becoming alive to the fact that the first and only words read were the ones to be looked for! But I fear I have pursued the subject too far.

I am tempted to write at some length on the subject brought forward under the heading, "Phenomenal Voice," but must confine myself to saying that the impossibility resides not in the vocal power at all, but in the capacity of the ear for distinguishing the intervals. The statement is very much the same as if the owner of a thermometer should claim that his instrument could show 1,000 different temperatures between 60° and 61° F. J. HERSCHEL.

A TRICYCLE CATCHES A BALL.

[1265]—Reading your article upon "Odd Coincidences" in your paper of May 9, I am induced to write you with reference to a "coincident" which occurred on Saturday last, May 10, on Clapham Common. A ball had been thrown across the road from some cricketers, when there was a cry of "Lost ball." It was found embedded between the spokes of a wheel belonging to a tricycle, the rider of which was unconsciously wending his way along the road. This is the more extraordinary as it was on the "off side." How it got there I cannot understand. ALFRED W. WILKINSON.

TENTERDEN STEEPLE AND GOODWIN SANDS.

[1266]—"And here by the way I will tell you a merry toy. Master More was once sent in commission into Kent to help to try out, if it might be, what was the cause of Goodwin Sands and the shelf that stopped up Sandwich haven. Thither cometh Master More and calleth the country before him, such as were thought to be men of experience and men that could of likelihood best certify him of that matter concerning the stopping of Sandwich haven. Among others came in before him an old man with a white head and one that was thought to be little less than a hundred years old. When Master More saw this aged man, he thought it expedient to hear him say his mind in this matter; for being so old a man it was likely that he knew most of any man in that presence and company. So Master More called this old aged man unto him and said, 'Father, tell me if ye can what is the cause of this great rising of the sands and shelves here about this haven, the which stop it up so that no ships can arrive here? Ye are the oldest man that I can espy in all this company, so that if any man can tell any cause of it, ye of likelihood can say most of it, or, at leastwise more than any man here assembled.' 'Yea, forsooth, good master,' quoth the old man, 'for I am wellnigh a hundred years old, and no man here in this company anything near unto my age.' 'Well, then,' quoth Master More, 'how say you in this matter? What think ye to be the cause of these shelves and flats that stop up Sandwich haven?' 'Forsooth, sir,' quoth he, 'I am an old man; I think that Tenterden steeple is the cause of Goodwin Sands, for I am an old man,' quoth he, 'and I may remember the building of Tenterden steeple, and I remember when there was no steeple at all there. And before that Tenterden steeple was in building, there was no manner of speaking of any flats that stopped the haven, and therefore I think that Tenterden steeple is the cause of the destroying and decay of Sandwich haven.' And so to my purpose, preaching of God's word is the cause of rebellion as Tenterden steeple was the cause that Sandwich haven is decayed" (from Latimer's "Sermons," quoted in "Chambers's English Literature").

Evidently the "legend" quoted by "A Man of Kent" [1210], with its hypothetical embankment, is merely an amplification of this story of the good Bishop's. Certainly the latter knew nothing of a tract of valuable land or a neglected sea-wall.

H. A. NESBITT.

"TWINKLE, TWINKLE."

[1267]—"G. G. H.'s" information in letter 1228 is not correct. Dr. Drury's version, "Mira, mira" was from Miss Taylor's words, and is so acknowledged in the sixth edition of "Arundines Cami."

I have not a "Gammer Gurton" by me to refer to, but, speaking from memory, there is nothing of the kind in the old comedy.

More than thirty nursery rhymes and familiar jingles are referred to Gammer Gurton in the "Arundines," few of which can be traced there. Many are of obviously later date, such as "The old man of Tobago," and "Who comes here?—a grenadier?" &c.

J. C. FLEWETT.

[Thanks. Neither can I remember any reference to "Twinkle, Twinkle," in "Gammer Gurton's Neele."—R. P.]

CRIBBAGE PROBLEMS.

[1268]—The answers which you have published to "H. H. H.'s" cribbage problem on p. 60, suggest some different questions, in which the amount of the opponent's score is taken into account.

The published answers, omitting some which seem to be incorrect, give the following scores:—

Dealer	75	Opponent	28	Difference	47
„	74	„	33	„	41
„	61	„	5	„	56
„	70	„	32	„	38
„	78	„	32	„	46

The following hands have been devised, the first to make as great a difference as possible between the scores; the second to make a large score for the dealer without the opponent scoring at all; and the third to make as large a difference as possible, and yet to have the opponent play rightly, instead of throwing away his best cards for the benefit of the crib. These are my best efforts to these ends. Others may, perhaps, do better:—

HAND 1.—AS PLAYED.

			Trump.	Crib.
Opponent	9, 4.	10, 9.	5	Kn. 5, 5, 5.
Dealer	6, 4, 4, 4.			

Score: Dealer, 74; opponent, 5.

HAND 2.—AS PLAYED.

Opponent	4, 3, 7.	10.	6	4, 5, 5, 6.
Dealer	4, 5, 6.	5.		or 7, 7, 8, 8.

Score: Dealer, 62; opponent, none.

HAND 3.—AS PLAYED.

Opponent	10, 5.	9, 9.	4	4, 4, 4, 7.
Dealer	5, 5, 5.	6.		

Score: Dealer, 70; opponent, 7. The opponent discarded a 4 and a 7.

A. B.

LETTERS RECEIVED.

J. WHITLEY. Thanks; but so many spots of that shape have been seen.—E. A. C. Three-quarters of an inch diameter is unusual for hailstones; but much larger hailstones than that have fallen.—W. J. HOLLAND. Know nothing of Nap.—THOS. COMMON. We have not space; but thanks for thoughtful and suggestive paper.—JOSIAH GREEN. Such relations are too familiar to the mathematician to be dealt with at such length here. You could fill hundreds of volumes with matter of the sort.—FLORI. Had forgotten Mr. Williams' promise to write an article on "Heating Dwelling Houses."—EXCELSIOR. If you *know* you are not mistaken, then of course ♈ and not ♎ is the right sign for spring. As a mere matter of fact the earth passes the first point of ♎ Libra at the spring equinox, but then, if you know she passes the first point of Aries, mere matters of fact may count for nothing. The occultation of Venus matter seems hardly worth further discussion; what you point out came out clearly in your letter and in "F.R.A.S.'s" reply. You and he probably use the words intimation and mention in different senses. There was mention of Venus under the elements of occultations, but no mention of an occultation of Venus as visible in Great Britain and Ireland. That practically sums up the whole matter.—C. W. T. "Difference of longitude" is quite correct.—T. H. Quite

so; you are as free to deny as I to assert. It may be that you do not regard injustice as immorality, or that you do not consider it unjust to punish the innocent for the offences of the guilty (if those can be called guilty who simply acted—we are told—in accordance with qualities which had been given to them), or to act in other respects with the blind wrath characteristic of the oriental despot (wrath which appears splendidly godlike, no doubt, to those brought up as the slaves of such despotism). In either case you can deny from your point of view the imputed immorality. Or, to pass to a later part of the volume, you may see no immorality in a decision—*imputed* only, be it noticed, I utterly reject the idea that the imputation was well-founded—according to which certain men, women, and boys were to be slain, while the young girls, their daughters and sisters, were to be handed over as slaves (we know what that means) to the warriors, after a certain portion had been set apart for the priests. I have no wish to convince you that all this really is as hateful as it appears to me, or that such immorality was most wrongly attributed,—so that though it comes in company with much that is historically true, the story as a whole may be rightly described as romance. But I most decidedly object to accept your doctrine that no one is capable of acting rightly who does not accept the teaching which comes in company with what appears to me inherently unjust and therefore immoral. I may be quite mistaken and you may be quite right. But viewing matters as I do, I should be a traitor to the cause of justice and morality if I allowed a line to appear here in support of immorality and injustice, no matter under what false clothing disguised. Read history a little, and learn what atrocious cruelty and villainy have been justified by the teaching you applaud, and you will understand somewhat of my feeling on the subject. I mean to act on the good old saying, *Fais ce que dois advienne que pourra.* But observe,—I should be running counter to the feelings I express and hold if I felt any personal dislike to those who think as you do. I simply loathe what you think should be loved and have no more reason for being angry with you than I should have for wrath against one who was warmly attached to a lady whom I held in the reverse of esteem.—S. HOLLAND. I have indicated my own experience as favourable.—G. MACKENZIE.—Your idea might have been presented in much smaller space.—VACNOLL.—R. B.—LIVE AND LET LIVE. How could anything I said be understood to bear in the remotest degree on the question of originality? What could I know about a system which if I understand you aright, is preserved as a secret, unless I had paid for the knowledge? As I have not done so, I am quite in the dark,—and obviously have not claimed to be otherwise.—S. FOSTER. Pardon me, I think your anecdote illustrates rather how naturally some dreams come true. What more natural than that your sister should have dreamed of her old friend as in the same place with her—and that place, London, being rather large, the circumstance, that the dream came true, can hardly be regarded as very wonderful.—A. KITSOM.—A. SINCLAIR.—A. AITKEN. Thanks.—J. BINDON CARTER. I thought you were funning, so followed suit. To treat the matter with due gravity,—let g be terrestrial gravity and γ the gravity in another planet, let m be the average height of our men, and μ the average height of an entirely supposititious (nice word) race of men in the other planet. Assume that the weight of those extra-terrestrial men is no more burdensome to them than our weight is to us. Now it has been shown that strength varies as the cross-section of the muscles, or as the square of their linear dimensions, consequently we have strength of our men : strength of those others :: $m^2 : \mu^2$, while weight of our men : weight of those others :: $m^3g : \mu^3\gamma$; consequently by our suppositon $m^2 : \mu^2 :: m^3g :: \mu^3\gamma$, or

$$\mu : m :: g :: \gamma.$$

Apply this formula to the case of Mars, and you will find that if there were Martian men, and they were as active as our men (under similar supposed conditions) they would be about 14 feet high.—H. P. MALET. Received with thanks.—J. HAMMOND, W. J. HOLLAND. Don't play Nap (indicative mood).—J. BUTTLE. Thanks.—ALEX. THURBURN. Thanks for interesting story. The evidence is very strong. Wish the story could have been condensed. As you say, one might fairly expect *some* spirit utterances to be other than trivial. One would be disappointed, so far as I can see.—W. BRISTOWE. Thanks.—E. W. YOUNG. Many thanks for your book, which I shall examine with great interest.—T. W. WINCH. The deviation of Caucus from "Caulker" House or Calk House, seems very doubtful—like many others of Dr. Brewer's.—J. M. BACON. I do not know of any work discussing popular prognostications of weather. Venus will scarcely be visible to the eye in full sunshine before July,—she will be at her highest in August.—ROB. B. COOK. You are mistaken as to thanks, which were offered at the outset when those papers were proffered with the remark that they were placed at our service.—J. T. ROUTLEDGE. Reflex action, doubtless.

Our Mathematical Column.

EASY LESSONS IN CO-ORDINATE GEOMETRY.

By RICHARD A. PROCTOR.

(*Continued from p.* 338.)

EQUATIONS TO LINES.

WE have seen how the position of a point is expressed by means of co-ordinates; we proceed to shew how the forms and positions of lines, straight or curved, may be determined by the same means.

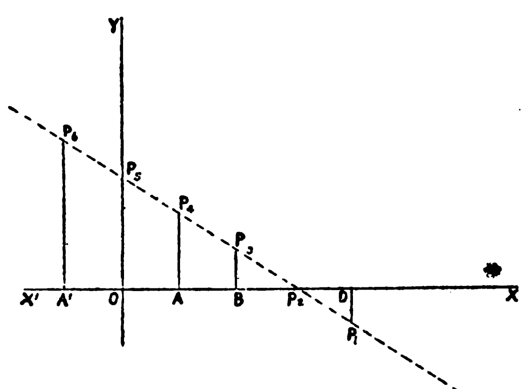
20. The learner has already seen that in geometrical problems in which the position of a point has to be determined, a certain number of relations must be given; and that, if one of the number is wanting, though we cannot determine the position of the point, we can, *in general*, determine a line straight or curved on which the point must lie. Thus if three points are given in position, the centre of the circle passing through them is known; but, if only two points are given, the centre of a circle passing through these two points is no longer a determinate point, since an infinite number of such circles can be drawn; yet we know that the centre of such a circle must lie on the straight line bisecting at right angles the line joining the two points. Again, if the base of a triangle is given in position, and a side, and the included angle, in magnitude, the vertex of the triangle is determined; but, if the included angle is not given the vertex is no longer a determinate point, yet it must lie on the circumference of a circle, having that end of the base from which the given side is to be drawn as centre, and the length of that side as radius.

We say, above, *in general*, because in some cases the omission of a single datum leaves the point altogether unrestricted.

Now we have seen that if a pair of equations of the form $x=a$, $y=b$ are given, the position of the point x, y, is fully determined. And since, if any two independent equations between x and y are given, we obtain by their solution one or more such pairs of equations as $x=a$, $y=b$, two equations enable us to determine either a point, or some finite number of points whose co-ordinates satisfy both equations. Hence we may expect, from what has been shown above, that *one* equation between x and y will enable us to determine, not a point or a finite number of points, but a line straight or curved, the co-ordinates of every point of which satisfy the given equation. And this, in general, is found to be the case. We shall consider two examples.

21. Take the equation

$$2x+3y-6=0 \quad (1)$$



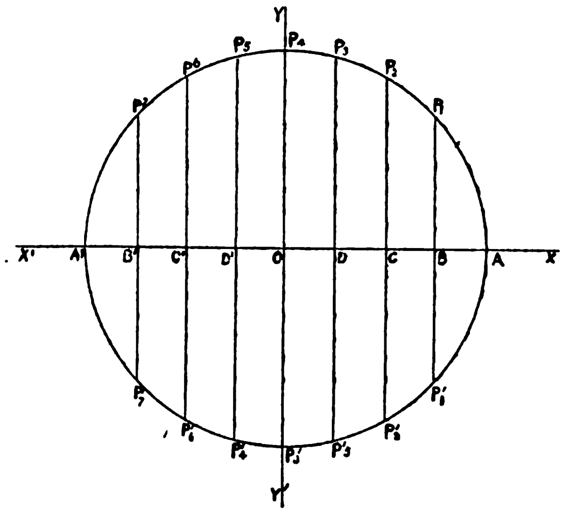
We are unable to determine from this equation the values of x and y; but we can assign any value we please to x and obtain a corresponding value for y. Each such pair of values of x and y determines the position of a point whose co-ordinates satisfy (1). Thus give to x successively the values, 4, 3, 2, 1, 0, −1; corresponding to these values of x we obtain for y the series of values—$-\frac{2}{3}, 0, \frac{2}{3}, \frac{4}{3}, 2, \frac{8}{3}$. Along X′OX measure OA′, OA, AB, BP_2, and P_2D, each equal to one unit of length, so that OD, OP_2, OB, &c. are the successive values we have assigned to x, and draw the ordinates D$P_1=-\frac{2}{3}$, B$P_3=\frac{2}{3}$, A$P_4=\frac{4}{3}$, $A_1P_6=\frac{8}{3}$, and take along OY, O$P_5=2$. Then P_1, P_2, P_3, P_4, P_5, and P_6 are the six points whose co-ordinates we have determined, as satisfying (1). An

obviously we may give to x any values we please intermediate between the values we have chosen, or greater than 4, or less than -1, and deducing corresponding values to y, obtain any number of points whose co-ordinates satisfy (1). We shall find that all these points lie along the dotted straight line in the figure.

22. Again take the equation

$$x^2+y^2=16;$$

as before we cannot obtain from this equation determinate values of x and y. Assigning to x the values 0, 1, 2, 3, 4, we obtain for each such value two values of y equal in magnitude but of opposite sign. These successive pairs of values are ± 4, $\pm\sqrt{15}$, $\pm\sqrt{12}$, $\pm\sqrt{7}$, and 0. If we give greater values to x we obtain



imaginary expressions for y, since such values of x make y^2 negative. Again, the negative values of x -1, -2, -3, and -4, give the same values of y as the corresponding positive values of x, and negative values of x numerically greater than 4 give imaginary expressions. Along X′ O X take O A = O A′ = four units of length, O B = O B′ = three units of length and so on, and draw through the points thus obtained $P_1 B P'_1$, $P_2 C P'_2$, &c., so that $P_1 B = P'_1 B = \sqrt{7}$; $P_2 C = P'_2 C = \sqrt{12}$, and so on. Then the points A, P_1, P_2, P_3, &c., P_7, A′, P'_7, P'_6, ... P'_1 are all points whose co-ordinates satisfy (2). We can further give x any values intermediate to those given, but not numerically exceeding 4, and obtain any number of other points whose co-ordinates satisfy (2). We shall find that all these points lie on the circumference of the circle in the figure.

These examples are sufficient to show that, in general, a single equation between x and y represents in Co-ordinate Geometry a line straight or curved. Such a line is called the *locus* of the equation, because it is *the place* in which lies every point whose co-ordinates satisfy the equation.

Similar remarks apply to a single equation between r and θ.

It is one of the objects of Co-ordinate Geometry to determine the form and properties of the loci corresponding to given equations. Another important object of this science is to obtain the equations corresponding to lines, straight or curved, drawn according to given definitions, or to fulfil given relations. We have used the word *locus* above: it has a more general meaning, and we append the following definitions.

23. *Def.* If every point of a curve satisfy a given relation, that curve is called the *locus* of points satisfying the given relation.

24. *Def.* If that relation is given in the form of an equation which is satisfied by the co-ordinates of every point of a curve, that curve is called the *locus of the equation*.

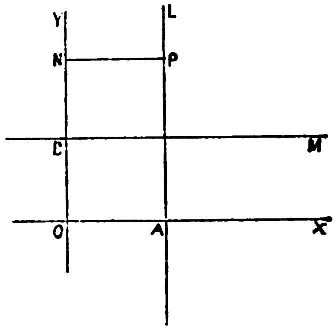
25. *Def.* And conversely, if we can express by means of an equation the relation which subsists between the co-ordinates of every point of a curve, that equation is called the *equation to the curve*.

Note.—The word *curve* in the foregoing definitions and throughout these papers includes the *straight line*. When a distinction is to be drawn between lines that are, and lines that are not, straight, we shall speak of the latter as *curved lines*.

The Straight Line.

We proceed to obtain equations to straight lines drawn according to different conditions.

26. *To find the equation to a straight line parallel to either of the co-ordinate axes.*



Let the straight line A L, parallel to the axis of y, meet O X in A; and let O A $=a$. From any point P in A L, draw P N parallel to O X, and let P N $=x$. Then P A O N is a parallelogram; hence

P N = OA, Euc. I., 34.

That is $x=a$,

the required equation.

Similarly, if the straight line B M, parallel to the axis of x, meet O Y in B and O B $=b$, the equation to B M is

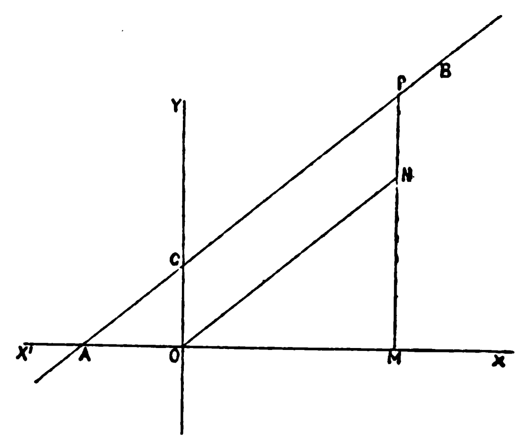
$$y=b.$$

The equation $x=0$, obviously represents the axis of y; and the equation $y=0$, the axis of y.

27. It is clear, also, that an equation of the form $x=a$, or $y=b$, necessarily represents a straight line parallel to the axis of y or x respectively, since such an equation expresses the relation that every point of the line which is the locus of the equation is equidistant from one of the axes, a relation which can only be true of a straight line parallel to such axis.

28. It is clear, also, that equations of the same form are obtained for straight lines parallel to the axis when these are oblique; and also that equations of these forms necessarily represent lines parallel to the axes when these are oblique.

29. *To determine the equation to a straight line inclined to the axes.*



Let AB be a straight line meeting the axes of X and Y in the points A and C, respectively; and suppose OC $=c$, and that the trigonometrical tangent of the angle CAO $=m$. From any point in AB draw PNM parallel to OY, and from O draw ON parallel to AB. Let the co-ordinates of P be x, y; then

$$\begin{aligned} y&=\text{PM}=\text{MN}+\text{PN}\\ &=\text{OM}\tan\text{NOM}+\text{OC}\\ &=mx+c \end{aligned}$$

the required equation.

30. If $c=0$, that is, if the line pass through the origin, the equation becomes

$$y=mx.$$

And it is clear also, conversely, that every equation of this form, that is of the form

$$\frac{y}{x}=m$$

represents a straight line through the origin inclined at an angle whose tangent is m to the axis of x, since from the definition of a trigonometrical tangent it follows that the relation expressed in the equation is true of the co-ordinates of every point in such a line, and of such points only.

31. As we shall frequently have occasion to make use of the equation to the straight line in the form

$$y=mx+c$$

it will be well to call the attention of the student to the meaning of the constants in that equation. Thus c or O C is the part of O Y cut off by the line, and is positive when C falls above O, negative when C falls below O and vanishes when C coincides with O. Again m is the tangent of the angle B A X or N O M,—that is

of the angle through which a line coinciding with O X would have to revolve about O in the positive direction, in order to become parallel to A B. If this angle is less than a right angle as in the figure, *m* is positive; if it be greater than a right angle, *m* is negative; and if it be a right angle, *m* vanishes.

The student should exercise himself in discussing these variations. It will be found, and we shall in future assume, that when a property has been established algebraically for any particular case the property is true in all cases if proper attention is paid to the directions in which lines are drawn and angles measured.

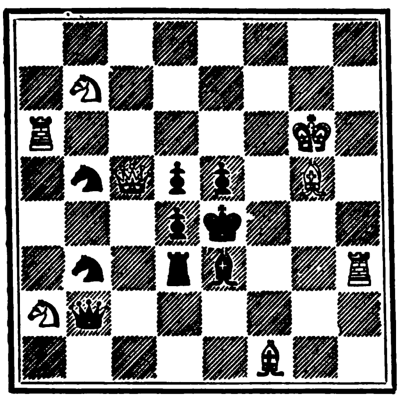
(*To be continued.*)

Our Chess Column.

SELECTED PROBLEM.

By G. L. Carpenter.

Black.



White.

White to play and mate in two moves.

HOME CHESS PLAY.

By Richard A. Proctor.

I HAVE found myself falling into the mistake which I noted in others. It is absolutely idle to undertake to teach the home Chess-player so many lines on each particular opening at Chess as I have run into already in dealing with the Philidor Defence, or as "Mephisto" gave in dealing long since with one form of the Giuoco Piano, and recently with the Allgaier Gambit. Only those who either play Chess professionally, or, being amateurs, are ready to give much more time to Chess than it really deserves, can possibly study the various lines of attack and defence which may be entered on in any opening studied down to the tenth or twelfth move, still less those which may be followed when the game is carried on to the twentieth move or so. To study these well enough to retain them always without confusing any sub-line with another, may be possible with those who have phenomenal memories, and will devote their powers in that direction to the Chess-openings, or with those who are possessed by the Chess-madness (which happily is seldom of long duration). But the average home Chess-player cannot learn all these lines, even when shortened as I proposed to try to shorten them.

Yet the home Chess-player need not therefore resign himself to play always at a disadvantage with the book-learned Chess-player. Under wiser guidance than that which I myself have given (hitherto) he can very easily learn all that is necessary for safety from such defeat as mere book-learning can give. Instead of learning what are the possible lines following from any opening, he should consider rather what are the only lines he need care to follow, whether playing the attack or the defence. It is very easy for him to study quite sufficiently a certain number of openings, and certain selected lines under such openings, without considering what would be best if his opponent played such and such moves along lines which he himself does not propose to follow. The idea entertained by many, and to some degree suggested by the method of treatment which Staunton, Wormald, Bird, and others have adopted in their guides to the game, is that, if you do not know at least as much as is contained in one of the standard Chess-books you are almost certain to lose against a player who knows them thoroughly, for he can select a line which leads to a bad game for you, whether you are playing the attack or the defence. But this idea is entirely erroneous. Nine-tenths, or more probably ninety-nine hundredths of the book-lore can be dispensed with altogether, if all you need is to be on even terms with the book-learned. All the openings dealt with in the books are interesting as Chess studies; but a knowledge of them is by no means necessary to Chess proficiency. They are most useful as Chess exercises, but not as a part of Chess knowledge.

Take for instance the opening with which I have been laboriously dealing in my last three papers,—Philidor's Defence. Well in the first place, as second player you can avoid the defence altogether, and all the trouble of inquiring into its complications—by the simple course of omitting to play 2. P to Q3 in response to 2. Kt to KB3. As first player you cannot, if you have played 2. Kt to KB3 (too valuable a second move to be left unused) avoid having to meet the Philidor Defence. But you can avoid every one of the lines I have so carefully considered by simply playing for your third move 3. B to QB4 or some other move which you may prefer,—instead of 3. P to Q4 as in every case thus far considered. Whatever line you thus take, you make all the analysis hitherto presented a dead letter. Of course on whatever line you thus enter there is *some* book-lore; but you have disposed of much more than is left. For you are not yet at the end of the saving effected by omitting to play 3. P to Q4 if you so please. Whatever move you take, the books if they show more than one reply for second player, show more than one subvariation in each. Whichever one he selects you can have your own selected subvariation and escape all others, at every step where the books give you choice of two or more lines. The process of sifting becomes indeed less and less effective at every stage, so far as the cutting out of unnecessary book-lore is concerned, but it is in greater or less degree effective to the last.

But the chances are that unless you yourself take to Philidor's Defence, you may never have occasion to oppose it at all; while when you do you have a steady and rather easy game to conduct. Practically by deciding, should you deem it well, never to play 2. P to Q3 in response to 2. Kt to KB3, you are enabled to pass over the whole chapter relating to the Philidor Defence, and to go on to an opening the study of which will (in that case) be more useful to you.

Now by acting on this principle, the *essential* part of the study of book-lore may be made a very easy and simple matter indeed. But manifestly Chess is a game worth some study. The question each player should consider is, how much time and attention he can afford to give to the game. When he has settled this, he can decide which openings, and which lines under each opening, he may deem it well to study. For instance, one who has scarcely any time to give to the study of Chess, but yet very much enjoys Chess play, and objects seriously (as many do) to being beaten through mere book knowledge, may decide to open always on some line not dealt with in the books, yet promising fair assurance of safety. There was a strong player at the Westminster Club for example, who, when first player, after P to K4 on both sides, would play his KKt to K2, and thence to KKt3. He beat Steinitz on a game thus opened, so that the possibility of the existence of first-class Chess power without knowledge of the book openings must be conceded. Or, if not quite so unwilling to look up book knowledge, which is really Chess experience, the player may limit himself to certain openings. With the first move he may always play Kt to KB3, after both Kings' pawns move out two squares; or he may always take for his second move either this or P to KB4, and so forth. With second move he may meet 2 Kt to KB3 *always* with Kt to QB3, and if a Ruy Lopez attack follows, may adopt a constant line of defence, or if an Evans gambit is proffered he may systematically decline it. So if at second move the first player proffers the KBP, second may constantly decline the gambit.

In such ways as these, a home Chess player may limit to any extent he pleases the amount of book-learning necessary for mere safety. But it should be hardly necessary to say that the more he studies Chess openings, even on lines which he may never care to follow in actual play, the more he will grow in Chess power. If it is excellent practice to watch two good players, or to play thorough good Chess games, how much more improving still must it be to examine lines of opening to the analysis of which many generations of the finest Chess players have devoted their powers.

I think I will ask "Mephisto"—my own work pressing rather too heavily on me—to give certain selected lines of opening which every home-player should thoroughly master;—leaving for the present the complete analysis of even any single line of opening to those who, if not professional Chess players, have much time to devote to the game.

With regard to "Mephisto's" first note on my last communica-

tion (KNOWLEDGE for May 9, p. 339), I may remark that although the second player has a cramped game,—especially after he has played (as I think he must) 7. P to QR3 and Queen back to her square,—it appears to me that White's attack is not so strong as it looks. If after 7. Q takes B—P to QR3, 8. Kt to Q5—Q to Qsq, White plays 9. B to KB4, to be followed by Castles Q side, &c., it seems to me that Black may with care develop an effective counter-attack by P to QB3, P to KKt4, B to KKt2, and so forth.

At a first view I thought that in the case inquired into by a correspondent (see the other foot-note on p. 339), White might take KBP, and if Black took Bishop, might regain the piece by checking with Kt at KKt5. But it is obvious that Black could take the Knight though supported by QB, White's own Queen being *en prise.*

(To be continued.)

Our Whist Column.

AVERAGE PLAYERS.*

THERE is a class of persons commonly described as "fair average Whist players." You may have met them; if not, you will recognise them from the following description: They have mostly played Whist, or think they have, for from 30 to 120 years—I don't think I have come across more than three who have exceeded the latter period, and even they were exceptions—they despise any one who has read a book on Whist, and consider him mechanical, themselves being bound hand and foot to half-a-dozen cast-iron rules, mostly wrong. I heard one of them rejoicing once that Mr. Clay was dead, on the mistaken ground that he did hope now he had heard the last of him. They detest Cavendish still more, though how he has injured them, who have never even seen his book, it is difficult to say. They are intensely superstitious, believe in numerous fetiches, and when they lose a rubber at once attribute it to some malignant influence of the cards and seats; if they have deliberately thrown it away themselves they are still of that opinion. With regard to their play—

(1) They lead singletons when weak in trumps and the game in no apparent danger; if they make a trump, which they might have done with equal certainty when the suit was led by somebody else, they are apparently much gratified; the effect on the mind of their partner is of no consequence.

(2) With two small trumps and a bad hand they lead a trump, and assign, as a reason, that their adversaries are at three, and that is the only way to save the game.

(3) When second player they never omit to ruff a certain trick of their partner's if weak in trumps, on the ground that it is the only chance of making a trump, thereby announcing loudly their weakness to their adversaries, and preventing their partner from developing any game he may have; even if moderately strong in trumps there is no certainty that they will not do this.

(4) They pass a winning card of the adversaries, if there is any possibility of being over-ruffed.

(5) They usually trump any winning card of their partner's below a ten, and below a seven invariably, whether any one else followed suit the round before or not,—unless very strong in trumps, when they will trump nothing for any consideration: they would sooner lose the game.

(6) They decline to return their partner's trump for several curious reasons. I. Because an honour being turned up to their right it is impossible for them to do so. II. Because they won with a Queen, and were afraid of killing their partner's King. If they won with a Knave, because the entire strength of trumps must be with the right hand adversary—the triumphant way in which he will make this remark is in itself almost enough to prove the average Whist player. III. Because they are able to ruff the suit on account of which their partner led trumps. IV. Because they thought their partner had no more, and many similar reasons.

(7) If with King, Queen, and another of their partner's lead, they make the Queen, they will carefully wait for hours in order to make the King. Those of them who by many years' practice have become skilful in annoying their partner, play the King first and then wait: but they are a rather superior class.

(8) With the adversary leading trumps they open a new suit every time they get in.

(9) They occasionally notice their partner's discard, but are compelled to lead that suit because they have a tenace in it and only two small ones in the suit asked for.

(10) If by any chance they see a call for trumps they always select the card most likely to deceive their partner as to what trumps they hold.

Now, will you kindly explain to these worthy people that all this is really not Whist, and that a better style of play might easily be acquired by, say, six hours a day steady practice for ten or fifteen years, without any necessity for reading any book at all.

You may enquire why, if they pay no attention to Moses and the Prophets, they should attend to you? *Why*, I am unable to say, but there is the undeniable fact that they do draw some line between a book and a periodical. In abstruse disputes, such as what is a revoke? is it a misdeal? they never think of looking at the rules of Whist, they don't believe in them, but they are always willing to refer the matter to the *Westminster Papers*. Again, I have often seen the average player with the *Papers* in his hand; that he was reading them I am not certain, but that shows they are not repugnant to his feelings, and I really think if you were to put the matter to them nicely they might alter their play to some extent, and it is quite unnecessary to specify what any alteration must be.

PEMBRIDGE.

* From a Letter by Pembridge to the Editor of the *Westminster Papers*.

ACCORDING to the *Engineer*, the Caledonian, North British, and Glasgow and South-Western Railways are rapidly fitting their carriages with the Pintsch gas apparatus; consequently, travelling in Scotland will, so far as light is concerned, soon be better than in England.

CONTENTS OF No. 133.

TERMS OF SUBSCRIPTION.

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The following arrangements are complete: the numbers in brackets referring to above list.

CAMBRIDGE, May 23 (3).
LEICESTER, May 26, 27, 28, 29 (1, 2, 3, 4).
RICHMOND (Star and Garter), June 5, 6 (1, 2).
NOTTINGHAM, June 11, 12, 18, 19 (1, 2, 3, 4).

NOTE.—*All communications respecting Lectures should be addressed to Mr. John Stuart, Royal Concert Hall, St. Leonards.*

KNOWLEDGE
AN ILLUSTRATED
MAGAZINE OF SCIENCE
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LONDON: FRIDAY, MAY 30, 1884.

CONTENTS OF No. 135.

THOUGHT-READING AND MUSCLE-READING.

By RICHARD A. PROCTOR.

MR. STUART CUMBERLAND has given a *quasi* private exhibition of what he calls his muscle-reading power, at the office of the *Pall Mall Gazette.* It is no injustice to say that the exhibition, however and by whomsoever arranged, was calculated to serve admirably as an advertisement of Mr. Cumberland's capabilities. He and Mr. Irving Bishop are rivals in the art of detecting or recognising—*somehow*—the thoughts of others. Mr. Bishop has achieved a few remarkable feats of that sort, and claims to be able, under suitable conditions, to read thoughts independently of muscular contact. Mr. Cumberland has been as successful under similar conditions, and denies that anyone can read thought otherwise than by noting muscular movements: he claims only exceptional skill in interpreting such movements.

Given Mr. Irving Bishop presenting the matter in one way, we might have expected to find a rival presenting the matter in another way. The claims of Mr. Bishop and the disclaimers of Mr. Cumberland (they must excuse me for not giving always their double-barrelled names) may be alike genuine. But they may also be unfounded. They may be alike clever attempts to duly impress a more or less gullible public. So far as the scientific question at issue is concerned neither one nor the other counts for anything. If I were an exhibitor of thought-reading, finding I possessed a thought-reading power which I could not myself understand and which at times seemed independent of actual contact, I fancy that were my object chiefly to attract attention and get money, I might be tempted to dwell somewhat on my supposed power of thought-reading as distinct from mere muscle-reading. But if I found some one else already in the field—with a double-barrelled name, suppose—I might think it "good business" to say that while *my* powers were at least as great as my rival's, so that I could tell all about the matter, I made no pretension to thought-reading apart from contact. This candid way of meeting the public would be very apt to tell in my favour, and against a "hated rival." (I should double-barrel my name, of course; *cela va sans dire*). By calling thought-reading without contact supernatural, and therefore impossible, I should get a pull upon my adversary, even though he asserted loudly that he claimed no supernatural power at all.

I think neither Mr. Bishop nor Mr. Cumberland has the slightest idea how they do what they undoubtedly can do. Each possesses a remarkable power of interpreting the thoughts of others. Each knows the power to be fitful and variable. Neither can make sure of success in any given attempt; yet each has met with some successes which cannot reasonably be interpreted as due to mere chance. Each has formed a tolerable idea of the unscientific credulity and of the equally unscientific incredulity of the public. One has thought it well to play on the former quality, the other deems it better to play on the latter. But as to a real idea of the nature of the power each feels he possesses, neither one nor the other has any whatever.

Of the experiments performed at the office of the *Pall Mall Gazette*, it may fairly be said that only two even approached the scientific character. The negative result in the case of Mr. Ray Lankester, and the apparently positive success in the case of Mr. Grant Allen, would be interesting and important, if *first* we knew that the former really gave that direction to his thoughts which Mr. Cumberland required, and if, *secondly*, we were fully assured that no outsider knew beforehand about the concealed object of which Mr. Allen was to think.

On the former point, with all confidence in Mr. Lankester's wish to be fair, I doubt whether his strong conviction that Mr. Cumberland could not in any way detect his ideas would not have had a greater influence in colouring his actual thoughts than any other subject which he supposed he was thinking about and really tried to keep constantly present before him. Mr. Cumberland's ready recognition that Mr. Lankester was a hopeless case might, for anything which appears, have been as good a bit of thought-reading as anything done that day at the *Pall Mall* office.

As to the other case, it presents some curious and noteworthy points. There is the possibility that the gentleman who concealed the loaf and suggested the experiment (the writer of the article, I believe) may not have been so careful as he might have been in concealing all traces of what he had done. Mr. Cumberland may have had some hint (from an outsider, of course) that such and such an object had been carefully concealed in such and such a place; and if he knew this, Mr. Cumberland might have had little difficulty in guessing that Mr. Grant Allen had that object in his thoughts and no other. I must confess that, granting the *bona fides* of the gentleman who concealed the loaf, whom I do not know, but of whom I am assured, by those who do know him, that he is confidently to be trusted in this matter, I think it unlikely that the concealment of the loaf would have been noticed by any outsider. But of course this is possible,—at least so far as the published evidence goes.

It is noteworthy, however, that, casually, the experiment was made much more effective than it would have been had Mr. Cumberland gone at once to the concealed object. Mr. Grant Allen, who is by no means a believer in thought-reading, nay whose disbelief verges in my opinion in the—let us say the extra-scientific, thought of the wrong house. Now Mr. Cumberland went to the house Mr. Allen was thus erroneously thinking about, not to the house where the loaf had been concealed. This certainly goes far to prove that by muscle-reading or otherwise Mr. Cumberland

was able to follow the direction of Mr. Allen's thoughts. It does more: it suggests *very* strongly that Mr. Cumberland had no information about the concealment of the loaf in a particular house: for if he had he would assuredly not have been led, by any muscle-indications, to the wrong house.

But there is yet another noteworthy point. Mr. Allen, when he arrived at the door of the wrong house, was wondering in his mind how Mr. Cumberland would get in, whether he would knock or ring. Mr. Cumberland, as if reading the doubt, resolved it by doing both. This may seem a small point; or it may have been a mere coincidence. But it appears to me significant. I do not think any muscular indications can conceivably have suggested the doubt. If they did, muscle-reading is at least as wonderful to my mind as thought-reading pure and simple. In any case Mr. Cumberland's resolute attempt to get into the wrong house confirms strongly the belief that he had no idea about the real position of the concealed object.

As to the question whether thought-reading without contact would be supernatural, I may remark that it would only be mysterious, not supernatural, nor would it be anything like so mysterious as the action of matter on matter without contact, &c., *instantaneously* over distances of hundreds of millions of miles. No man who admits—as every man of science must—that law of attraction which is the most stupendous mystery known to science yet rejects the idea that this great law of nature is not a natural law, need regard the idea of thought transference as involving aught of the supernatural. The influence of matter on matter without contact is at least as great a mystery as would be the influence of mind on mind at a distance.

I should have been glad if a few more men of science had been present, even though the main object of the experiments may have been questionable. The absence of Mr. Labouchere on the other hand was not to be regretted. One wonders, indeed, why he should have been invited. He has never shown any aptitude for scientific inquiry, but on the contrary a very marked development of unscientific incredulity—a quality as fatal to success in seeking after truth as unscientific credulity,—which also, by the way, as usual with unscientific disbelievers, Mr. Labouchere has often displayed. In such matters his one argument has been blatant abuse, his one test *a wager!* A man who has proposed to settle a matter of this sort by laying a wager has *ipso facto* pronounced himself unfit for scientific discussions.

DREAMS:

THEIR PLACE IN THE GROWTH OF PRIMITIVE BELIEFS.

By Edward Clodd.

INTRODUCTORY.

In a series of papers contributed by the present writer to the earlier numbers of this journal, and since reprinted in the volume entitled "Leisure Readings," the evidence as to pre-historic man's material furniture and surroundings was summarised. Although this evidence was in the main gathered from places with which we are most familiar, it is supported by that which has been collected from every part of the globe inhabited in past or present times, and its uniform character has therefore enabled us to determine what lies beyond an horizon which within the last half century was bounded by the hazy line of myth and tradition. So rigid seemed the limit defining man's knowledge of his past that some forty years ago even the Geological Society passed over with barest reference the unearthing of relics witnessing to his presence in Britain hundreds of thousands of years ago. The canon was closed, and no one ventured to add to the sayings of the book. But the discoveries which had disproved belief in the earth's supremacy in the universe, and in its creation in six days, led the way to researches into the history of the life upon its surface, and especially of that which, in the language of ancient writ, was "made in the image of God." When the long-forbidding line, imaginary as the Equator and lacking its convenience, was crossed, there was found the evidence of the conditions under which man emerged from a state quite other than that which had formed the burden of legends sacred with the hoariness of time. Those conditions, it is wellnigh needless to remind the readers of Knowledge, accord with that theory which holds man to be no specially-created being, started on this earth fully equipped, Minerva-like, with all ripeness of wisdom and loftiness of soul, but the last and long result of an ever-ascending series of organisms ranging from the lowest, shapeless, nerveless specks to *homo sapiens*, "the foremost in the files of time." Evolution is advance from the simple to the complex. The most primitive forms reach maturity in a shorter time than the higher forms, and fulfil their purpose quicker, and this doctrine applies not only in relation to man and the inferior creatures, but as between the several races of man himself. Herein the differences, which are determined by size, still more by increase in complexity, of brain-stuff, are greater than between the lowest man and the highest animals. That is to say, the savage and civilised man are farther apart than the savage and the anthropoid ape. The cranial capacity of the modern Englishman surpasses that of the aboriginal non-Aryan Hindu by a difference of sixty-eight cubic inches, while between this Hindu skull and the skull of the gorilla the difference in capacity is but eleven inches, and if we were to take into account the differences in structural complexity, as indicated by the creasing and furrowing of the brain-surface, the contrast would be still more striking. The brains of the earliest known races, the men of the ancient Stone Age, ape-like savages who fought with woolly-haired elephants, cave-lions, and cave-bears, amidst the forests and on the slopes of the valleys and hills where London now stands, and who in the dawn of human intelligence, applying means to ends, came off victorious, were doubtless much nearer to the chimpanzee with his thirty-five cubic inches than to the Papuan with his fifty-five cubic inches. Indeed, we need not travel beyond this age or island; it suffices to compare the brain-quality of the rustic thinking of "maistly nowt" (one is inclined to class the "masher" with him) with that of the highest minds amongst us, as evidence of the enormous diversity between wild and cultivated stocks in mankind.

Unless we are so enchained to fond delusions as to place man in a kingdom by himself, and deny in the sympathetic, moral, and intellectual faculties in brutes the germs of those capacities which, existing in a pre-human ancestry, have flowered in the noblest and wisest of our race, we may find in such differences as are shown to occur between civilised and primitive man, further evidence of the enormous time since the latter appeared. For unnumbered ages man—then physically hardly distinguishable from apes—may have remained stationary. Certainly the relics from the Drift show no advance. Given no change in the conditions, the species do not vary, and man, once adapted to his surroundings, changed only as these changed. But, obscure as are the causes, there came a period when conditions arose which induced some variation—no matter how slight—in brain development, which was of more need than any

variation in the rest of the body, and when an impetus was given which, leaving the latter but slightly affected, quickened the former, so that man passed from the highest animality to the lowest humanity. Slowly, in the course of a struggle not yet ended, "the ape and tiger" were subdued within him, and those social conditions induced to which are due that progress which ever draws him nearer to the angels.

The discussion of this in detail lies outside the limits of these papers. Here, after briefly noting on what lines it must run, we are concerned with man at that far later stage in his development when the physical and material evidence respecting his bodily development gives place to the psychical and immaterial evidence respecting his mental development. Chipped flints, flakes, and scrapers of the Drift are indispensable witnesses to his primitive state, but during the long ages that he was making shift with them, he remains within the boundaries of the zoological; he is more geological than human. Gleams of the soul within that will one day be responsive to grace of form and harmony of colour appear in the rude portraits of mammoth, reindeer, urus, whale, and man himself, scratched on ivory and horn; indications of germinal ideas about an afterlife are present in the contents of tumuli with the skeletons in defined positions and weapons presumably for the use of the departed in the happy hunting-grounds. In these last we are nearing the historic period, for a vast interval exists between the tomb-building races and the men of the Reindeer Period, yet even then the ages are many before man had so advanced as to bequeath the intangible relics of his thought, disclosing what answer he had beat out for himself to the riddle of the earth and the mysteries of life and death. Although the story of his intellectual and spiritual development is a broken one, of the earlier chapters of which we can have no record, enough survives to induce and strengthen the conviction that in this, as in aught else, there is no real disconnection. In the shaping of the rudest pointed flint-tool and weapon there are the germs of the highest mechanical art; in the discordant war-whoop of the savage the latent strains of the "Marseillaise," as, quoting Tennyson, in the eggs of the nightingale sleeps the music of the moon. And if we cannot get so near to the elemental forms of thought as we could wish, we must lay hold of the lowest extant, and trace in these the connection to be sought between the barbaric and the civilised mind. We must have understanding of the mental condition of races, still on low levels of culture, and if the result is to show that many highly-elaborated beliefs among advanced peoples are but barbaric philosophies "writ large," the conception of an underlying unity between all nations of men that do dwell, or have dwelt, on the face of the earth will receive additional proof.

It is therefore with some illustrations of this that our next paper will be concerned.

THE SATELLITES OF MARS.*

By Prof. Payne, Editor of the *Sidereal Messenger.*

ASIDE from the books written by Professors Newcomb and Holden, and the paper by Professor Hall, very little has been published concerning the satellites of Mars. Though several years have passed since the discovery of these small bodies, scarcely anything new can now be added that will interest the student of astronomy, or the general reader who has access to these valuable books and paper above mentioned. Although this be true, it seems desirable, from the queries of recent correspondents, to make a restatement of what is known of the satellites of Mars for the convenience of readers who may not have the information at hand.

This notable discovery was made August 11-17, 1877, by Professor Asaph Hall, of the Naval Observatory, Washington, D.C. It was not accidental, for Professor Hall began the study of the subject in the spring of that year, examining all authorities within reach, and when the time of opposition came, a systematic search with the great equatorial was undertaken, and prosecuted through discouraging circumstances until the two moons were found, and until it was settled that there were two and not three, as at one time supposed, on account of the rapid motion of the inner one.

It is impossible to convey in words [!] the impression that this discovery made on the minds of leading astronomers everywhere. It was justly esteemed a triumph of patience and skill of high order, to which will be given appropriate place in the history of modern astronomy.

Of the many names suggested for the satellites of Mars, Professor Hall chose those suggested by Mr. Madan, of Eton, England, viz.:—

For the outer satellite, Deimos.

For the inner satellite, Phobos.

The classical scholar will remember that these are the names which Homer applies to the horses which drew the chariot of Mars, and that the passage, as translated by Bryant, reads:—

> "He spoke and summoned Fear and Flight to yoke
> His steeds, and put his glorious armour on."

The time chosen for the search was very favourable, on account of the relative positions of Mars and the Earth. This happens (*sic*) when Mars is as near the Sun as possible, and the Earth as far away as possible, and Mars opposite from the Sun, and can occur only once in fifteen years.

In 1877 these conditions were nearly fulfilled, and the planet Mars was, at nearest approach, about 36 millions of miles from the earth. The sun was in apogee July 3, and Mars was in perigee August 21, and opposition occurred September 5. If these phases of the planets had taken place nearer the same date, the distance between them would have been less, and by so much, observation more favourable.

These satellites are the smallest celestial bodies known. In the largest telescopes they appear as points of light, and hence cannot be measured at all in the ordinary way. The inner satellite is a little brighter than the other, and, therefore, possibly a little larger. Their diameters are probably less than ten miles, and are assumed to be about six and seven miles respectively.

The outer satellite, Deimos, revolves around the planet in 30h. 16m., and the inner one, Phobos, in 7h. 38m. The latter is 5,800 miles from the centre of Mars, and less than 4,000 miles from its surface. It would, therefore, be quite possible with one of our telescopes on Mars to know if the inner satellite is inhabited, for the motion of living beings could be easily recognised.

With these facts before the mind, it is impossible to realise the problem of difficult seeing that was mastered by the aid of the telescope, in observing these minute bodies at the distance of 36 millions. The outer satellite was seen at a distance of about six or seven million times its own diameter; as Professor Holden has aptly said, it is like suspending a ball two inches in diameter and viewing it with a telescope at a distance equal to that between Boston and New York. "Such a feat of telescopic seeing

* From the *Sidereal Messenger.*

is well fitted to give an idea of the power of modern optical instruments."

The rotation-time of the planet Mars is 24h. 37m. As before said, the time of revolution of Phobos is 7h. 38m., so that the satellite evidently would revolve more than three times about the primary while it was making but a single rotation on its axis. To an observer on Mars this curious relation of the two motions would cause most interesting physical phenomena, some of which may be briefly noted here. It will be remembered that the motion of the planets around the sun, and all the satellites, with two exceptions, around their primaries, are in the same direction, and that is from west to east. This is also true of the rotation of the planets on their axes so far as known. Now, if this be so, the apparent diurnal motion of a satellite in the heavens will be approximately the difference of the two motions. If the planet rotates on its axis more rapidly than the satellite revolves about the common centre of gravity, then the satellite will appear to rise in the east and set in the west. This is constantly observed in the case of the Moon; also in Deimos, whose revolution period is 30h. 16m. [But] this satellite's motion is a little slower than that of the planet, and so there would be a slow westward apparent diurnal motion that would be very strange to people like ourselves. On the other hand, because of the rapid motion of Phobos, it would appear to rise in the west and set in the east, and because the relative motions of planet and satellite are greatly different, the apparent diurnal motion of this satellite would be as surprisingly rapid to an observer on Mars as that of Deimos was slow. The phenomenon of two satellites rising at the same time—for example, one in the east and the other in the west—and the latter sweeping through the sky eastward, at the rate of about 45° per hour, and, in less than four hours, meeting the other a few degrees above the eastern horizon on its slow westward march, would be a view at first sight almost as impressive as Mitchel's first sunset in Adam's time. Add to this the supposition that the planet Mars is inhabited by beings not greatly unlike ourselves, with telescopes or some other means of aiding the natural powers of sight equal or superior to those we enjoy, and the field of speculation is limitless and interesting to those possessing a lively imagination.

THE EVOLUTION OF FLOWERS.

BY GRANT ALLEN.

LILIES AND RUSHES.

ON the dry, sunny hillsides behind Mentone, there grows in early spring a pretty little blue lily, erect and usually solitary on the end of a long, stiff stem, and singularly rush-like in its mode of growth and general appearance. It rejoices in the scientific name of *Aphyllanthes Montpeliensis*. As a rule, I don't care to go outside our own wild British flora, or the common cultivated flowers of our gardens, for my main examples, because it is best as far as possible to deal with well-known cases, where the reader's personal interest and memory count for something; and small as our native collection of plants really is, it generally affords quite as good illustrative examples of the various levels of evolution as could be got by ransacking the hothouses at Kew for the rarest and most unfamiliar exotic palms or orchids. This little blue lily of the Riviera, however, with a few of its congeners in Australia or the Malay Archipelago, forms such a beautiful specimen of a bridge or connecting-link between two somewhat distinct families that I am tempted to step aside from my usual practice for once, and interpolate a foreign plant among our illustrations of the evolution of flowers.

Aphyllanthes, as its Greek name imports, has no true leaves; its foliage has all been transmuted into short dry sheathing scales, which clasp and protect the base of each flowering stem. The work usually performed by the leaves—that of taking carbonic acid from the air to be built up into the living material of the plant as starch or cell-wall—is undertaken in this curious lily by the tall, green, rush-like flower-stalks alone. At the top of each such stalk, a few dry, brownish bracts form an involucre or protective covering for the unopened buds; and within this involucre, in due time, a single sky-blue flower (more rarely accompanied by one or two more) opens its bright blossom to the Italian sun. As in the true lilies, there are three sepals and three petals, all alike beautifully coloured; but one of the bracts (or very reduced leaves) here serves the place of a calyx, as the true calyx has been perverted from its original function to share in that of the corolla. There are six stamens, as usual, in two whorls—the three outer somewhat shorter than the three inner; and the pistil consists of three cells, welded firmly together into a single ovary. In short, so far as technical characters are concerned, the Aphyllanthes is a true lily. A botanist who went strictly by the artificial marks set down in the text-books would have no difficulty at all in deciding that it belonged to the true lilies, and not to the rushes. For the distinguishing mark of a rush, in formal botany, consists in the fact that its perianth is "dry and scarious, not petaloid;" and as the perianth of Aphyllanthes is

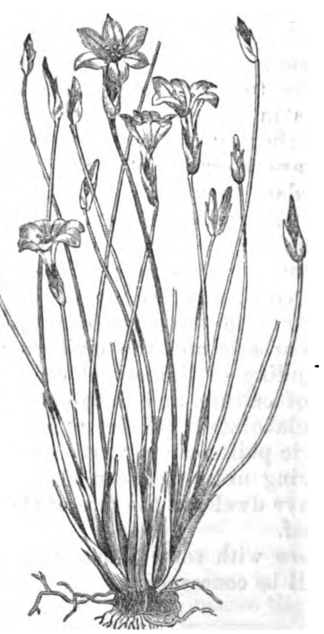


Fig. 1.—Aphyllanthes Montpeliensis.

bright and juicy, of course it is a lily by definition. But if you look at the accompanying figure of our common English rush, you will see that it differs essentially from Aphyllanthes in hardly anything except the comparative size and colour of the perianth. To be sure, the flowers on each stem are much more numerous in the rush; but that, as we know, is a very small matter; in all other respects the resemblance between the two is extremely noticeable. To put it briefly, Aphyllanthes is a lily far gone on the road toward the rushes—an arrested stage, no doubt, in the development of the family; while the rushes are lilies like

Aphyllanthes which have given up producing brightly-coloured perianths, and taken to small, dry, brown, inconspicuous little blossoms instead.

Why is this? Well, the rushes afford us an excellent example of flowers in a retrogressive condition, though in their case the degeneration has not gone far, and has produced no evil effects upon the habits of the species. All the plants that we have yet considered have been fertilised by insects; the rushes have gone back to the possibly older, but somewhat wasteful, habit of being fertilised by the wind. The pollen is shaken out from their little, loose, hanging stamens by every puff of the summer breezes, and then floats away till it is caught by the three long, feathery stigmas of some other flower. These two conditions of the stamens and stigmas are very characteristic of all wind-fertilised blossoms: the pollen-sacs always hang out in a very mobile fashion, shivering and quivering before the faintest breath, while the stigmas protrude boldly from the centre, and are minutely divided into tiny plume-like points, which catch and retain every grain of the fertilising dust wafted to them by the unconscious breeze.

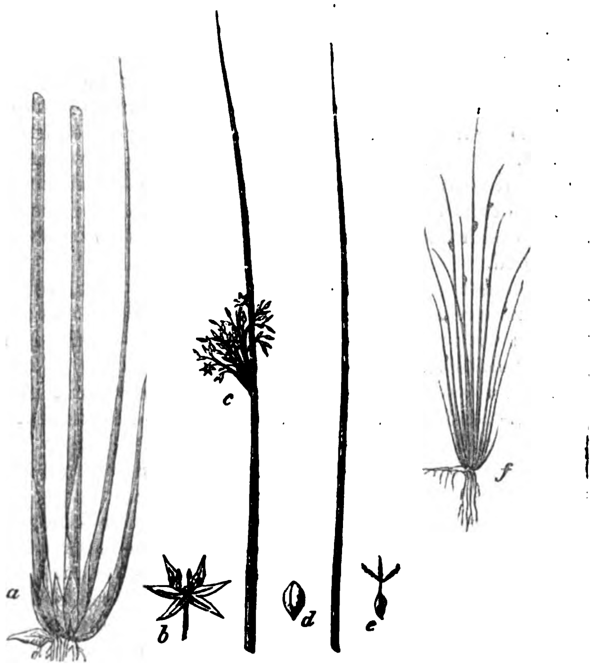


Fig. 2.—Juncus communis.

But how do the rush flowers, then, escape the chance of self-fertilisation with its attendant evils? By a very simple but effectual contrivance. The stamens and stigmas of each blossom do not come to maturity together. The pistil is the first to ripen, and it protrudes its three tiny plume-like stigmas through the bud, before the six dry perianth-pieces begin to unfold; thus, it is pretty sure to catch a grain or two of pollen, carried towards it from some neighbouring head. Meanwhile, the stamens of its own flower are safely huddled up within the tightly-closed perianth. But, by-and-bye, the perianth in turn opens, the stamens unfold themselves upon their long, thin foot-stalks, and the pollen-sacks split down their sides, and shed the pollen lightly to the breeze for the benefit of other surrounding flowers. This is a common device with many wind-fertilised blossoms.

Thus we see that the slight differences between the rushes and Aphyllanthes are solely caused by a single fact in their respective economies; the one is impregnated by the wind, and the other by insects. If the rushes were to take to the habit of insect fertilisation they would doubtless soon acquire brightly-coloured petals like those of the lilies; if Aphyllanthes were to take to the habit of wind-fertilisation it would doubtless soon lose its bright petals, because it would have no further need for them. Nay, they would even become a positive disadvantage to it, inasmuch as they would induce insects (for whose utilisation as carriers it was no longer adapted) to come and plunder it undeterred of its precious pollen. Observe, however, that the rush has not entirely lost its petals, now that they are no longer of use to it as coloured advertisements; it has merely found a new mode of employment for them. By making them hard and dry it has turned them into a protective covering for the stamens before they mature, and as they are persistent (that is to say, do not drop off after flowering) they serve once more as a similar covering for the seeds and capsules during the ripening process. Such economy of existing structures meets us everywhere in Nature as an ordinary accompaniment of evolution.

The flower of the rush is still, however, essentially a lily, with three sepals, three petals, three outer stamens, three inner stamens, and a three-celled ovary, bearing a united style with three separate stigmas. In the common rushes, the seeds are also numerous in each cell, as in the simpler lilies; though in Aphyllanthes and the wood-rushes, they are reduced to one each, for a reason to which I must recur hereafter.

I have left myself hardly any room to notice the most conscipuous external peculiarity of the rushes with which we are usually most familiar. I mean the cylindrical, almost hollow, pithy leaves. But it is easy enough to see the use of these stout, strong, and often prickly-tipped organs; they are, of course, admirably adapted for the places in which rushes commonly grow, in wet, marshy spots, and they serve to protect them against being either trodden down or eaten by cattle. The cylindrical form, however, though frequent among the rushes, is by no means universal; and we can trace every intermediate stage, from the quite flat, grass-like, or lily-like foliage of the two-flowered rush (*Juncus biglumis*), through channelled leaves with a fine cylindrical tip like the chestnut rush (*J. castaneus*), to those in which the whole leaf has become cylindrical throughout, like the sharp rush (*J. acutus*) whose very stiff, prickly points are nasty things to pierce one's hand with on the coast in Devonshire.

THE INTERNATIONAL HEALTH EXHIBITION.

BY JOHN ERNEST ADY.

I.—INTRODUCTORY.

THE odium of comparison sinks into oblivion when we come to apply it to the excellent appointments which have recently been instituted over the site of last year's "Fisheries Exhibition" at South Kensington. Rather let us look upon the arrangements for this year in the light of progressive development; and, when viewed thus, we are insensibly led to congratulate all those who are concerned with the direction of its management.

Of the more obvious improvements we may more specially notice those which provide for the comfort and convenience of visitors. The covered archways, the awned out-of-door spaces, and the judiciously selected spots for refreshment-stalls and their adjuncts. Nor must we omit to mention the superior attractions which appeal more directly to our

sense of the beautiful; the olden houses of London, the lovely illumination, the galleries of costumes, and the musical provision.

It is not within our province to record the minutiæ of the Exhibition as a whole, as a resort for mere amusement, nor will we attempt to give the reader an account of how to make the most of an afternoon at the place, by a hurried and meagre sketch of the multitudinous objects which are to be seen. That has already been done in the daily newspapers, and in sundry brochures both official and non-official. It is necessary, however, that our readers should seek out some such information, in order that they may be enabled to follow us out more fully; and, to that end, we would direct their attention more specially to one of the official guide books, or to the excellent popular reports issued by the *Pall Mall Gazette*, entitled "The Popular Guide to the Health Exhibition," which may be obtained only outside of the buildings, and may as readily be recognised from afar by its hideous orange-brownish paper cover.

In the *Pall Mall Gazette* Guide the author launches forth into an introductory ridiculism, if we may so term it, of the title "Health Exhibition," and the heterogeneity of its contents. Now, if we look somewhat more carefully into the matter, we will be led to discover that the distribution of the, what might be called, "true sanitary exhibits" has been so thoughtfully arranged throughout the allotted space, and so intermingled with its "indirectly sanitary shows," that an end of the greatest importance, viz., the "fitness of all things," is so established. The visitor who comes to idle away a few hours is not surfeited with a continuous round of pleasure; his enjoyment is tempered every now and then by the contemplation of something substantial and useful. The sanitary student, on the other hand, who comes intent on study is pleasurably surprised, and imperceptibly led to find how much easier his task becomes under the soothing influences of a *dîner à la Duval*, with music thereafter, and when his eye is delighted by exquisite dress, Doulton's potter's art, beautiful furniture, or quaint houses; and then to see the living cows and goats milked! Why, after that, he can go on his sanitary tour with redoubled vigour. But, after all, the veriest pleasures which allure one have their bearing upon health; and some of them, especially those concerned with dwellings and dress, to a very marked degree.

It will be our duty hereafter to make notes for the benefit of our readers of such of the exhibits as bear more emphatically upon health, to point out the advances which they have made over past sanitary measures, and to comment upon them. To submit analyses of such as conduce to personal prosperity and comfort, and to point out how the salubrious condition of small and large communities is and ought to be dealt with. In short, ours shall be a series of "*popular* Health Exhibition reports," equally adapted to the wants of those who desire to attend and view the Exhibition, as to others who are compelled through fortuitous circumstances to remain at home.

From the nature of the subject, and our statements above concerning the arrangements wisely adopted by the Executive Council and Committees, it follows, that much of what we shall treat of will be fragmentary; but our best endeavours shall be exerted to make each fragment perfect. Thus, were we to "follow our nose," we would have to comment upon bronze statues and electroliers, bibles, cabbages, and parasitic pests, vile vegetable dinners, dairies, and such a multifarious crowd of incongruities, that our readers would be justified in using strong "language, not intended for publication, but merely given as a guarantee of good faith." We have, therefore, decided to treat of special exhibits in isolated groups, and to complete, as far as possible, one item in each article.

Let us now conduct our reader to the main entrance of the Exhibition, and thence along the main South Gallery to its termination. We shall have occasion, in future papers, to revisit this "foods and feeding" section of the buildings to notice many things of extreme interest; but, for the present, we shall turn to the right and ascend the staircase leading to the Aquarium—a remnant of last year's Fisheries Exhibition. As noted in our pages at that time, Mr. Maignen's "Filtre Rapide" attracts our attention here, and we are once more constrained to notice this admirable contrivance. We refer to our former pages for an explanation of the essential construction of these water purifiers; and what we then stated concerning their efficiency we are now able to reindorse, after a year's constant usage. In spite of the utmost care of the water companies, whose grand pavilion we shall visit ere long, contamination products do often creep into our household pipes and cisterns: that is unavoidable. But surely it must be the fault of the householder when such useful and inexpensive filters are to be had. M. Maignen has greatly extended his exhibit, and, *pro bono publico*, Filtre Rapide water may be quaffed *ad libitum* in the grounds of the Exhibition, where some of his fountains are set up. The water of the aquaria, too, which has been under his care, seems to be thoroughly purified.

In passing through the Aquarium last week, we observed that nearly all the cod-fish in one of the tanks were infested with numerous brownish parasites or fish-lice (probably *Entomostraca* of the genus *Argulus*). We tried to obtain a few of these for careful examination; but, alas! at the end of the week they had all simultaneously vanished, leaving the fish quite healthy. In another tank a poor solitary Lump-fish (*Cyclopterus lumpus*) attracted crowds of observers to view his uncouth form; he was literally swarming with pests of a peculiar kind, possibly the result of his confined, non-tempestuous prison. Last week he died, but we were not able to procure a single parasite; they had all fled from the presence of death, and were not anywhere to be found. Tank No. 10 deservedly attracts the attention both of naturalists and lovers of aquarian beauty. It is full of exquisite sea-flowers or anemones (*Actiniæ*), many of which were described last year in our columns by Mr. Kimber. Their healthy condition, we find, is largely due to the care bestowed upon them by their energetic keeper, Mr. Edel. Amongst other old friends we may notice the funny little sea-horses (*Hippocampus*) in small aquaria of their own, the curious, spider-like King crabs of the Moluccas, which is, indeed, by some eminent biologists, said to be a kind of spider (*Arachnid*), and the lovely, quiet, golden tench from the Duke of Bedford.

Behind the Aquarium, the new Fish Culture Department is localised; and here may be seen a large tank of pellucid water, with some fine trout and other *Salmonidæ*. The Canadian salmon, which were hatched out last year in the "Fisheries Exhibition," may also be seen thriving here.

From this section we shall pass, next week, into the parallel adjoining Western Gallery, to make the first of our special health inquiries into the processes employed in the aëration of soda-water, beer, fruit, and other drinks. Our remarks will be fully illustrated with engravings.

Car Wheels in the United States.—The number of car wheels in use in the United States is estimated at 10,000,000. About 1,250,000 tyres are worn out each year, requiring about 312,500 tons of iron, about eleven-twelfths of which are supplied by the used-up wheels themselves, which are broken up and recast.

SENT TO THE BOTTOM.

By Richard A. Proctor.

(*Continued from page* 365.)

MORE might, of course, be done to make this system of lighting effective. I pass on somewhat doubtfully to note what might thus be tried, because there is a natural and proper unwillingness on the part of most sea captains to have any observations at all nice or difficult to make in time of sea risk. A friend of mine has found very little favour accorded to him (and really he had little right to expect any) when he proposed at a recent meeting of the British Association that when a ship is approaching shore and the weather is thick, the captain or chief officer should make microscopical examination of the water to see what evidence the matter floating in it might afford respecting the neighbourhood of land. Nor again was any very enthusiastic reception accorded by experienced seamen to his idea that in thick weather chemical analysis of the water (taken from before the bows) should be made for evidence of the neighbourhood of any other ship which she might be approaching. I have been at sea under both conditions, and certainly as a passenger I should have been sorry to see these measures adopted instead of the old-fashioned and less scientific ones. In January 1881 the *City of Sydney*, under the command of Captain Dearborn, was approaching San Francisco in thick weather after four or five days during which no observations had been possible. Captain Dearborn did not have water taken on board for microscopical or chemical analysis. I fancy if any one on board of scientific proclivities had come along with a bucket of the sea water and a microscope or means for chemically testing it, and had invited the captain and his officers *very* persistently to investigate the chemical qualities of the water or to seek for evidence about matter held in solution in it, he would have stood a good chance of a rather severe rating even from so courteous a commander as Captain Dearborn. That gentleman had a pestilent habit (to use a phrase Dickens has employed, though it is not quite the right one, even humorously applied) a pestilent habit of attaching very great importance to the seamanly duties on which the safety of the ship and lives entrusted to his care depended; and I have a shrewd suspicion that even the wish to be polite and pleasant would not have caused him to waste a single minute from those duties on the scientific twaddle, which measures too refined for times of danger at sea assuredly involve.

But there are some things which might be learned from the signal lights I have described at a single view, if more were desired than that general information which, as I have shown, those lights would at once afford.

Suppose for instance that the three lights forming the midships triangle were set at a fixed distance from each other in all ships. That distance need not be great, for when a ship is far away it would not be very important that the side lights forming a triangle should be separately seen by unaided vision. If the ordinary night-glass separated them that would do very well. Suppose they were a yard apart. Then the two which are in a vertical line would always lie at their true apparent distance,—apart from rolling and pitching, which since they only carry the hull from one side to the other of her mean position, have no real bearing on the result. In other words there would be no foreshortening in their case as in the case of the other sides of the equilateral triangle. So that even to the naked eye, or as seen in a telescope constantly in use (so that its magnifying power was known) they would give a good idea of the distance of the stranger just as the apparent height of a soldier gives the practised observer an idea of his distance, as seen in a familiar military exercise for judging distances. But it would be easy to arrange matters so that using the ordinary night-glass an officer could at once determine the distance of a strange ship from the apparent distance between her vertical pair of side lights. Three or four fine parallel equi-distant threads, fixed in a small diaphragm at the focus of the telescope's eyepiece, would at once tell this, if they were set at such a distance from each other—say a yard—as seen through the telescope at a distance of 100 yards, would exactly correspond to the distance between two threads in the field of view. This would correspond, if there were five threads, to the span between the two outermost for a distance of 400 yards.

There would be another advantage about such an arrangement as this, in the circumstance that the position of the stranger's hull could be more precisely determined than without such threads. Thus, suppose the apparent distance between the two vertical lights were seen in the night-glass to be four of the spaces between the threads, while turning them horizontally the distance of the forward light of the three from the line joining the others were found to be only about one and three-quarters of the spaces: that would signify that the stranger's hull was inclined about thirty degrees to the line of sight, and so on for other angles.

The field of view of the night-glass might easily be divided into squares by two sets of cross threads, by which arrangement a single observation, or one may say a simple look (by a practised eye) through the telescope, would show the distance of the stranger and the exact position of her hull.

If it seemed better—and there are some considerations which suggest that it might be better—to have two of the three lights at the same level, a right-angled triangle, (with two equal sides) might be substituted for the equilateral triangle; thus—

* *   *
instead of   * ;
*   *

* *   *
and   instead of *
*   *

in this case the two upper lights would be seen simultaneously when as yet the distance of the stranger was so great that the convexity of the water-surface concealed the lowermost from view; and a ship's side-lights would then be seen farther off, the two upper lights showing at a distance as a single light of double the luminosity of either. But so far as the risk of collision is concerned, there would be no special advantage in this arrangement, as there is small risk of collision in any case where a ship's lights can be sighted while she is as yet almost "hull down."

(*To be continued.*)

OPTICAL RECREATIONS.

By a Fellow of the Royal Astronomical Society.

(*Continued from p.* 352.)

WE may take it, for our present purpose, that geometrical optics—with which we shall first deal—has to do with three separate phenomena. (1.) The passage of light through a homogeneous medium (including a vacuum):

which, we have seen, occurs in straight lines. (2.) The effect produced when it passes from such homogeneous medium (or vacuum) into another medium—or back again—and (3.) The behaviour of light incident on what is called a reflecting medium, through which it cannot penetrate and from which it is thrown back. The last shall be first. If we take up any book on optics, we shall find a law enunciated in these (or nearly identical) terms. "When a ray of light is incident upon a reflecting surface, the reflected ray is in the same plane with the incident ray, and the angles of incidence and reflection are equal." The simple piece of apparatus shown in Fig. 5 will serve to

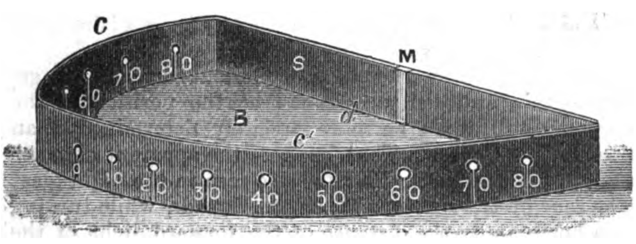


Fig. 5.

illustrate this. Get a piece of board, a foot wide and $\frac{5}{8}$ths inch thick when planed up, and cut out of it a semicircle, B, 12 inches in radius. Against the diametrical edge *d* of this is to be glued or screwed a rectangular piece of board, S, two inches high and $\frac{1}{4}$-in. thick. In a groove in the middle of this, over the centre of the semicircle, is securely fixed a little slip of looking-glass, M, accurately upright, or square to the surface of B. Now we take a strip of stout cardboard, C C′, also two inches in width; fit it carefully round the semicircular edge of B, and cut it off at its ends, where it joins on to the piece S. Having thus gauged its length, we must divide it accurately into eighteen equal parts, drawing lines through such divisions square to the length of the cardboard; so that when the latter is replaced our lines shall be upright. On each of these lines a small hole is perforated, as shown in our figure, and we then mark the middle division 0, those on each side of it 10, the next two outer ones 20, and so on to 90—the two 90's falling on the inner edges of the ends of the board S. Pretty evidently each of our divisions will represent 10°. Finally, we must refasten the strip of cardboard round B by glue and tacks, and so adjust it that when we look through the small aperture in the middle, marked 0, its image shall be seen centrally in our strip of looking glass. Let us place this little contrivance on a table, and light the hole marked 50 by placing a candle close behind it. Then, if the eye be applied to the opposite hole with the same mark, the little lighted circle will be seen shining in the mirror. In any other position of the eye, however, it will be invisible. This experiment should be repeated with the other corresponding holes; or, instead of looking *through* the one opposite to that through which the candle is shining, it may be looked *at* from the inside, and will be seen to be illuminated, while to the right and left of it is darkness. Now the line drawn from 0 square to the surface of the mirror M is the perpendicular at the point of incidence, and a mere glance at our apparatus will show that the angles 60 M 0, 60 M 0, 20 M 0, 20 M 0, and so on, are absolutely equal, and this is what is meant by the law enunciated above. The way in which this law operates in producing the reflection of an image from an ordinary mirror will be easily comprehensible from Fig. 6.

Here we perceive a lady regarding herself in a looking-glass, LL′, the rays of light from her eye, E, falling perpendicularly on to the mirror, will obviously return by the same route, and she will see an image of her own eye precisely in front of it, at E′. The rays from the top of her head, T, will strike the glass at *p*, and if we there erect a perpendicular, we shall find that the reflected ray will follow the direction, *p* E, and, as we always see an object

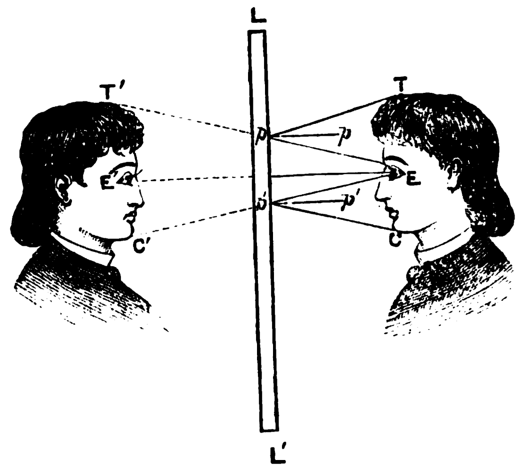


Fig. 6.

in the precise direction in which the rays emanating from it reach the eye, this will seem to come from T′. Exactly the same reasoning applies to the pencil of rays from the observer's chin, C; whence it will be seen that, by the combination of the optical images of every part of her face, her reflected image will be a facsimile of herself (save that obviously it must be reversed right for left), and must appear exactly as far behind the surface of the mirror as she herself is in front of it. A little thought will show that two such mirrors placed parallel to each other, would produce an infinite number of reflections of an object placed between them, but for the loss of light, which ultimately renders them invisible. A pretty and effective toy, shown in Fig. 7, will illustrate this.

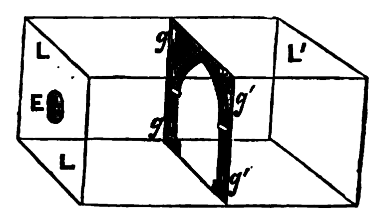


Fig. 7.

Make a box a foot long, six inches wide and six inches deep, and at each end place a piece of good, thin looking-glass, L and L′, six inches square. In the middle of one end of the box a small hole, E, is bored to look through, and the quicksilver must be scraped off that part of the back of the mirror which covers this hole, in order that it may offer no obstruction to vision. Then in the middle of the box must be placed a cut-out scene, painted on cardboard, of some object, whose repetition gives the elements of a picturesque view. Overhanging trees, a single bay of the Crystal Palace, or an arch in a cathedral nave, will suggest suitable subjects. A favourite one with ourselves in our youth used to be an arch of the Thames Tunnel. The cardboard should be similarly painted on both sides, and requires to be very well lighted. Looking through the eye-hole, E, a perfectly interminable vista is seen, and the spectator seems to be gazing along an

endless avenue of trees, or arches, as the case may be. Grooves at g, g' enable the view to be changed at will, and, if it be desired to prevent spectators from getting to the back of the miracle, the top of the box may be covered with finely-ground glass, or with oiled tissue-paper. If, however, instead of placing our mirrors parallel to each other we incline them, we shall obtain a multiplication of images dependent upon the angle of such inclination. For example, if we place two pieces of looking-glass on end on a table touching along one edge, and square to each other, and place a marble between them, and then look down between the glasses, we shall see four marbles—three reflected images and the real marble producing them. If now we close up our mirrors until they contain an angle of 60° only, we shall have five reflected images, so that we shall now see a circle of six marbles, and so on. This is the construction of that extremely well-known toy, the kaleidoscope. The mirrors, in contact at one edge, are placed in a tin or brass tube. One end of this is covered by two discs of glass, the inside one plain, the outer one ground, and between them are broken bits of coloured glass or other transparent material, put in so loosely that they can fall about into all sorts of positions. The other end of the tube is closed, save for a central eye-hole. The resulting effect of most diverse geometrical patterns is too familiar to need any further reference here. Were the mirrors inclined at an angle of 45°, we should have seven reflections (and, of course, eight images), but some of the resulting divisions become very dim from so many reflections. Another amusing piece of apparatus—producing, however, a totally different and startling effect by means of a series of reflections—is often seen at country fairs and similar places, where the showman offers to "let you see through a brick for a penny." Fig. 8 shows the means by which this pseudo-miraculous effect is accomplished.

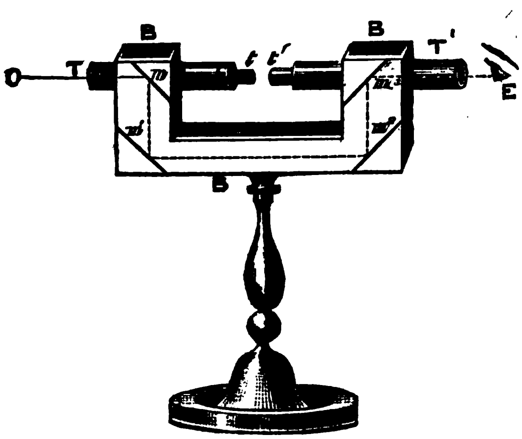


Fig. 8.

It represents the external appearance of the instrument the side next the spectator, however, being supposed to be transparent to show the internal arrangement. We have here a telescope T T' supported by the closed box B, B, B, whose shape will be seen from the figure. The central part of this telescope is separable by the sliding apart for a short distance of the tubes t, t'. When first shown, the ends t, t' are in contact; and in this condition the telescope (which may equally well be a plain tube without lenses) is directed to any object, which will be at once perceived by the eye placed at E. The ends t, t' are now separated, and a brick, or book, or the hand placed between them; notwithstanding which the object towards which the instrument is directed remains as visible as before in every respect The whole secret lies in the fact that at the points m, m^1, m^2, and m^3, are placed four mirrors at angles of 45° (*i.e.*, half-way between vertical and horizontal), with the direction of the rays of light from the object O and the axis of the tube. Hence the light enters the tube, or object-glass of the telescope, at T, and follows the direction T, m, m^1, m^2, m^3, E, shown by dotted lines in our figure, without ever passing through t, t', at all; the spectator being, though, of course, in absolute ignorance that it has not come straight along the line O T, t, t', E.

(*To be continued.*)

THE MORALITY OF HAPPINESS.

By THOMAS FOSTER.

CARE OF OTHERS AS A DUTY.

(*Continued from page 346.*)

WHILE we thus recognise that our well-being depends so greatly on the well-being of others—their health and bodily capacities, their sense and knowledge, and their moral qualities—that due regard for others is essential to the happiness of self, we see further that each member of the body social gains directly by the possession and exercise of such qualities as lead or enable him to help his fellows. Among the proverbs which present in brief the ideas of a race as to what is good and bad, are many which imply that regard for the interest and welfare of others is bad policy. Such proverbs cannot be regarded as expressing "the wisdom of many" by "the wit of one," for experience proves abundantly that the policy of hardness and indifference is unwise and short-sighted. Even mere material success—which does not always mean happiness—is not advanced in the long-run by disregard of others. The man of business gains in unnumbered ways by consideration for the rights and interests of his fellow-workers, and loses in as many by selfish disregard for them. Nay, even in the trivial affairs of ordinary life, at home and abroad, the kindly and considerate gain constantly, while the careless and indifferent as constantly suffer. It is, however, when we consider happiness as distinguished from mere material success, and the general balance of comfort and enjoyment as distinguished from the effects of individual actions, that we see how much men gain by sympathetic and kindly conduct. We see even first-rate abilities and untiring energy beaten easily in the race of life by the kindliness which makes friends of all around and leads to opportunities which the hard and ungenial fail to obtain. But when we rightly apprehend the nature of life, and what makes life worth living, we find the chief gain of the kindly, not in these material opportunities, but in the pleasanter ways along which their life's work leads them. Compare two men, towards the evening of life, of whom both perhaps have achieved a fair amount of material success in life, but one of hard unkindly manners the other genial and sympathetic, one alone in life's struggle the other with "troops of friends" from first to last. Who can doubt as he compares the worn and weary look of one with the bright and cheerful aspect of the other, that regard for others counts for something towards the welfare and the happiness of self?

Care for others helps so surely in life's struggle that it would be good policy for the naturally hard man to benefit others for purely selfish motives, and still better policy to cultivate kindliness and consideration as qualities sure to be fruitful of profit. The kindly nature which leads to spontaneous goodwill towards others, independently of any consideration of gain to self, is even more profitable than

cultivated kindliness. Those are lucky who possess such a nature—lucky rather than deserving of special credit, seeing that a sympathetic nature is born in a man, not made by culture. Yet the will has much to do with the development of kindliness; and many, by sensible reflection and constant watchfulness over the undue promptings of self, have trained themselves to a kindliness and geniality of manner such as they were not naturally gifted with, and this without any direct reference to self-interest, but as a matter of right and justice to their fellows. Such men deserve much credit for their care in correcting inherent tendencies to undue care of self. The increased happiness of their lives (in so far at least as happiness depends on conduct) is their reward.

Among the good effects of kindly regard for others we may note the reflected happiness derived from those around. Men vary with their company, and undoubtedly the man of sympathetic temperament whose presence is a pleasure to others, finds others much pleasanter in their relations with him than they would be were he of hard ungenial nature. The wife and children of the kindly man are a constant pleasure to him, where the wife and children of the sour-tempered ungenial husband and father are apt to grow gloomy and quarrelsome. His friends and relatives are kindlier than those of the harsh and selfish. Abroad, he sees few faces which do not reflect something of his own brightness and cheerfulness. As Mr. Herbert Spencer well says: "Such a one is practically surrounded by a world of better people than one who is less attractive: if we contrast the state of a man possessing all the material means to happiness, but isolated by his absolute egoism, with the state of an altruistic man relatively poor in means but rich in friends, we may see that various gratifications not to be purchased by money come in abundance to the last and are inaccessible to the first."

But in yet other ways do we find illustrated by the effects of due care for others the saying "to him that hath shall be given, and from him that hath not shall be taken even that which he seemeth to have."

Not only has the hard and ungenial man fewer gratifications but those which he has he enjoys less than the man who cares for the wants and wishes of others. The one loses the power of enjoyment through his over anxiety for self-gratification, the other unconsciously pursues—through his kindliness of character—the very course which a wise and thoughtful consideration of the plan best qualified to secure self-gratification would suggest. The one, while caring unduly for himself, is exhausting and satiating his power to care for any form of pleasure, the other while ministering to the enjoyments of others is fostering his own capacity for enjoyment. Here again if one wished to suggest a course of action by which a man who suffered from life-weariness might again know the charm of happiness, one could advise no better course than to minister systematically to the enjoyments of those around. The very tide of life is made fuller thus, even as the tide of thought is made fuller by turning from mere reflection to an interchange of ideas and thoughts with those around. While there is work to be done in the way of increasing others' happiness, no man—not even the most jaded and satiated—need ask himself the sickly question, "Is life worth living?"

Especially is this so when the tide of life is ebbing. Mr. Spencer's words on this point are worthy of careful study, by those in particular who know of him only as the teacher of some hard unsympathetic system of Gradgrindian philosophy, for they afford an apt example of his kindly and lovable teaching:—

"It is in maturity and old age that we especially see how, as egoistic pleasures grow faint, altruistic actions come in to revive them in new forms. The contrast between the child's delight in the novelties daily revealed, and the indifference which comes as the world around grows familiar, until in adult life there remain comparatively few things that are greatly enjoyed, draws from all the reflection that as years go by pleasures pall. And to those who think, it becomes clear that only through sympathy can pleasures be indirectly gained from things that have ceased to yield pleasures directly. In the gratifications derived by parents from the gratifications of their offspring, this is conspicuously shown. Trite as is the remark that men live afresh in their children, it is needful here to set it down as reminding us of the way in which, as the egoistic satisfactions in life fade, altruism renews them while it transfigures them."

But not only does altruism increase the pleasures of life; the exercise of the altruistic qualities is in itself pleasurable. The state of mind when kindly actions are performed affords pleasure. It directly increases happiness, and thus (like other pleasures) enhances physical well-being. It is true that a sympathetic nature suffers where a hard and callous nature would feel no pain. Undue altruism has no doubt its bad effects, nor can it be denied that even such altruistic feelings as are desirable for the social well being, cause, at times, some degrees of suffering; but the exercise of the altruistic qualities is in the main pleasurable, and it cannot be doubted that altruistic emotions give more pleasure than sorrow. When we sorrow for a friend's grief we experience pain and undergo such depression of the vital functions as always accompanies pain; but in the long run the joy felt in sympathy with the joys of others surpasses the sorrow occasioned by their troubles.

Then too it must be remembered that those pleasures which we derive from the arts owe a large part of their value to altruistic emotions. Consider the pleasure given by a painting representing a scene which moves our sympathies, or the delight with which we read some work of fiction in which kindly emotions are dealt with, and it will be seen how large a portion of our æsthetic gratifications depend on our sympathy with others. The hard and selfish care little for art and nothing for fiction. How should we bear to lose the pleasures which painting and sculpture, music and fiction, afford us? How even should we bear to change the pleasures given by the kindly and sympathetic art of to-day for the harsher effects of the arts of harder times when only deeds of conquest or ceremonial observances were represented in paintings and sculptures, suggested in musical strains, or recited in story or in song? What material gains, what sensual gratifications, what power, wealth, or fame, would make up (to us) for the pleasure we derive from the higher emotions? and how largely do these depend on the sympathies by which men are moved to loving care for the wellbeing of their fellows?

(To be continued.)

THE GULF STREAM.

BY WILLIAM HOSEA BALLOU.

[I quote this paper from the *Saturday Evening Herald*, Chicago, partly because of its interest, but chiefly because the writer asserts—correctly enough, I daresay—that a theory urged by me, sixteen or seventeen years ago, had been taught earlier in McNally's System of Geography, used twenty years ago in the district schools of America. Mr. Ballou writes as if I had claimed priority and had attached importance to the point. Now, if I know myself at all, I

never cared two straws about priority in such matters. I get interested in a subject, and try to puzzle out the truth respecting it, both by examining the evidence and by inquiring into the reasoning of others. In dealing with such reasoning I always mention by name those whose views I am considering, and where I present reasoning without mentioning any name, it may be taken for granted that I know of no one who has urged such reasoning. But I have never claimed priority or cared for priority in such matters. Nay, I cannot avoid a feeling of contempt for those who do trouble themselves about priority. We want to get at the truth, not to know who first took a particular line towards the truth. If there is anything much more contemptible than claiming priority at all, it is claiming priority unjustly. It is stupid as well as mean. A fellow is sure to be found out. As for presenting another man's theory for one's own, and using his "almost exact wording," that is sheer idiocy. I suppose Mr. Ballou means, in the present case, that my views are so like Mr. McNally's (of whom I now hear for the first time, unless he be the junior member of the well-known American house Rand & McNally) that he—Mr. Ballou—could readily present my views in almost the exact words used by Mr. McNally. This is likely enough. But he ought not to have so worded his account as to leave the impression that I had dishonestly (and most stupidly) tried to deprive Mr. McNally of whatever credit his prior adoption of this particular theory may entitle him to.

It is singular how apt weak-minded people are to imagine "claims" to priority where a man is simply trying to puzzle out the truth. My first book Saturn, chapter V. contained a good deal of reasoning about the Saturnian rings,—immediately comes down on me a Mr. Carpenter, of Greenwich, with the announcement that the real credit of the ideas discussed is due to Professor Pierce (was it, or Clerk-Maxwell? I forget; but I know full credit is given both, in the book). In my book on the Sun, again, all sorts of theories are suggested and discussed, without a word as to claim or credit for ideas naturally presenting themselves in the discussion of the evidence: but,—does not some silly reviewer in the *Athenæum* find in the mere discussion of the evidence "too eager claim for credit?" I wrote to the editor asking for one word of claim to be indicated; but, instead of honest admission of error, he allowed that mendacious reviewer to shuffle off under the inappropriate plea that not the validity of the claims, but their advisability was in question!—the only question being whether they were made at all.—R. P.]

CIRCULATION is not confined to the blood of man nor to the currency of a government or bank. It is the essential factor visible everywhere in nature. The network of the rivers is the life-blood of land, the winds of the atmosphere, and currents of the ocean. It is death to stand long in the snow, because the circulation of the system becomes blocked; thus circulation of some kind is necessary to the preservation of all the elements of nature, social and physical. Man dies when the circulation of the blood ceases. The land would similarly die, so far as habitation or cultivation is concerned, if the rivers should cease their flow. The air would similarly die and no longer afford refreshing breath should the winds cease and the cyclones fail to purify it. The ocean without its currents would soon die, and its surface be blocked with dead fishes and lower forms of animal life. This earth will remain habitable just as long as these infinite methods of circulation are perpetuated; and when the force we call gravitation fails to circulate the orbs in space, the doom of the universe will be sealed.

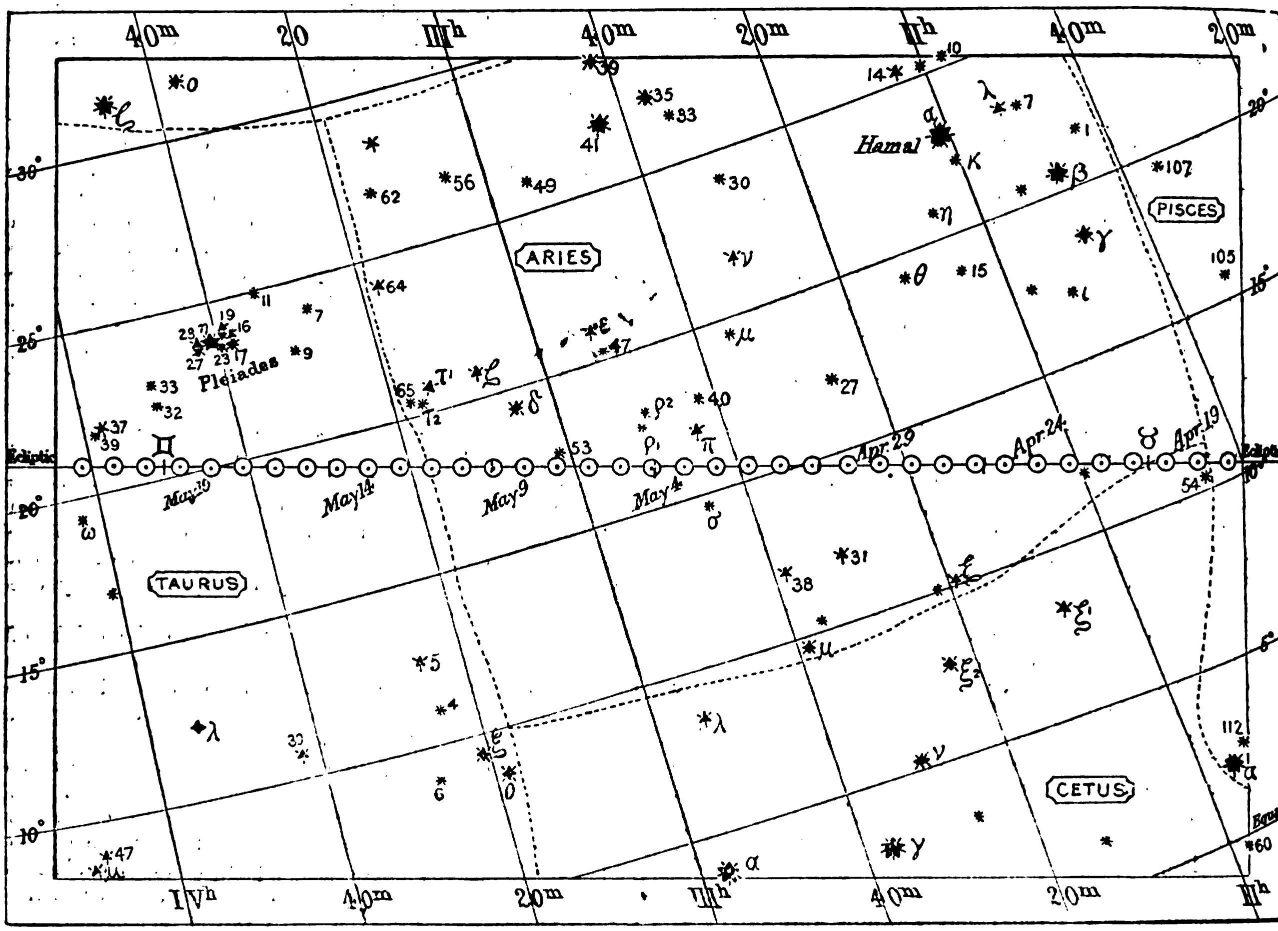
The Gulf Stream is the largest and longest body of flowing water extant. [Qy.] We are to regard it as the steam-pipe which conveys the heat from the equatorial furnace of the earth to points where the sun is not sufficiently operative. The amount of heat thus transferred is easily estimated at nearly eighty quintillions [or *Anglice* trillions.—R.P.] of tons annually. The evidences of geology exhibit this stream in a fickle light. It has not been constant in its devotion to Northern Europe and England. When it sought other idols, the cold currents flowing south occupied the greater part of the Atlantic, and cooled the now moist westerly winds. Then, in Northern France, the Arctic fox, reindeer, and glutton prowled about. After this, there was a gradual change, and the current returned with greater warmth than is now experienced, so that the fig-tree and canary-laurel flourished where Paris now is. Then it was that lions, tigers, and elephants held sway in the valley of the Thames, and London was founded by the denizens of jungles. Some one has been foolish enough to express a fear that an isthmian canal would turn this powerful current into the Pacific Ocean, forgetting that the dimensions of such a canal would *hardly* average fifty miles in width by one thousand feet depth.

There are numerous theories in regard to the origin of this perpetual-motion current. The most ancient supposed the Missisippi river the parent, but it was found that its volume was one thousand times too small for the purpose, although its waters mingle with it. Captain Livingston ascribed it to a sort of yearly tide, conceived by the sun's apparent yearly motion and influence on the Atlantic. Dr. Franklin held that the stream was the reflux of waters piled up in the Gulf by the trade winds; but these gentle breezes only blow about 111 days per year, and could not possibly pile up so much water. Besides, water being eight hundred times heavier than air, it is scarcely presumed that the trade winds develop strength enough for such a task.

Captain Maury next took into account the action of the sun's heat. He believed that the water at the equator was made lighter by the action of the sun, and flowed over the surface toward the poles. The cold water of the Polar seas rushed in to take its place, but, being heavier, formed a submarine current.

Sir John Herschel maintained that such effects were impossible, since, if the waters became lighter, they could only have an upward, downward, or sidewise tendency. The latter could only result from the gradual sloping caused by the bulging of equatorial waters. Such a slope was too slight for such an effect.

Richard A. Proctor, the astonomer, next takes the stand, and argues for a theory most generally held to-day. He proceeds to show that the great heat of the sun at the equator has a drying as well as a warming effect on the waters. It evaporates enormous quantities. This causes an intense suction to take place over the whole equatorial Atlantic, and a submarine current of cold waters from the poles results, and takes the place of the waters evaporated, also causing a surface flow of warm waters towards the poles. He says: "Having once detected the main-spring of the Gulf Stream mechanism, or rather the whole system of oceanic circulation—for the movements detected in the Atlantic have their exact counterpart in the Pacific—we have no difficulty in accounting for all the motions which that mechanism exhibits. We need no longer look upon the Gulf Stream as the rebound of the equatorial current from the shores of North America. Knowing there is an underflow towards the equator, we see there must be a surface-flow towards the poles. This must inevitably result in



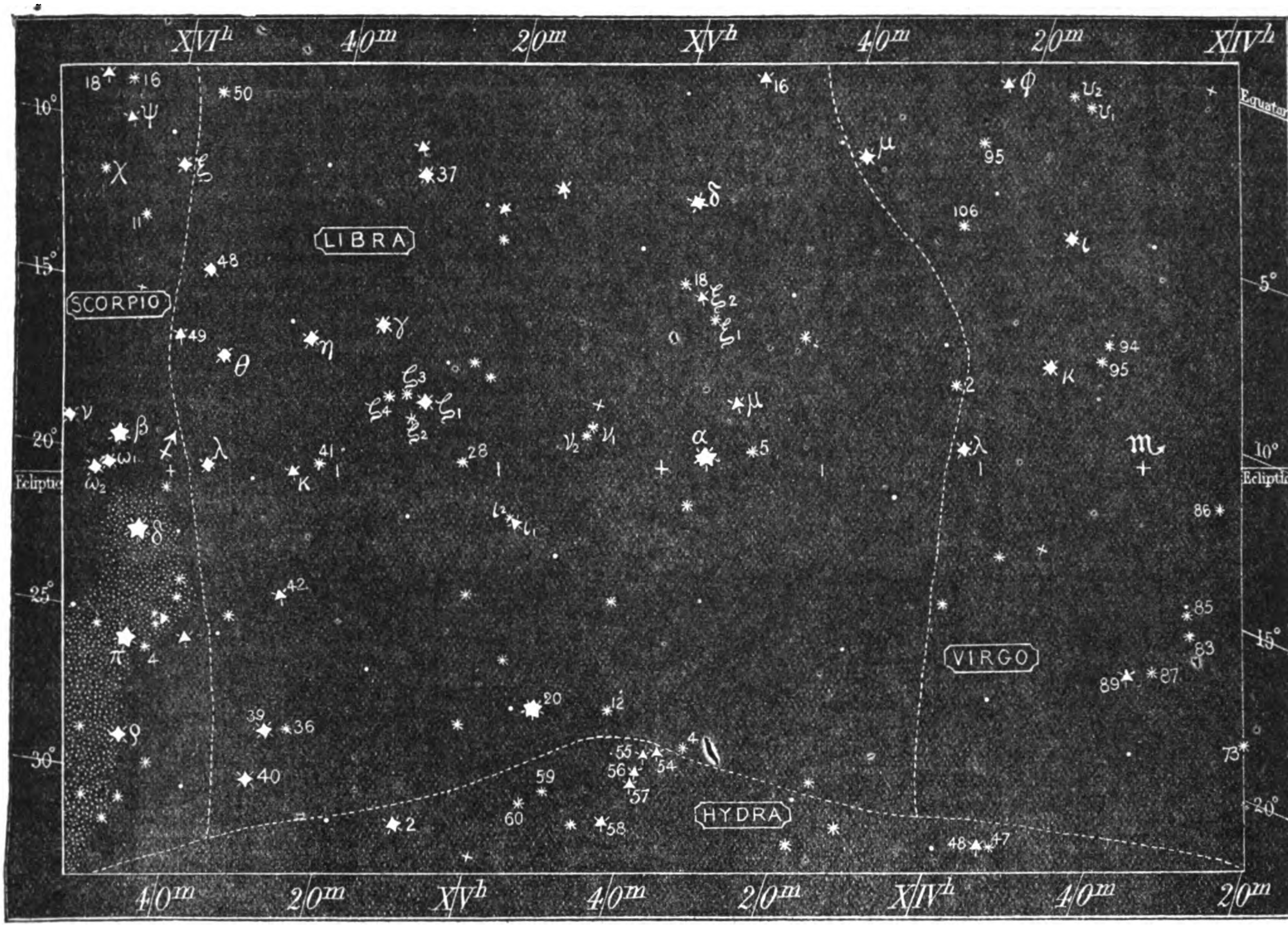
The Day Sign for the Month.

an easterly motion, as the underflow towards the equator results in a westerly motion. We have, indeed, the phenomena of trade and counter-trades exhibited in water currents instead of air currents."

I protest in the name of every student who attended the district school twenty years ago, that this Proctorian theory is almost the exact wording of that of Francis McNally in his "System of Geography," then studied. Yet at that time the Royal Geographical Society was questioning the very existence of the Gulf Stream. In brief, our unpretentious geographer, who made but a three-page analysis of the physical features of the earth, quietly advanced the only correct theory of oceanic currents which were only advanced in after years by the great scientists abroad as a result of a regular process of evolution of ideas, given in the last paragraphs. To my own mind, all the causes taken into consideration, which were either accepted or rejected, contribute their quota to oceanic currents. The vast volume of water constantly contributed by the Mississippi and tributaries must render a portion of the prodigious force and volume of the Gulf Stream possible. The trade winds banking up waters in the Gulf must add something by the reflux. The bulging of equatorial waters may contribute a little. Of course, the evaporation by the sun is more potent than all other forces combined; but Richard A. Proctor has not an iota of claim to priority for that theory.

The equatorial current is not continuous as a submarine flow. The United States Coast and Goedetic Survey has prepared a map, showing an "inner cold wall" from outside New York to Cedar Keys, by which term is meant the equatorial current, flowing from the Arctic Sea. It is not surprising, then, that the warm waters of the Gulf sweep the bed of the ocean for many hundreds and perhaps thousands of square miles. It consequently happens that its bed, as well as the Gulf Stream itself, has a distinct fauna and flora of its own, perhaps the most marvellous in any area of the globe. The dredging and trawl nets of the United States Fish Commission in this area have brought up literally thousands of new species of fishes and the lowest forms of animal life. There seems to be no end to the species discovered here. Every year a new section of ocean bed is explored, and a new series of animal life brought forth. The warm waters of the Gulf Stream, bringing a constant supply of food and soil from the Gulf and the far interior of the west and north-west United States, makes this a rich field for the support of life. Here, too, have been discovered the breeding and hiding places of large schools of new or long-known edible fishes. One acre of land on the ocean-bed touched by the Gulf Stream is worth a hundred acres of the richest prairie land. The products of this area find their way to Chicago, and may be had at the table in a line of eating-houses and dining-cars as far west as Salt Lake City. Thus the soil which is lost to the West by the depradations of the Mississippi is returning its par value with interest to the same West by aiding in the support of food-fishes far out in the Atlantic.

The Metropolitan Railway now issues 72,000,000 tickets yearly, of which 80 per cent. are third class, averaging 1·8d per ticket.



The Night Sign for the Month.

ZODIACAL MAPS.

By RICHARD A. PROCTOR.

WE give this week both the day sign and the night sign for the month, one showing the zodiacal sign now high in the heavens at midnight, the other showing the region of the zodiac athwart which the sun pursues his course at this part of the year.

MOVED FROM AFAR.*

II.—THE WILL.

(*Continued from page* 348.)

WE choose an experimental case which is of interest as showing a "subject" midway between the normal and the mesmerised condition. It is interesting, too, as the first publication, on first-hand authority, of an after-dinner incident which made much sensation in Yorkshire society when it occurred, and which even twenty years afterwards was still alluded to with bated breath as a manifest proof of the alliance of mesmerists with the devil. The modern inquirer will rather regret that this diabolical assistance was so frequently perverted to mere works of charity and mercy; for Mr. H. S. Thompson (formerly of Fairfield, now of Moorlands, near York) has devoted his almost unique mesmeric power mainly to the cure and comfort of his tenantry and poorer neighbours, and has only incidentally made, and rarely recorded, those experiments on "the silent power of the will" which few men, we fear, in a generation are able to repeat.

"Moorlands, York, November, 1883.—Dear Sir,—I will give you a sketch of some of the experiments I have tried, and which lead me to the conclusion that the will is sufficient to influence some people either far or near. In 1837, I first became acquainted with mesmerism through Baron Dupotet. The first experiment I tried was upon a Mrs. Thornton, who was staying with some friends of mine, Mr. and Mrs. Charles Harland, of Sutton. She told me that no one had ever succeeded in mesmerising her, though she soon submitted to being mesmerised by me. She went to sleep at once, and was very strongly influenced by my will. One night when I was dining with Mr. Harland, after the ladies had left the room, some gentleman proposed that I should will her to come back again, which I did. She came directly, and after this I could not go to the house without her going to sleep, even if she did not know that I was there. I have met with many cases of thought-reading, but none so distinct as in a little girl named Crowther. She had had brain fever, which had caused a protrusion of the eyes. Of this ill effect I soon relieved her, and found that she was naturally a thought-reader. I practised on her a good deal, and at length there was no need for me to utter what I wished to say, as she always knew my thoughts. I was showing some experiments to a Dr. Simpson, and he asked me to will her to go and pick a piece of white heather out of a large vase full of flowers there was in the room,

* From a paper on Apparitions, in the *Nineteenth Century*, by Messrs. Edm. Gurney and Fred. W. H. Myers.

and bring it to me. She did this as quickly as if I had spoken to her. All these experiments were performed when the girl was awake, and not in a mesmeric sleep.—Believe me, dear Sir, yours truly, HENRY STAFFORD THOMPSON."

The following cases differ from the last in that the desire became operative at a distance, without any expectation of such a result on the part of the person who exercised it.

Extract from a letter to Professor Sidgwick.—"Cathedral Yard, Winchester: Jan. 31, 1884.—Sir,—As a constant reader of the *Times*, I have noticed the 'Proceedings' of the Psychical Society, and as your society has invited communications, I respectfully beg to offer you a short statement of my experience on a subject which I do not understand. Let me premise that I am not a scholar, as I left school when twelve years of age in 1827, and I therefore hope you will forgive all sins against composition and grammar. I am a working foreman of masons at Winchester Cathedral, and have been for the last nine years a resident of this city. I am a native of Edinburgh. It is now more than thirty years ago that I was living in London, very near where the Great Western Railway now stands, but which was not then built. I was working in the Regent's Park for Messrs. Mowlem, Burt, & Freeman, who at that time had the Government contract for three years for the masons' work of the capital, and who yet carry on a mighty business at Millbank, Westminster. I think it was Gloucester-gate, if I mistake not. At all events it was that gate of Regent's Park to the eastward of the Zoological Gardens, at the north-east corner of the Park. The distance from my home was too great for me to get home to meals, so I carried my food with me, and therefore had no call to leave the work all day. On a certain day, however, I suddenly felt an intense desire to go home, but as I had no business there I tried to suppress it,—but it was not possible to do so. Every minute the desire to go home increased. It was 10 in the morning, and I could not think of anything to call me away from the work at such a time. I got fidgetty and uneasy, and felt as if I must go, even at the risk of being ridiculed by my wife, as I could give no reason why I should leave my work and lose 6d. an hour for nonsense. However, I could not stay, and I set off for home under an impulse which I could not resist. When I reached my own door and knocked, the door was opened by my wife's sister, a married woman, who lived a few streets off. She looked surprised, and said, 'Why, Skirving, how did you know?' 'Know what?' I asked. 'Why, about Mary Ann.' I said, 'I don't know anything about Mary Ann' (my wife). 'Then what brought you home at present?' I said, 'I can hardly tell you. I seemed to want to come home. But what is wrong?' I asked. She told me that my wife had been run over by a cab, and been most seriously injured about an hour ago, and she had called for me ever since, but was now in fits, and had several in succession. I went upstairs, and though very ill she recognised me, and stretched forth her arms and took me round the neck and pulled my head down into her bosom. The fits passed away directly, and my presence seemed to tranquillise her, so that she got into sleep, and did well. Her sister told me that she had uttered the most piteous cries for me to come to her, although there was not the least likelihood of my coming. This short narrative has only one merit; it is strictly true.—ALEXANDER SKIRVING."

Dr. Fischer, whom we quoted, describes how he was himself once driven forth from the midst of a jubilee-dinner, by the urgent desire (as it turned out) of a person whose need of his attendance was at the time quite unknown to him; and we have reason to believe that the experience is by no means unique in medical practice. We received the following very similar case from Mrs. Clow, 11, Upper Hamilton-terrace, N.W.:—

"Dec. 17, 1883.—On Dec. 2, 1877, I was at church. My children wished to remain at a christening. I said, 'I cannot; somebody seems calling me; something is the matter.' I returned home to find nothing; but the next morning two telegrams summoned me to the death-bed of my husband, from whom I had had a cheerful letter on the Saturday, and who left me in excellent spirits the Tuesday before. All Sunday he was dying, and my friends could not telegraph, and there was no train. I only arrived in time to see him die. As soon as I read your letter, my sons both said they remembered the circumstance quite well, and signed the enclosed. George was ten years old, John twelve years.—ELLEN CLOW."

"We remember perfectly our mother leaving the church, saying she felt she was wanted—some one was calling her. The next day our father died, December 3, 1877.—GEORGE CLOW, JOHN A. CLOW."

Here we have instances of an impression powerful enough to produce a distinct and unusual course of action—for Mrs. Clow assures us that, under ordinary circumstances, she would certainly have remained where she was—yet so obscurely seated in the mind that its own source remains unrealised and unknown.

(To be continued.)

ASBESTOS PAINT AT THE INTERNATIONAL HEALTH EXHIBITION.—A fire broke out at one of the exhibitor's stands in the central annexe of this Exhibition early on Saturday week, but was confined to the room in which it originated by the resistance offered by the asbestos patent fireproof paint with which the timber walls were coated fifteen months ago by the United Asbestos Company, of London. The authorities are to be congratulated on having escaped so lightly, also the United Asbestos Company, on this additional proof of the practical value of their paint.—*Engineering.*

THE REFRACTION OF WAVES.—At the Birmingham meeting of the Physical Society, on May 10, Professor J. H. Poynting exhibited an experiment designed to illustrate by means of water waves the refraction of waves when they pass from one medium to another in which the velocity is different. The apparatus consisted of a tank 2 ft. 6 in. square with a plate-glass bottom. Water is poured into the tank to a depth of, say, five or six millimètres. The lid of the tank consisted of a calico screen, and was slightly tilted up. A naked lime-light placed under the tank threw on to the screen a picture of the waves in the water. Plates of glass three or four millimètres thick were placed in the tank, thus reducing the depth of the water. If waves were now sent across the tank they travelled more slowly across the shallower water over the plates and were seen to be refracted. When circular or lenticular plates were employed it was easy to show that the refracted waves converged to a focus.

THIS is the order in which the 1.15 train from Waterloo was made up on Sunday week:—(1) Engine, (2) a very old van, (3) a second-class carriage, (4) a first-class carriage, (5) a third-class carriage, (6) a first-class carriage, (7) a third-class carriage, (8) a first and second composite carriage, (9) a third-class carriage, (10) a composite first and second-class, (11) a first-class, (12) a third-class, (13) a brake-van, (14) a first-class, (15) a composite first and second-class, and finally, three vans for milk, &c. Nicely-arranged train! So easy for passengers to find the sort of carriages they want—first, second or third-class, very old, old, or recent!—*Engineer.*

DR. LECHER recently made an experiment to prove whether Faraday's famous experiment of rotating the plane of polarisation by an electric current could be inverted. He has attempted to generate currents by rotating the plane of polarisation of light. The arrangement was as follows:—A ray of plane-polarised light was sent through the interior of two powerful helices of wire situated at some distance from one another. Through the first of these a powerful alternate current was sent, which impressed upon the ray a rapid oscillation of its plane of polarisation. The second helix was connected to a sensitive receiving telephone in the hope that sounds might therein be heard, as would be the case if the rapid rotations in the plane of polarisation of the ray were capable of setting up currents in the surrounding wire. Absolutely nothing was, however, heard.

Editorial Gossip.

THE foolish lad, Castles, led by Bandit-and-Pirate literature to enter on a buccaneering career, and now in prison for wounding a policeman, is an illustration of reversion to ancestral types. It is a case of atavism. We all pass in our babyhood through the monkey stage, and in our childhood through the savage stage of life, as certainly as before birth we passed through lower stages still. Every boy passes through the warlike stage (of course, the goody-goody boy is only the exception proving the rule—being a monstrosity and utterly abnormal). Day dreams of chivalrous deeds, of the valorous and effective wielding of sword and spear, curtal-axe or mace, of riding on powerful destrier or on more active war-horse, fighting *à outrance* for imprisoned princesses, and so forth, passed through the minds of all of us, I take it, when we were boys. Yon curly-headed lad of yours, who paces along with your walking-stick over his shoulder, or swordwise by his side, his eyes turned inwards (as Charles Reade puts it) is dreaming of such deeds even now. "I would myself have been a soldier," if in after-life I had carried out the ideas of my boyish days,—unless I had been an equally doughty sea-warrior. And though I smile when I see my youngsters so full of warlike thoughts, at that particular stage of life, I should be sorry to see them otherwise. It is inborn in the race, this love of fighting,—a natural part of a boy's life. He is tolerably sure to grow out of it,—unless, of course, he inherits no better traits, the development of which belongs to later years.

BUT sometimes—as in the case of the unfortunate lad Castles, this fighting element in the blood shows itself with altogether exceptional strength. That it should be associated with bandit notions or piratic aspirations, is nothing. Your own boy in the warrior stage is as ready to be a great (but generous) highwayman, or a daring (but on the whole kindly) pirate, as to be a general or an admiral. He has a dim idea indeed, that predatory chieftainship is more easily attained than more formal military or naval distinction. But in his case there is in the background a steadying influence. He knows, though he would not admit as much for a good deal, that after all it is "only pretend." Even Dickens's boy pirates though they pretended very fast, yet stopped short of actual deeds of daring, — and their young brides (of from five to ten last birthday) asked ungrammatically with tears in their pretty little eyes, If grown folks won't pretend too, what use is our going on? But every now and then we have a case in which the boy bandit does go on, and goes on pretty far. He is for the time possessed by the spirit of his remote progenitors. He is as much out of place as a rock pigeon among tumblers, or a striped horse in a stable where all the rest are of the modern breed. We must of course keep him out of mischief; but his case is to be pitied. There is no wickedness about him. He can no more control that inherited trait of ancestors too remote for any use, than he could prevent the undue development of canine teeth or a gorilla-like length of arm.

WE see elsewhere, in ways less remarked, the survival of ancestral tastes now utterly unsuited to the environment. The country squire in headlong pursuit of a fox is an apt illustration. He does not want the fox. He can acquire skill in riding and get a sufficient amount of exercise in many better ways. His hunting dogs have no value outside this useless work. Nothing comes of it all—unless he chances to break a limb or his neck. Yet he goes on at it all his life as if it were some worthy pursuit benefiting not himself alone, but the whole human race!

THE hearty country squire corresponds in a sense with the healthy boy longing for war and plunder glory and blood. He illustrates a stage in the development of a race, even as in the lad the development of the individual man is illustrated. But what shall we say of beings like those who find sport in shooting at wretched pigeons let out of traps? They are rather survivals like the unfortunate little ruffian recently imprisoned. Their fault is rather stupidity than mere cruelty. There was something as pitiable in the defence they made, in the House of Lords, as in the only pleas which can be urged in extenuation of the offence of young Castles,—like him, they were born so. They (obviously) know no better. The process of Development has not yet eliminated their inherited savagery.

I FANCY there are some few cricketers at this moment who rather regret that something has not been done in the direction I indicated a year or two ago. It is apparently a settled conviction in the minds of the cricketing community that a great match must depend on two innings a-side, whether these be long or short, too long perhaps to be completed in the allotted time, or so short that the match lasts but half the time assigned to it, or even less. Why a match should not last out the allotted time, and be determined by the doings of the rival teams in that time, no one can say. One would imagine that men who really loved cricket would be sorry to see a match undecided for want of time, or match after match, as of late, completed so soon as to leave a large part of the time assigned to it wasted. Yet, again, one would suppose all real lovers of the noble game would be unwilling to see a match made unfair by chances outside those inherent in the game itself,—as if *these* were not enough. Yet by the present arrangement we constantly see an eleven defeated through sheer chance, outside the cricket chances. Even in the fine weather we have had lately this has been so. Only a few days ago I saw an eleven sent in to bat, after their opponents had had all the best of the day, with so bad a light that half their batting strength was lost, and five of their best wickets fell ingloriously. In many matches yet to be played this season we shall see the like.

THE united wisdom of Marylebone seems unable either to see how much the game is injured by such defects, or how they might most readily be removed. Just now, however, the pockets of many skilful cricketers are feeling the effects of cricket conservatism. One whole day lost at Lords, another in the match with Surrey, two whole days at Birmingham, all this means a large loss of money besides many opportunities for good cricket wasted.

THEY talk of narrowing the bat, because it looks as if the bat were getting rather the better of the ball. The match at Birmingham suggested quite the reverse. But there is one way of helping the bat against the ball effectively, which was urged as a fatal defect in the plan I suggested. According to that plan, a batsman would not have the same advantage from getting his eye in as by the present system, though when two got well set, *together*, there might be a good deal of leather-hunting. This, which was urged as a defect, was really an advantage. For however pleasant it may be to the batsman, very long scoring is not a good feature in a match. By the suggested plan the test of the opening overs would come oftener and be

severer; but it would affect all alike, and by helping the ball against the bat, would meet a growing want.

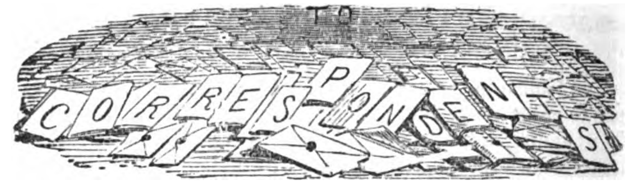
BUT then it is a settled custom for an eleven to take about half an hour in going out to field,—and this custom of course must not be changed! Better nine matches out of ten were unfinished each season.

I HAVE been struck by the wide room there is—even with the best elevens—for improvement in the science of fielding. Men practise bowling and batting systematically, wicket-keeping and long-stopping with fair steadiness. But practice in fielding is haphazard. (You often see practice in stopping straight-thrown balls, but fielding is not merely stopping the ball.) Thus even in important matches you constantly see ill-judged fielding, the line taken being far from the best for quick return of the ball. You see a man run towards the ball, when his only chance of intercepting it is by running towards a point in its course farther from the wickets, and on the other hand you often see a fielder run to such a point and then have to wait for the ball, where by running to a nearer point he could have met it sooner. Onlookers note as bad fielding cases (of frequent enough occurrence they are) where the fielder runs across the ball's course and has to work back to it; but a run is often lost where no such obvious mistake has been made. Practice in fielding by which instinctively the right course is started on for taking the ball neither too near nor too far, but just where it may most quickly be taken, would be good too for judging catches, and would prevent such misjudgment as was repeatedly noticeable in the contest between the Australians and Marylebone. What can be done in this way we may see in the fielding of Mr. A. P. Lucas, who takes as by instinct (begotten really by practice) the precise course for reaching the ball soonest. His marvellous catch at the Oval in 1882 was worth going miles to see, as an illustration of perfect judging; as Bonnor drove the ball, Lucas seemed to start at full speed, his course was not appreciably changed as he ran at his swiftest all the time, and he just reached the ball on the boundary: had he succeeded only in touching it he would have deserved as much applause for his perfect attempt as he obtained when the catch came off.

IN theory, fielding is the most purely scientific part of cricket.

WONDERFUL BICYCLE RIDE.—Mr. J. H. Adams, who started from Land's End on Saturday, the 17th inst., at 5.20 a.m., arrived at John O'Groat's on Saturday, the 24th inst., at 5.5 a.m., thus accomplishing the distance—about 930 miles—in 6 days 23 hours 45 minutes, and beating all previous records, including those of Keith-Falconer, Nixon, and Lennox by from 3 to 7 days. Mr. Adams rode a 46-in. "Facile" Safety Bicycle, and was accompanied throughout, partly by road and partly by rail, by Mr. Tom Moore, of the *Bicycling News*. Mr. Goodwin, of Manchester, who started from Land's End on a 38-in. "Facile" 24 hours in advance of Mr. Adams, also rode the entire distance in 8 days 15 hours, thus beating all records but the above.

OVERHEAD WIRES.—The decision of the Railway Commissioners which was given last week, concerning overhead wires in Wandsworth, is opposed to that of Mr. Justice Stephen, who in the Telephone Company case held that the local authority had absolute power over everything between the street-pavement and the sky. The Railway Commissioners have decided the local board authority is limited by the powers conferred by the old Post Office Acts. The Post Office carry up to five wires overhead, but more than five wires they put underground. The recent decision, therefore, is that when there are few wires it is quite legitimate to carry them overhead, providing that all the wires are of copper, all the standards of iron, and that when a wire crosses a street the distance between the two supports does not exceed one hundred yards.



"Let Knowledge grow from more to more."—ALFRED TENNYSON.

Only a small proportion of Letters received can possibly be inserted. Correspondents must not be offended, therefore, should their letters not appear.

All Editorial communications should be addressed to the EDITOR OF KNOWLEDGE; *all Business communications to the* PUBLISHERS, *at the Office, 74, Great Queen-street, W.C.* IF THIS IS NOT ATTENDED TO DELAYS ARISE FOR WHICH THE EDITOR IS NOT RESPONSIBLE.

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NO COMMUNICATIONS ARE ANSWERED BY POST, EVEN THOUGH STAMPED AND DIRECTED ENVELOPE BE ENCLOSED.

TRICYCLES.—TWO-TRACK MACHINES.

[1269]—A correspondent requests that I will give my views on two-track machines. For fairly level ground, I consider the central-geared Coventry Rotary the best two-track machine made, but it is not a good machine for specially hilly districts, as the wheel is apt to skid in mounting steep hills. The machines of the second type, which have a pair of large double driving-wheels, and the steering-wheel placed on one side, either in front of or behind the larger wheels, obviate this difficulty, but introduce another. The wheel-base of the machine is reduced to a minimum, and it is much more likely to turn over than a machine of similar make with the steering-wheel placed centrally between the two large wheels.

I must take the opportunity of thanking the editor for defending me for a want of courtesy when riding, but I will allude further to the matter in an article which I propose to communicate very shortly. JOHN BROWNING.

STRANGE DREAMS.

[1270]—A few nights since I was unable to sleep, owing to an attack of toothache; after a long time I became unconscious for an unknown period, and awoke, laughing at the following ridiculous dream:—

I thought there were about a dozen persons in a large room, all of whom were adults, except one child about a year old, who sat in a conspicuous position, bolt upright. There was not a particle of hair on the child's head. One of the company prayed aloud in these words:—"O Lord, bless the old men and maidens, the young men and children, and *the bald-headed coot*."

The allusion to the child's absence of hair being so appropriate and a surprise, was too much even for a dreamer—I awoke laughing. Although I had to get up and pace the room in pain, I could not avoid laughing at the concluding words of the prayer.

J. HAMMOND.

[The dream illustrates oddly the way in which old memories, supposed to be dormant, may be aroused in dreams. Mr. Hammond seems even now not able to recall hearing or reading the story. It is, nevertheless, an old one.—R.P.]

RECALLED IMPRESSIONS.

[1271]—Your article for May 9th on coincidences and superstitions calls to my mind certain experiences of my own, as well as suggests a solution of them. On one or two occasions when I have been experiencing some unusual occurrences, the conviction has forced itself upon me that I have experienced exactly the same combination of circumstances before. I could not possibly have done so in the waking state, as it has occurred at places which I have visited for the first time. I have therefore had no alternative but to conclude that a corresponding experience has occurred during a dream. If there are two distinct series of mental impressions, each alternately shading into each other as in waking and sleeping, is it contrary to the law of probability that a particular combination of impressions in the sleeping state should happen to closely correspond to a combination of impressions in the waking state subsequently experienced? According to Herbert Spencer a discharge having passed through a nervous plexus, makes a subsequen

discharge easier. If, then, a waking experience sent a discharge along the same combination of nervous structures as had been sent during a sleeping experience, would not the greater ease with which the discharge passed generate a conviction that the same event had occurred before? Considering the immense variety of impressions we have in dreams, all of which soon pass out of memory, but which must nevertheless have worked their effects on our nervous structures, may not the experience I have mentioned, and perhaps some other mental phenomena, be explained as the occasional coincidence of waking impressions with previous sleeping impressions?

At the close of your article, after speaking of the theory of brain-waves, you add, as if apologetically, "I express no opinion." This would seem to imply that you are anxious not to identify yourself with any opinion on the subject. I have a conviction that a great many men of science avoid investigating mental phenomena from a fear of appearing to give any countenance to the theories of Mesmerists and Spiritualists. Because these theories are mostly wrong, they are assumed to be entirely wrong. This unwillingness to recognise the "soul of truth in things erroneous" has, I believe, serious consequences, causing, as it does, the investigation of the phenomena to be left to uncultured minds—minds which, like the savage's, are liable to assume supernatural agency on the slightest pretext; whereas, if they were investigated by men capable of doing so impartially, valuable truths might be discovered, and perhaps much light thrown on subjects that are at present obscure.

G. E. SUTCLIFFE.

IRELAND.

[1272]—To form even a slight knowledge of Ireland requires more than a holiday visit. It is an advantage to know somewhat of its social and political history and anthropology. Very different pictures of the people are presented to the observer, according to the locality visited. The North is largely peopled with the descendants of the Scotch and English settlers that came over at the plantation of Ulster. The farmers of Down, Antrim, and Derry speak as broad Scotch, have as snug homesteads, and eat as good food as their brethren of North Britain. In the South and East there is a considerable mixture of Danish and Norman blood, particularly the latter, as is shown by the names—Fitzgerald, Fitzhenry, Fitzsimon, De Courcey, &c. With few exceptions, the farmers have comfortable homesteads, flocks of cattle, barns, and outhouses, and live on a wholesome and varied dietary. The West, including Mayo, Galway, and the Isles, is peopled by the descendants of the ancient Firbolgs or Belgæ, who are pure Celts, speak the Irish language, live in small cabins, their cattle almost invariably occupying the same apartment with themselves.

At the battle of South Moyturah, these Firbolgs were conquered by the Luatha D'Danaans, and compelled to occupy these poor and barren wastes.

They built on the Arran Isles those great Cyclopean stone cahirs or forts, which are the oldest stone monuments at present in Europe.

They are to a great extent a potato-eating people; they have preserved their language, their customs, and their dress, which does not change with the fashions, like other parts of Ireland. Tourists usually visit those places where there is fine scenery. If the West is their destination, they procure a circular ticket at Dublin and proceed direct by train to Galway. From thence by steamer to Arran, usually returning same day, as the village of Kiloonan is the only place where very limited and humble accommodation can be procured. Old stagers carry their eatables and drinkables with them, and don't run the risk of having to subsist on potatoes and salt, as Mr. Williams did on the occasion of his visit. According to his own showing, and he has had experience, "It would render one unfit for any kind of vigorous mental or bodily exertion," and this would not be a desirable condition for a tourist.

Returning to Galway, the tourist goes by a two-horse public car through Connemara, or by steamer up Lough Corrib, and thence by Maam through Connemara to Westport and Ballina, finishing up at Sligo. If a longer tour is contemplated, the route by Bundoran, Ballyshannon, to Donegal is the next stage, and from thence through the Wieds, by Carrick, Glenties, and Gweedore, finishing at Letterkenny and Londonderry. Now, it so happens that the counties through which this tour lies—Galway, Mayo, and Donegal—contain the largest percentage in Ireland of poor and barren land, the largest number of small holdings, the largest percentage of people who can neither read or write, and the largest percentage of people who speak Irish. These counties supply seven-eighths of all the labourers who migrate to England to reap the harvest, which is an absolute necessity, as their holdings are too small to support their families.

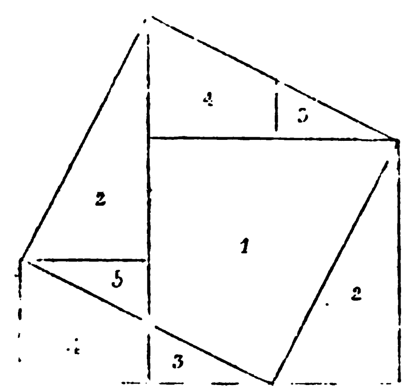
These counties include all the congested districts that have so much exercised the minds of philanthropists and of the Government. They also supply the only specimens of the wild Irish that many English people never see. These are only three counties out of thirty-two, yet, from his experience of a holiday tour in this district, Mr. Williams forms his opinion of the Irish people that they exist almost exclusively on potatoes, and in his last letter gives a most extreme example. His host on that occasion, though designated by Mr. Williams a farmer, would not be so styled in Ireland. He could not supply his guest with a cup of milk, which the humblest farm-labourer in the north could have done.

In the "Traits and Tales of the Irish Peasantry," by Carleton, are pictures from the life of the social condition of the peasantry and small farmers; and they are never represented as living exclusively on potatoes. I have travelled over all the ground I have named in Ireland, and a great deal more; have mixed with the people, and been their guest on many occasions, but I never happened so unfortunately as Mr. Williams. Should he favour us with a visit to see the north of Ireland as well as the west, we have some fine, bold scenery round the Mourne Mountains in County Down, and around the Antrim coast, by Larne, Glenarm, Cushendall, and Ballycastle, and thence to the Causeway. Should he get lost in admiration of the magnificent scenery, and get belated on the road, he is certain to procure Irish hospitality, and get something, as they say in Ireland, to kitchen his potatoes. At the village of Bushmills, where, by-the-bye, a wee drop of very good whisky is made, he can take train and go to Portrush by electricity, having a beautiful view of the headlands and the open Atlantic on the way. I am at one with Mr. Williams in his sympathy for the poor peasants in the west, and believe a brighter day is near at hand, not alone for them, but all Ireland.

SEATON F. MILLIGAN.

SUPERPOSITION.

[1273]—Your proof of 47 in KNOWLEDGE, p. 318, with a slight addition, as in the enclosed tracing, gives a good example of what



Prof. Keland called superposition. Two similar rectangles may be treated in the same way, and composed into a similar rectangle.

B. BRODIE.

DIVISION BY SEVEN.

[1274]—The following method of ascertaining the divisibility of any number by seven was brought under my notice some time ago by a friend, a pupil of Mr. Rickard, of Birmingham, who informed me that it was due to that gentleman.

Point off the given number into periods of three figures, as if for the purpose of reading it. Add together, separately, alternate periods, and find the difference of the sums thus obtained. If this difference is divisible by seven, then the original number is also divisible by seven. An example may serve to exemplify this:—Suppose it is required to know if 220,974,901 is divisible by seven. Following out the above rule, it is divisible by seven if $(220+901)-974$ or $1121-974$ or 147 is so divisible; but 147 is seen to be divisible by seven, hence the given number is divisible by seven.

I should much like to see the general expression for this rule, as I have not been successful in discovering it for myself.

I am led to understand that a similar method may be used for testing the divisibility of any number by 13, but have not seen it.

H. ASKEW.

FIGURE CONJURING.

[1275]—I have amused some of my friends lately with experiments on the figure conjuring according to the process given in your No. 90, vol. 4, p. 45.

In the explanation which you have appended you add "provided

always that N is not greater than 10 nor the age than 100." I submit that the proviso as to N (the month) is not necessary. The process will bring out the 11th or 12th month correctly. A proviso is necessary that if the age exceeds 99 the conjuror must be informed that it exceeds 99, and does not exceed 199, or exceeds 199 and does not exceed 299, and so on. All that he will have to do is to prefix the century to the age, found by the regular process, and deduct it from the numbers representing the month—thus:—For 25th Dec., age 110, by the regular process, the results will be 251310 (a), and he will prefix 1 to the 10, giving 110 as the age, and deduct 1 from 13, giving 12 for the 12th month. Thus:—

```
(a)  25.12
         2
     -----
      5024
         5
     -----
      5029
        50
    ------
    251450
       110 age
    ------
    251560
       365
    ------
    251195
       115
    ------
   25·13·10
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L. W.

PERSPECTIVE OF A CUBE.

[1276]—I can hardly realise the difficulty of your correspondent signing "R. Jones," 1244, as to the perspective of a cube.

He does not give the names of the "many elementary books" which profess to show the sides to a spectator in full front; but they ought certainly to be corrected.

He, however, is in error when he asserts that the moment you see one *side* the front must be out of horizontal!

Let him only place his cube on a convenient shelf and draw on his floor a line parallel thereto.

He has answered the question as to standing exactly opposite; but, let him move a few paces *on his floor line*, either right or left, and he will immediately see one side of the cube foreshortened, whilst the front of the same remains parallel to himself, and must, therefore, be so represented.

The same will happen if *he* keeps his original position and causes *the cube* to be shifted.

In order to get the view he gives in his second diagram, either he or the cube must be twisted, *i.e.*, placed at different angles from each other. ROS. VANSITTART.

Rome, May 21.

JAPANESE SUPERSTITION.

[1277]—I write to give you an instance of a superstition held by the Japanese, which you are at liberty to insert in your paper, if you think fit.

Some few days ago a friend of mine was visited by a batch of Japanese, who came to his house to ask permission to be allowed to inspect a certain large tree in his garden. My friend went out to them and asked why they wanted to see the tree. They said that a relative of theirs was very sick, and had been ailing for some time, and that they believed an enemy of theirs had driven a large nail into that sacred tree (pointing to the fine tree at the end of the garden), and until the nail was found and extracted, their relation would still remain on a sick-bed, and without hope of ever getting well.

My friend laughed, and ridiculed them, trying to persuade them out of such an absurd idea. Understanding (in fact, being a student of their language), he reasoned with the people, but to no purpose—they held firmly to their belief. My chum then waxed angry, thinking they evidently had an eye to his plate, and not the tree, tried to drive them away. But no—they would not go, and begged and implored him to allow them to examine the tree. He then gave way to the poor people, and asked them to come on a certain day, when he would be at home, and they might then search the tree as long as they liked. They then thanked him heartily, and went quietly away to their respective homes.

The day they called to inspect the tree, my friend asked me to come and take tiffin with him; just before tiffin, the Japanese marched into the garden, and made directly for the big tree, and instituted a long search. It was amusing to watch these poor heathens examining and fingering the tree, looking very anxious, and talking most seriously to one another all the time.

However, they went away at last, satisfied and quite happy at finding no nail. F. A. VINCENT.

PUIR DOGGIE.

[1278]—Let "Ida" wet the doggie's feet next time, and then form the electric circuit through the *soles*. Hair is a non-conductor of the magneto-electric current. IBO.

THE CORNCRAKE.

[1279]—I and a friend have been discussing about the habits of the corncrake, or landrail; but we can come to no conclusion which will satisfy both of us. One of us supposes that the bird is a ventriloquist, and thus deceives the anxious person who wishes to find it. The other party assumes that:—1. The bird living almost wholly on the ground, must be more susceptible to vibrations caused by an approaching footstep. This susceptibility will also be heightened by the bird's quicker sense of hearing. 2. It will at once retreat under cover of the grass which, at the time of its arrival, will have begun to grow. 3. It calls to its mate from another position. If disturbed, similarly it will retreat to another position. 4. The sounds heard need not necessarily be made by one bird; there will at least be two somewhat near to each other. I am supposing that the male and female have a similar call-note. May I be allowed to suggest that the corncrake would be a good subject for an article by Mr. Grant Allen? JNO. JOS. JAGGER.

WORKS ON BOTANY.

[1280]—There are many persons very fond of flowers and ferns who have not sufficient time to make botany a scientific study, but who would be glad to have a book which would be of some use to them in country rambles. Such a book I can recommend. "John's Flowers of the Field" (Society for Promoting Christian Knowledge, 4s.) gives a description of all common wild flowers, and notes most species that are rare. The introduction describes the organs of plants, and explains those terms which are necessary beyond the ordinary English words. Even should a person intend to go more deeply into botany this little book would be very useful. It contains a large number of woodcuts—most of them very good—and these are a great help to beginners.

For ferns, a very useful little book is "British Ferns and their Allies" (Routledge, 1s.), and by the time these two have been mastered the student will have learnt what books would be further useful.

Mr. Grant Allen's advice (1129) should be followed by those who intend making botany a thorough study. I would merely add that I prefer Hooker's "Flora" to Bentham's; but the illustrations to Bentham's "Flora," without the letterpress (Reeve & Co., 12s.), are exceedingly useful, and may be well used with Hooker's "Flora." G. HANN.

TRANSMISSION OF SOUND.

[1281]—The following case of transmission of sound comes to me by direct evidence on which I can entirely rely. The phenomenon has been repeatedly observed during the last two years.

The house in which it occurs is one of a row in a London suburban road, rising from south to north, and is on the eastern side. From time to time the sound is heard of the front door opened by a latch-key, and closed in the usual way. This is followed by the tread of a man's footsteps in the passage, pausing to hang up his hat, and then passing into the ground-floor front sitting-room. Everything is perfectly natural, except that no one has come into the house, and the sound is heard indifferently in all the rooms. All that is clear is that the sounds must necessarily be those of some one entering another house: the telephonic conditions are at present unknown. It is needless to add that no inquiries have been made, or any food for local gossip supplied. For this same reason I do not add my own signature.—I am, &c., Q. T. V.

COACHES.

[1282]—It is probable that some more frivolous pursuits have prevented your witnessing the recent displays of the Four-in-Hand and Coaching Clubs in Hyde-park. This is to be regretted, for you might otherwise have enlightened us as to the principle on which these stately equipages are constructed. To an ordinary

observer it would seem that the object has been to combine the minimum of comfort with the maximum of danger and expense. They are so contrived that when loaded the weight is placed as much as possible on the top, and the carriage consequently reels on the slightest provocation. The driver stands, rather than sits, on his box, with, doubtless, a good command of his horses if all goes well, but liable at any shock to be pitched on to the wheelers or the road, which has happened more than once to my knowledge. To climb to the roof is a work of danger and difficulty. It requires no little skill and practice to discover the whereabouts of the contrivances that do double duty for steps and handles; and when this preliminary knowledge is acquired, some amount of activity is requisite to make it available. But, having accomplished the feat, what is the reward? Those who are perched up in front enjoy a near view of the backs of the driver and of the happy occupant of the box-seat, whilst, for those who are relegated to the hinder part of the coach, there is the privilege of contemplating the dignified attitude and folded arms of the groom.

Now, suppose that these imitations of a byegone age were unknown. Suppose it open to competition to devise a carriage to which four horses are to be harnessed and easily driven—a carriage which is to combine the qualities of elegance, safety, convenience for passengers, a certain carrying space, and as much lightness as is compatible with strength. Is it likely that anything like the present form of "drag" would be invented and adopted? I doubt it.

FREDERICK RICARDO.

COINCIDENCES.

[1283]—In the lives of two ladies, both well known, who died not many years ago, there is a curious set of coincidences, which I will illustrate as follows. Let any person be asked to answer the following question:—Who was the lady whose life history is as follows?—

Her maiden name was Marian Evans. She married a gentleman named Lewis. Soon after the death of her first husband she married a gentleman many years younger than herself. She died during the lifetime of her second husband widely known by a name which was neither her maiden-name nor the name of either of her husbands, and her death was a matter of national interest. Who was the lady?

When people are asked this question they invariably answer* [what?].—L. E. B.

LETTERS RECEIVED.

W. G. HOWARD. Thanks.—W. F. Curtis. I have no time to quibble over words. A body may (i.) be larger, (ii.) look larger, (iii.) or seem to look larger,—the first by real change of size, the second by approach or by magnification through a lens or the like; the third by illusion. The sun and moon only seem to look larger when near the horizon; they subtend no larger an angle. It is an optical illusion. The atmosphere has no magnifying power such as you imagine.—THOS. HOWELL. Have nothing to do with advertisements. Please apply to the publishers.—E. A. TINDALL. Thanks. It is rather the geometrical position of the planets than their position in the heavens which is in question. Doubtless Whitaker's Almanac shows well enough where each planet is on the sky. But many are interested to know where each planet lies in space. The trouble about such problems is that about a hundred solutions are sent in, and each solver expects his to be not only mentioned, but examined. For this neither our time nor space suffices. Mr. Prince has published, but we believe for private circulation only, much of the work of Hevelius.—A. R. DAMES. Not at all improper; but we are not able to give time to such matters.—A. E. BEAN. The constant of aberration represents the angle whose sine is the distance traversed by the earth in a second, while the radius is the distance traversed by light in a second. Hence its determination gives the velocity of light; and knowing the velocity of light we can tell how far away the sun must be for elipses of Jupiter's satellites to seem hastened or delayed so much as they are observed to be, according as the earth is on the side of her orbit towards or away from Jupiter. We could not well give the solution of the problem in detail.—A. J. HARVEY, F. J. DOWNEY. Quite impossible. After about two hours' talk with you I might form an idea what might perhaps suit you as a course of universal history. And very likely what I might suggest would not suit you.—RICHARD E. Have not been in town long enough to see that picture. Thanks.—W. CLARK. Am away from my books; but think Scoresby found in the Atlantic no wave higher than 40 ft. from trough to crest.—ROS. VANSITTART, HESPERUS. It is impossible to decide so suddenly whether a reflector or a refractor is the better sort of telescope, or what book of astronomy a beginner should start with.—J. H. E. H. Elliptical orbits is too precise an expression; up and down does better; it does not mean in straight lines.—M. T. (i.) It was a vain fancy; an empty notion; not based on science, and *one* which theologians should have scouted. *What* an interpretation! The account says they saw his star, and some would vainly suggest that they saw three planets too near together to be separately discerned. (ii.) The south magnetic pole has never been reached. It is not opposite the north magnetic pole: no doubt like that pole it changes in position. —A FACTORY HAND. My dear sir, I don't care a brass farthing to convince you or any others among the "inteligent" artisan class that the earth is not flat. I doubt whether a man exists, artisan or otherwise, of decent "intelegientz" (how's that, umpire?) who really imagines the earth is flat. Parallax knows better, a thousand times. I have done enough in bringing out the facts that (i.) every pretended proof of earth flatness is a falsehood, and (ii.) that the pretended system of science called the Zetetic has been advanced for no other purpose on the face of the earth (round flat or polyhedral), but to help sell quack medicines. Do you suppose I am likely to care to convince folk who are (on the face of things) either utter rogues or utter fools? One set *won't*, the other can't see. I can neither make one see nor help the others. And I do not want to. KNOWLEDGE is for folks having an average brain power above the Earlswood matriculation standard.—JOHN HAMPDEN. The same to you.—SAM RIDEAL. Thanks: but no space for the earthquake essay. I wonder if the "seismic energy" really has frightened folk in England. For my own part my idea of my countrymen in this matter is that

Si fractus illabatur orbis
Impavidum ferient ruinæ,

which being freely translated means that if St. Paul's and the Monument were falling about their heads they would placidly order dinner among the ruins.—A. N. SOMERSCALES. Stay awhile, and I will form an opinion about the resistance to bicycles.—MOLECULE. Dear Brother Molecule, wait till your dream (which gave me as cold a shiver as you could have wished) has been fulfilled. (The moan was probably a misinterpreted furniture-squeak.) At present my only idea is to ask, What had you taken for supper? I am, in any case, not a Daniel, to interpret the dreams of others.—DIOGENES. Really we cannot give the treatment of such problems; we are not private tutor to each of our multitudinous readers. If we were we should get a tidy income at average rates for such work.—H. J. MADGE. I fancy the history of ninety-nine out of a hundred collisions is scarcely consistent with the views you express. —J. BELL. I should like to have the details of your wonderful (window)-paneful experience more precisely. You say the panes flew out; but how? Did they fly out and fall in pieces on the road? or fly out and stop out, in the air? or how? The coincidence is of a kind which would be full of interest if confirmed by names, dates, confirmatory statements as to notes made at the time and so forth.—F. DOUGLAS. Ah! my dear young friend—to adopt your own kind manner—for I presume you are young, nay that you are very young,—and a trifle foolish (but that is a detail), oh! consider your ways in time. Ah! remember that time is a blessing, and reason is a God-given quality, not to be frivolously wasted in idle dreams. Oh! remember that you have time for work and for good work; if you are only willing. Ah! consider that if you waste time now you are young, the end of your life may come and find very little done of the good you might have done. And oh! remember that every man may find enough in his own field of work, and mayhap too much, and that looking over a neighbour's hedge to inquire too curiously what work he may be doing, is but an idle way of passing the time. Oh! my dear young friend reflect; and ah! my poor foolish friend remember; and oh! would you but mind fervently your own business, ah! how very much better might it be for you, yea, and for me, and mayhap for many more. Wherewith I bid you fervently farewell, and pass on my way rejoicing.—B.M., F.R.C.S., thanks for interesting letter, which shall appear soon.—A. F. RAVENSBEAR. Your thoughtful letter shall appear; albeit, I doubt if the murderously minded are so numerous or so clever that we need consider *them* so much as you suggest.

* They generally answer in one way. Our correspondent gives what is in reality the truer answer: but we leave the question for a fortnight to see what the answer is likely to be.—R. P.

*** Owing to an unusual pressure of letters and questions we must leave many correspondents unanswered this week. We have not even been able to initial all the letters received,—not a very useful work in any case. But all correspondents may rest assured that due notice has been or will be taken of their communications.

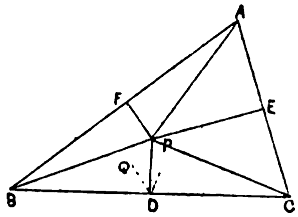
Our Mathematical Column.

NOTES ON EUCLID'S FIRST BOOK.

By Richard A. Proctor.

(*Continued from p. 360.*)

Prop. XXVI.—*Let the sides,* A B, A C, *of the triangle* A B C, *be bisected in the points* F *and* E, *and let* F P *and* E P, *at right angles to* A B, A C, *meet in* P: *then the line drawn from* P *at right angles to* B C *shall bisect* B C *in* D; *and the line drawn from* P *to bisect* B C *is at right angles to* B C.

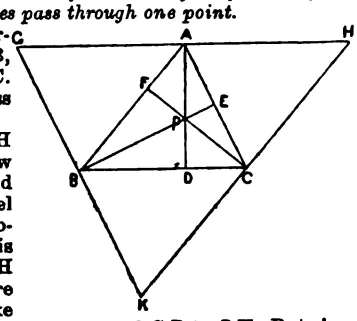


For, because A F is equal to F B, and F P is common to the two triangles A F P, B F P and at right angles to A B, these triangles are equal in all respects. Therefore B P is equal to A P. In like manner A P is equal to P C. Therefore B P is equal to P C. Hence, in the isosceles triangle B P C, P D at right angles to B C, bisects B C in D, and *vice versâ.*

Prop. XXVII.—*The three lines bisecting the sides of a triangle at right angles pass through one point.*

If F P, E P two of these bisectors (same fig.) meet in P, the third bisector, through D shall pass through P. For if it has any other position as D Q, the angle B D Q is a right angle. But, by Prop. XXVI., P D is at right angles to B C. Therefore the angle P D B is equal to the angle Q D B, which is absurd. Therefore the perpendicular from D passes through the point P.

Prop. XXVIII.—*The lines drawn from the angles of a triangle at right angles to the opposite sides pass through one point.*



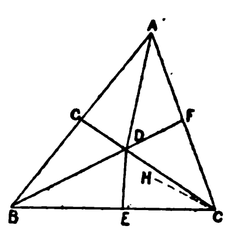
Let A D, B E, C F, be perpendiculars on B C, C A, A B, the sides of the triangle A B C. Then shall A D, B E, C F pass through one point.

Through A draw G H parallel to B C, through B draw G B K parallel to A C, and through C draw K C H parallel to A B. Then G C is a parallelogram, and therefore G A is equal to B C. Similarly A H is equal to B C. Therefore G A is equal to A H. In like manner K C is shown to be equal to C H, and G B to B K. But since the angle G A D is equal to the alternate angle A D C, D A is at right angles to G H; similarly BE is at right angles to G K; and C F is at right angles to K H. Hence, by Prop. XXVII., A D, B E, and C F pass through one point P.

Cor.—If through the angles of a triangle lines are drawn parallel to the opposite sides, the sides of the triangle thus formed are bisected at the angles of the first triangle, and form a triangle four times as great as the first triangle (Prop. XX).

On account of the importance of Proposition XXVIII. we shall give other proofs of it presently.

Prop. XXIX.—*In the triangle* A B C *let the lines* A D, B D, *bisect the angles* B A C, A B C *respectively; then shall* C D *bisect the angle* A C B.

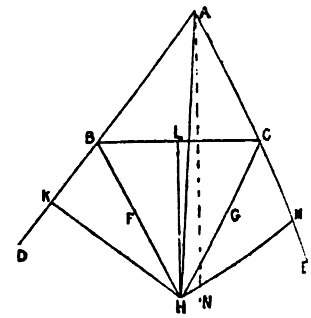


Draw D E, D F, and D G perpendicular to B C, C A, and A B respectively. Then the triangles A G D, D E F are equal in all respects (Euc. I., 28); therefore D G is equal to D F. Similarly D E is equal to D G. Hence D E is equal to D F: and in the triangles D C E, D C F, D E is equal to D F; the angles D E C, D F C, opposite to the common side, D C, are equal (being right angles); and the angles D C E, D C F, opposite to the equal sides D E and D F are both acute (Euc. I., 17), since the angles at F and E are right angles. Hence the triangles D C E, D C F are equal in all respects; therefore the angle D C B is equal to the angle D C A.

Prop. XXX.—*The three lines bisecting the three angles of a triangle pass through one point.*

If A D, B D, two of the bisectors, meet in D, the third must pass through D; for if it had any other position, as C H, then the angle E C H would be equal to half the angle B C A. But, by the preceding proposition, D C E is equal to half the angle B C A. Therefore the angle D C E is equal to the angle H C E, which is absurd. Therefore the three bisectors all pass through one point.

Prop. XXXI.—*If two sides* A B, A C *of the triangle* A B C *be produced to* D *and* E, *and the angles* D B C, E C B *be bisected by the lines* B F, C G, *these bisectors will meet, and the line joining the point in which they meet with* A *will bisect the angle* B A C.



For the angle D B C is less than two right angles, and therefore the angle F B C is less than one right angle. Similarly the angle G C B is less than a right angle. Therefore the two angles F B C, G C B are together less than two right angles, and B F, C G will meet if produced far enough. Let them meet in H, and draw H K, H L, H M perpendiculars on A D, B C, and A E. Then, the triangles H B K, H B L, are equal in all respects (Euc. I., 4); therefore H K is equal to H L. Similarly it may be shown that H M is equal to H L. Hence H K is equal to H M. Therefore in the triangles H K A, H M A, K H is equal to H M, the angles A K H, A M H opposite to the common side H A are equal (being right angles) and the angles K A H, M A H, opposite the equal sides K H, M H, are both acute—Euc. I., 17 (since the angles at K and M are right angles); hence the triangles K A H, M A H are equal in all respects; and therefore the angle K A H is equal to the angle M A H.

Cor.—If two exterior angles of a triangle are bisected the intersection of the bisecting lines is equidistant from the three sides of the triangle.

Prop. XXXII.—*If two sides of the triangle* A B C (*same figure*) *be produced to* D *and* E, *the lines bisecting the three angles* D B C, B A C, *and* E C B, *will all pass through one point.*

Let the bisectors of the angles D B C and B C E meet in H; then the bisector of the angle B A C must pass through H. For if it had any other position as A N, the angle N A M would be equal to half the angle B A C. But, by the preceding proposition, the angle H A M is equal to half the angle B A C. Hence the angle H A M is equal to the angle N A M, which is absurd.

Cor.—The bisectors of the angles B A C, B C E intersect, and the line joining their intersection with B bisects the angle D B C.

(To be continued.)

EASY RIDERS ON EUCLID'S FIRST BOOK,

With Suggestions.

Prop. 30.

88. The parallel lines A B, C D, and E F are intersected by the line B D F G; show that the bisectors of the angles A B G, C D G, and E F G are parallel to each other.

89. With the same construction as in Ex. 81 show that the lines bisecting the angles A, C, and A E C, are parallel to each other.

Prop. 31.

90. From a given point without a given line draw a line which shall make a given angle with the given line.

91. Draw a line D E parallel to the base B C of the triangle A B C, so that D E shall be equal to B D.

92. A B C is an isosceles triangle. Determine points D, E in A B, A C respectively (these being the equal sides of the triangle) such that the lines B D, D E, and E C may be equal to each other.

93. Draw a line D E parallel to the base B C of a triangle A B C, so that D E may exceed C E by a given length.

94. A B C is an isosceles triangle. Determine points D and E in A B and A C produced, so that D B, B C, and C E may be equal to each other.

B E *must be the bisector of the angle* D E A *and therefore must be parallel to the bisector of the angle* A C B.

95. Draw a line D E parallel to the base B C of the triangle A B C, so that B D and C E together shall be equal to the line D E.

Suppose the line drawn, and take a point P *in it such that* D P *is equal to* B D, *and therefore* E P *to* C E. *Notice that the triangles* B D P *and* C E P *are isosceles, &c.*

96. Draw a line D E parallel to the base B C of the triangle A B C, so that D E shall be equal to the difference of B D and C E.

Suppose D E *drawn as required, and produce* D E *to* P *making* D P *equal to* D P (*supposed greater than* C E.) *Then* E P *is equal to* B C, *and the triangles* B D P *and* C E P *are isosceles, &c.*

PROP. 32.

97. If one angle of a triangle is equal to the sum of the other two the triangle is right angled.

98. If one angle of a triangle be greater than the sum of the other two the triangle is obtuse angled.

99. In an acute-angled triangle the sum of any two angles is greater than the third angle.

100. If the base of an isosceles triangle be produced, the exterior angle exceeds a right angle by half the vertical angle.

101. If the base of a triangle be produced either way, the sum of the two exterior angles thus formed exceeds two right angles by the vertical angle of the triangle.

102. If the three sides of a triangle be produced either way, as in the preceding example, the sum of the six exterior angles thus formed is equal to eight right angles.

103. If F G be joined in the figure to Euc. I., 6, B C and F G are parallel.

104. In the figure to Euc. I., 8, the difference between the angles D and G is equal to the difference between the angles D E G and D F G.

105. In the figure to Euc. I., 21, the angle B D C exceeds the angle B A C by the sum of the angles A B D and A C D.

106. In the triangle A B C, B D and C D are drawn bisecting the angles A B C, A C B. Show that the angles B A C, D B C, and D C B, are together equal to the angle B D C.

107. With the same construction show that the angle B D C exceeds a right angle by half the angle B A C.

108. Determine the magnitude of the angles of a regular pentagon.

(To be continued.)

Our Whist Column.

WHIST-TORTURE.*

THE absence of intelligence we cannot help. Inattention is unpardonable when another's welfare is at stake. If we could establish a Whist Bee, we doubt (if the players were examined without notice) whether in the middle of a hand one-half of the players could tell the trump card. We saw a player lose a rubber a short time ago. He had a Heart (all the trumps were out), and his partner had all the rest of the Hearts palpably in his hand. His partner said, "We had every trick. You knew I had all the Hearts, and yet you would not lead one." This is scarcely a fair way of putting it. We often hear men say, "You knew I had so and so," but the fact is (as in this case) the player knew nothing about it. He had no more notion of the position of the Hearts than of any other card. He has not the faculty to infer. A player may reason incorrectly, but some do not reason at all. Thus *A* leads Ace in a plain suit, and follows with another round. Second hand wins with the King, and returns the suit. The now second player (*A*'s partner) has none of the suit, and with a strong trump hand he puts on the trump 2. With a strong hand he trumps a doubtful card, doubtful we mean from his point of view. He did not stop to realise what *A* had led from. He does not attempt to place himself in his partner's position. He might have reflected and said, "What does the lead of Ace and another mean?" My partner is of one school or the other. He may have led Ace and another, desiring to trump. Bad play, we think; but there are players who thus play, and we must take men as we find them. If he be of the ruffing school, surely it would be good policy to let him trump instead of my so doing, and in the meantime by my discard I can tell him my suit. But he may further reason, "An Ace and another originally led should mean my partner has five of the suit. If so, there is no reason why he should not have the Queen; why, therefore, with my strong hand, should I trump a doubtful card?" Some such reasoning as this should pass through his mind. It is but the work of a second, and the result either way should be to induce the player to avoid doing what he did do, viz., trump and get over-trumped. The first Whist lesson, we have said, should be to keep your eye on the table, not to let the attention be drawn to other parts of the room, and not to let the eye rest on your own cards. The last defect is the reason for many of the stupid things we see committed at Whist by men of intelligence. The second lesson is to attempt to reason. A bad reason is better than none at all; with practice, the reasoning faculty should become sharpened, and that which is at first an effort will become mechanical. Let us not be misunderstood. The reasoning process must take place in the player's mind. It must not break out into fervid declamation or testy argument; and the third lesson to be drawn from the above example is the value of a discard.

The best players have not yet fully realised this most difficult subject. The number of games won and lost by a discard is incalculable. In the case before us we have the discard in its most simple form, it at once directs *A*'s attention to *B*'s suit, and the knowledge of that suit governs the play during the remainder of the hand. When a player once realises the value to him of a discard, he may be induced to think of the value of a discard to an opponent; thus, when a player passes a winning card he knows we suppose that he is giving away a trick, but does he reflect that he may be doing much more than that? The discard that the third player may make will tell his partner what to do, and in like manner again direct the remainder of the game. The players who are so stingy that they will not sacrifice one of their trumps on a winning card seldom become good Whist players. It is much better to play a small trump with the certainty that it will be over-trumped, than to let the trick go. The same stinginess often loses a game when the second player will not cover the second best of a suit because he knows the third player will trump. The third player does not trump, partly because he suspects the second player is holding up, or because he thinks his partner desires to clear the suit, or lastly, because the player can get a good discard. The odd thing about this is that the stingy player, who will not sacrifice one of his trumps or good cards, is the player of all others that does not hesitate to sacrifice his partner's cards. He who is a screw with his own is the most venturesome with his partner's. This style of player, with weakness in trumps, is sure to lead trumps. Will they never realize that the cards of their partner are for the purpose of the game, their own cards; that they play with 26 cards on their side, and not 13 only. There is a little point in play that weak players do not seem to understand. A player leads originally for a ruff, and he finds (happy man) his partner with the command of the suit, and the leader gets two discards. The case we saw was this:—*A* led a single Spade, his partner had Ace, King, Queen, and *A* got rid of the ten and nine of diamonds. By the fall of the cards the last player cannot have another spade, so that *B* knew that if his partner trumped he would be over-trumped. To continue the spade was bad play, *B* had the Diamond Ace, and he could have led that and followed with another; when *A* would have had a better chance for the ruff; or he could have led the plain suit which is presumedly his partner's; or he could have led trumps, seeing that *A* could only have two suits and one Diamond, Queen or Knave, or two Diamonds, Queen Knave or King Knave. It would seem improbable that a player would lead a singleton with a suit of King, Knave, ten, nine, or Queen, Knave, ten, nine, and of course in such matters we must be guided to some extent by our knowledge of the players; because there are old-fashioned and new-fashioned players. The one would lead the singleton to make a trump, and the other would only venture on a singleton when he has great strength in trumps; but neither school ever lead the singleton with such a four suit as King, Knave, ten, nine, or Queen, Knave, ten, nine. We should therefore expect to find that *A*'s original hand consisted of single Spade, three Diamonds to the Knave, four trumps all small, and five of the other suit, so small that the player calculated that by no possibility could he establish the suit. Then having got three tricks and Ace of Diamonds for the fourth, we should have been inclined to go for the fifth on the Diamonds. Our partner has asked to be forced, and we can force him with greater advantage in Diamonds than we can in Spades. We do not say this is good play, but by analogy we are justified in the force, and the force in the one suit is clearly better than in the other.

It is useless to tell any player that, except in critical positions, it is seldom any use to pass a King in a plain suit having the Ace. If men will not learn this by pocket experience, they will not learn it from our preaching or from reason. It seems equally useless to protest against one partner taking another's trick without weighty reason, and without an accurate knowledge of the position of the cards. As to forcing without justifiable strength, this is too sore a point to be touched upon. The player of six years old should occasionally look to the score, but we are satisfied that players of sixty years and upwards often fail to do so, and as to clearing the partner's suit, and getting out of his way, one would fancy the players had been brought up on the top of a 'bus, where the chief business and pleasure in life seems to consist in blocking the way.

If we add that those players who pretend to understand the value of the discard generally reverse their theory in practice; they are afraid to lead a winning card, because some one will get a discard; we think we have suggested and recalled as many subjects of torture as were ever invented in the Middle Ages.

* The above capital paper we extract from "Jottings," by Mr. Mossop, editor of the *Westminster Papers.*

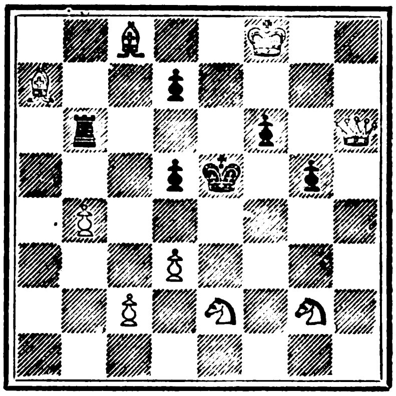
Our Chess Column.

By Mephisto.

PROBLEM No. 119.

By H. W. Sherrard

Black.

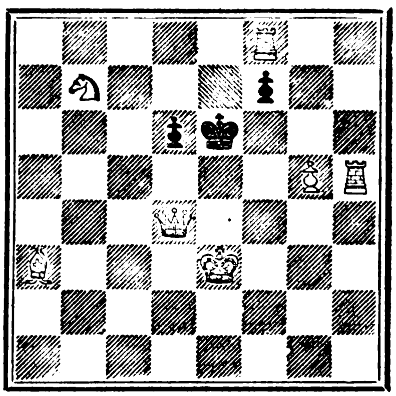


White.

SELECTED PROBLEM.

By S. Lloyd.

Black.



White.

White to play and mate in two moves.

One of twelve blindfold games played simultaneously by Dr. Zukertort on the 25th ultimo.

EVANS' GAMBIT.

White. Dr. Zukertort.	Black. Mr. O. M. Tennison	White. Dr. Zukertort.	Black. Mr. O. M. Tennison.
1. P to K4	P to K4	10. Kt takes P	Kt to Q sq (a)
2. KKt to B3	QKt to B3	11. B to R3	P to QB3
3. B to B4	B to B4	12. QR to Q sq(b)	P to QKt4
4. P to QKt4	B takes KtP	13. B to Q3	Q to K3
5. P to B3	B to R4	14. Kt to K4	B to QB2 ? (c)
6. P to Q4	P takes P	15. Q to B2	P to KR3
7. Castles	P takes P	16. KR to K sq(d)	Kt to QKt2
8. Q to Kt3	Q to B3	17. Kt to Q6 (ch)	K to Q sq
9. P to K5	Q to Kt3	18. B to KB5 ! (e)	

And Black resigns.

NOTES.

(a) Very inferior, and leading at once to inextricable difficulties. 10. * * KKt to K2 is proper.

(b) Clearly enough foreseeing that Black's plans will only result in playing his adversary's game.

(c) This only makes matters worse. 14. * * Q takes Q might have somewhat delayed the attack, but even then Black remains with an execrable, if not a losing, position.

(d) Played with wonderful accuracy, considering that Dr. Zukertort was simultaneously carrying the complication of eleven other games in his head.

(e) Applying the *misericordia*.

Played April 30, 1884, at New Orleans.

TWO KNIGHTS DEFENCE.

White. Mr. Jas. M'Connell.	Black. Dr. Zukertort.	White. Mr. Jas. M'Connell.	Black. Dr. Zukertort.
1. P to K4	P to K4	15. P takes Kt	Q to Kt3 (ch)
2. Kt to KB3	Kt to QB3	16. K to R sq	Q to R4
3. B to B4	Kt to B3	17. R to K3	K to Kt sq
4. Kt to B3(a)	B to Kt5 (b)*	18. R to KKt sq	B to R6 (f)
5. P to QR3	B takes Kt	19. Q to Q4	Q to B2
6. QP takes B	Kt takes P (c)	20. B to R6 !	P to KKt3
7. B takes P(ch)	K takes B	21. Q to R4 ! (g)	B to K3
8. Q to Q5 (ch)	K to K sq	22. B takes R	R takes B
9. Q takes KKt	P to Q4	23. R from Kt sq to K sq	R to K sq
10. Q to QR4	Q to Q3	24. K to Kt sq !	B to Q2 (h)
11. Castles	R to B sq	25. R takes R(ch)	B takes R
12. R to Ksq(d)	K to B2	26. R to K7	
13. KttakesP(ch)	Kt takes Kt		
14. B to B4 (e)	Kt to B6 (ch)		

And Black resigns.

NOTES.

(a) This resolves the opening into a variation of the Giuoco Piano. 4. Kt to Kt5 was best.

(b) The ordinary continuation here would be: 4. * * B to B4; 5. P to Q3, P to Q3; 6. Kt to K2, or 6. Castles, with an even game.

(c) Taking greater risks, we are inclined to believe, than the benefits derived warrant.

(d) Mr. McConnell considers this the key move to White's successful conduct of the game from this point.

(e) 14. Q to KB4 (ch) is apparently better, *e.g.*: 14. Q to KB4 (ch), K to K3 (if K to Kt, 15. Q takes Kt, remaining with a rather better game, and a Pawn ahead); 15. Q to KKt4 (ch) when if Black play R to B4; 16. P to KB4, &c.

(f) Evidently with an eye to QR to K, but there is no time under White's vigorous attack.

(g) All of this is very well managed.

(h) Overlooking the annihilating resource at his antagonist's command.

ANSWERS TO CORRESPONDENTS.

** Please address Chess Editor.*

Correct solution received of Selected Problem from Stettin, George Gouge, Caissa, E. Ridgway, A. W. Overton.

Inquirer.—The King is in check.

Clement Fawcett.—If Q takes Kt, then B takes B, and there is no mate.

S. Balliozian (Smyrna).—Problem 118. If R to K8 (ch), K to Q6. B to R6 (ch), the R interposes. No. 117 has two solutions. See p. 361.

Clement Fawcett *v.* F. Watson.

Contents of No. 134.

* We prefer Kt takes P.

KNOWLEDGE
AN ILLUSTRATED
MAGAZINE OF SCIENCE
PLAINLY WORDED-EXACTLY DESCRIBED

LONDON: FRIDAY, JUNE 6, 1884.

CONTENTS OF No. 136.

THE MOVEMENTS OF A GIANT SUN.

BY RICHARD A. PROCTOR.

IT may be remembered perhaps that when as yet (in the northern hemisphere at least) the great comet of 1882 had only been seen in the full glory of sunlight, and nothing of its real course was known, M. Thollon at Nice from a single spectroscopic observation was able to announce that the comet was receding from us. Nothing more promising perhaps was ever done by the spectroscopic method of determining movements of recession and approach. It had but recently been discovered that some comets show the lines of sodium as well as the broad carbon bands (useless for the method in question) and these lines in the spectrum of the great comet of 1882 had been visible but a short time, and were studied by Thollon but a few minutes. Yet in those few minutes he had been able to detect what seems at the first view hopelessly out of the range of spectroscopic work, the fact that the comet's distance was increasing.

It seems likely that the scientific world will soon hear a good deal of this promising but most difficult line of research. The great star which was the first to respond to the spectroscopic method of measuring motion is beginning to give information of a new and most unexpected character, associated with other strange information which it had given in response to inquiries of another sort.

The giant star Sirius belongs to a class of suns differing in very marked manner from our own. He is so much larger judged by the quantity of light which he emits, that for that reason alone we should place him in another class, just as we separate the giant planets from the earth and Mars, Venus, and Mercury on the one hand, and from orbs like our sun on the other. Although the average of the best measurements (or rather attempts to measure) of his distance sets him at least fifteen years' light journey from us, he shines much more brightly than any star in the heavens, and, according to the photometric observations of Zöllner, about 200 times as brightly as our sun would shine if set beside him. This, assuming the surfaces to be of equal intrinsic lustre would imply a surface 200 times as great, a diameter about 14 times as great and a volume nearly 3,000 times as great as our sun's. But the spectrum of Sirius gives reason for believing that his intrinsic lustre is considerably greater than the sun's, so that we may largely diminish this estimate of his volume. Still we cannot assign to Sirius much less than 1,000 times the volume of the sun; and probably his mass exceeds the sun's in not much less degree. I have just mentioned the spectroscopic evidence as to the quality of his light. Along with about 300 stars out of 600 observed by Secchi he shows a spectrum remarkable for the great strength of the hydrogen lines; and the assumptions seem permissible that all the stars showing this spectrum belong to a higher class (in regard to size and mass) than our sun, that they are younger suns, and glowing with more intense lustre.

When the spectroscopic method of measuring movements of approach and recession was first suggested it naturally occurred to spectroscopists to apply it to this most splendid star. The method itself may be readily indicated though not quite so readily explained. It is particularly interesting to myself, because it was reading an early adumbration (so to speak) of the actual method, by M. Döppler, which first moved me to commit my thoughts on any scientific subject to paper,—as the printed pages of the *Cornhill* (for December 1863) are "alive this day to testify." I may also mention, with perhaps pardonable pleasure, that I chanced to be the first who publicly enunciated the actual method by which alone successful results can be obtained; albeit this was so clear and obvious a deduction from Döppler's imperfect but pregnant reasoning that it must have occurred to several even earlier, and when I wrote was actually being tested by Huggins and Secchi.

The principle may be thus presented—

Light travels at the rate of some 186,000 miles a second, being propagated (we know not how) in a series of undulations, the broadest waves giving red light, the narrowest giving violet light, the breadths ranging from these extremes through those belonging to the colours red, orange, yellow, green, blue, indigo, and violet. White light is produced by the combination of all these waves just as noise is produced by the combination of multitudinous sound tones. Now when we cross a series of undulations in water, meeting them as they travel onwards, more pass us in any given time than when we are at rest, while if we move the same way so that they overtake us, fewer pass us in a given time. Judging by the number so passing and their known rate of motion, the inference would be (were we unconscious of our own movement) that they were narrower in the former case and broader in the latter than they really are. Apply this reasoning to sound-waves and it will be seen that if we are approaching a source of sound the sound waves should seem narrower, that is the tone higher, while if we are receding from the source of sound the sound waves should seem broader or the tone deeper. Experiment has shown this to be so, the change of tone being found to correspond precisely with that shown by calculation to be due to the measured rate of motion towards or from the source of sound. A rough observation of the change of tone can often be made during railway travelling, especially in America where, besides steam-whistles, bells are used. For it will be found that the tone of the whistle or bell of a passing engine lowers markedly at the moment when in passing the whistle or bell ceases to approach and begins to recede.

Now as light travels in a series of waves it is manifest that the same law must apply to light as to sound. If we are approaching a source of light of one definite tint the light waves will be shortened and therefore the

tint changed in the direction from red towards violet in the spectrum. If we are receding from such a source of light there would be a similar change, but in the opposite direction,—that is from whatever the tint might be to a tint somewhat nearer the red end of the spectrum. All that would be necessary in such a case would be that the velocity of approach or recession should be comparable with the velocity of light, or 186,000 miles per second.

Döppler's idea was that movements of recession or approach among the stars might be indicated in this way, the stars of ruddier tints being those which were receding from us, and those of bluer tints being those which were approaching. He overlooked the circumstance that the stars do not shine with definite tints, but with white light, that is with all the colours of the rainbow combined. It would be as impossible to judge by the sound-wave test of the approach or recession of something moving with noisy clatter, as to determine by the colour-test whether a star is approaching or receding. But if among the sounds producing a noisy clatter were only one whose tone was distinct and known, we might, despite the noise, determine the question of approach or recession. So, if we can select even among the multitudinous tints forming the light of a star a single tint which we know, that tint will tell us of the star's approach or recession, if only the rate of such motion is great enough to cause measurable displacement of the known tint towards either the red or the violet end of the spectrum. Now in the spectrum of Sirius, as already mentioned, the lines of hydrogen are very strong; they are quite unmistakable also *as* the lines of hydrogen, so that the astronomer can compare any given line of hydrogen—say the one in the red part of the spectrum—with the corresponding line of hydrogen as given by the glowing gas in one of his tubes.

The comparison so made by Dr. W. Huggins, the most skilful of our English spectroscopists showed that Sirius was receding from the earth at the rate of more than twenty miles an hour. Later observations at our chief national observatory confirmed his results.

So far only what was originally likely enough had been recognised. The observation, like others applied to the stars, showed a more rapid rate of motion among the stars than many astronomers had supposed to exist. In particular the theory of M. Otto Struve that stellar motions average between three and four miles per second was roughly shaken. But I had already shown from other considerations that Otto Struve was probably mistaken.

But of late years the evidence obtained at Greenwich has tended to show that the motion of Sirius is diminishing. And now it is found that the motion of recession has become so slow that we may expect it presently to change into a motion of approach,—which may probably increase, reach its maximum, then diminish, change into a motion of recession, and so forth, as though Sirius were travelling in a mighty orbit with movements alternately carrying him towards and from our sun.

Now Peters and Auwers long since showed that the thwart motion of Sirius (that is the star's apparent motion on the vault of heaven) is affected by a peculiarity indicating orbital motion. Mr. Alvan Clarke, the celebrated optician of Cambridge, Mass., discovered a companion of Sirius which has been regarded as probably the cause of the motion of Sirius,—*not* the centre round which Sirius is travelling, but the cause of the motion of Sirius around the point which is their common centre of gravity. The orbit estimated from either star as a centre has a diameter not less than 100 times greater than the orbit of the earth round the sun, yet (so great is the combined mass of the two stars) the period of circuit is less than half a century. Supposing the mass of Sirius to be ten times greater than the mass of the faint companion, the orbit of Sirius around the common centre of gravity would have a diameter certainly not less than nine times that of the earth's orbit, and the average velocity of Sirius in that orbit would be not less than a fifth of the earth's velocity in her orbit, while when nearing perihelion a much greater velocity than this might be attained. Supposing that a portion of the velocity which is in the direction towards and from us to be about ten miles per second, and the system to be travelling at about the same rate from the sun, the apparent velocities in the direction of the line of sight would range from rest to a rate of recession of about twenty miles per second.

Whatever be the actual movements of Sirius, orbital or otherwise, it is clear that the new method of measuring motion is capable of giving us such information about these movements as cannot but help us notably in the determination of their true character. The same method applied to Procyon and other leading stars will probably do more to enable science to interpret the constitution of the stellar heavens than any method devised since astronomy became a science.

THE CHEMISTRY OF COOKERY.

XXXV.

By W. Mattieu Williams.

"Pease-pudding hot, pease-pudding cold,
Pease-pudding in the pot, nine days old."

I LEAVE to Mr. Clodd the historical problem of determining whether this notable couplet is of Semitic, Aryan, Neolithic, or Paleolithic origin. Regarded from my point of view it expresses a culinary and chemical principle of some importance, and indicates an ancient practice that is worthy of revival.

I have lately made some experiments on the ensilage of human food, whereby the cellular tissue of the vegetable may be gradually subjected to that breaking up of fibre described in No. 28. One of the curious ahcievements of chemical metamorphoses that is often quoted as a matter for wonderment is that of converting old rags into sugar by treating them with acid. The wonderment of this is diminished, and its interest increased, when we remember that the cellulose or woody fibre of which the rags are composed has the same composition as starch, and thus its conversion into sugar corresponds to the every-day proceedings described in No. 30. All that I have read and seen in connection with the recent ensilage experiments on cattle fodder indicate that it is a process of slow vegetable cookery, a digesting or maceration of fibrous vegetables in their own juices which loosens the fibre, renders it softer and more digestible, and not only does this, but, to some extent, converts it into dextrine and sugar.

I hereby recommend those gentlemen who have ensilage-pits and are sufficiently enterprising to try bold experiments, to water the fodder, as it is being packed down, with dilute hydrochloric acid or acetic acid, which, if I am not deluded by plausible theory, will materially increase the sugar-forming action of the ensilage. The acid, if not over-supplied, will find ammonia and other bases with which to neutralise itself.

Such ensilage will correspond to that which occurs when we gather Jersey or other superlatively fine pears in autumn as soon as they are full grown. They are then hard, woody, and acid, quite unft for food, but by simply storing them for a month, or two, or three, they become lusciously

soft and sweet, the woody fibres are converted into sugar, the acid neutralised, and all this by simply fulfilling the conditions of ensilage, viz., close packing of the fibre, exclusion of air by the thick rind of the fruit, *plus* the other condition which I have just suggested, viz., the diffusion of acid among the well-packed fibres of the ensilage material.

In my experiments on the ensilage of human food I have encountered the same difficulty as that which has troubled graziers in their experiments, viz., that small-scale results do not fairly represent those obtained with large quantities. There is besides this another element of imperfection in my experiments respecting which I am bound to be candid to my readers, viz., that the idea of thus extending the principle was suggested in the course of writing this series, and, therefore, a sufficient time has not yet elapsed to enable me (with much other occupation) to do practical justice to the investigation.

I find that oatmeal porridge is greatly improved by being made some days before it is required, then stored in a closed jar, brought forth and heated for use. The change effected is just that which theoretically may be expected, viz., a softening of the fibrous material, and a sweetening due to the formatiou of sugar. This sweetening I observed many years ago in some gruel that was partly eaten one night and left standing until next morning, when I thought it tasted sweeter, but to be assured of this I had it warmed again two nights afterwards, so that it might be tasted under the same conditions of temperature, palate, &c., as at first. The sweetness was still more distinct, but the experiment was carried no further.

I have lately learned that my ensilage notion is not absolutely new. A friend who read my Cantor lectures tells me that he has long been accustomed to have seven dishes of porridge in his larder, corresponding to the days of the week, so that next Monday's breakfast was cooked the Monday before, and so on, each being warmed again on the day fixed for its final execution, and each being thus seven days old. He finds the result more digestible than newly-made porridge. The classical nine days' old pease-pudding is a similar anticipation, and I find, rather curiously, that nine days is about the limit to which it may be practically kept before mildew—mouldiness—is sufficiently established to spoil the pudding. I have not yet tried a barrel full of pease-pudding or moistened pease-meal, closely covered and powerfully pressed down, but hope to do so.

Besides these we have a notable example of ensilage in sour-kraut—a foreign luxury that John Bull, with his usual blindness, denounces, as a matter of course. "Horrid stuff," "beastly mess," and such-like expressions, I hear whenever I name it to certain persons. Who are these persons? Simply Englishmen and Englishwomen who have never seen, never tasted, and know nothing whatever of what they denounce so violently, in spite of the fact that it is a staple article of food among millions of highly-intelligent people. Common sense (to say nothing of that highest result of true scientific training, the faculty of suspending judgment until the arrival of knowledge) should suggest that some degree of investigation should precede the denunciation.

In the cases of the saur-kraut and the ripening pear there is acid at work upon the fibre, which, as I have before stated, assists in the conversion of such indigestible fibre into soluble and digestible dextrin and sugar; but the demand for the solution of the vegetable casein or legumin, which has such high nutritive value and is so abundant in peas, &c., is of the opposite kind. Acids solidify and harden casein, alkalis soften and dissolve it. Therefore the chemical agent suggested as a suitable aid in the ensilage or slow cookery, or the boiling or rapid cookery, of leguminous food is such an alkali as may be wholesome and compatible with the demands for nutrition.

Now, the analyses of peas, beans, and lentils, &c., show a deficiency of potash salts as compared with the quantity of nitrogenous nutriment they contain; therefore I propose, as in the case of cheese food, that we should add this potash in the convenient and safe form of bicarbonate, not merely add it to the water in which the vegetables may be boiled, and which water is thrown away (as in the common practice of adding soda when boiling greens), but add the potash to the actual pease-porridge, pease-pudding, lentil soup, &c., and treat it as a part of the food as well as an adjunct to the cookery. This is especially required when we use dried peas, dried beans of any kind, such as haricots, dried lentils, &c.

I find that taking the ordinary yellow split-peas and boiling them in a weak solution of bi-carbonate of potash for two or three hours, a partial solution of the casein is effected, producing pease-pudding, or pease-porridge, or purée (according to the quantity of water used) which is softer and more gelid than that which is obtained by similarly boiling without the potash. The undissolved portion evidently consists of the fibrous tissue of the peas, the gelatinous or dissolved portion being the starch, with more or less of casein. I say "more or less" because at present I have not been able to determine whether or not the casein is all rendered soluble. The flavour of the clear pea-soup which I obtained by filtering through flannel shows that some of the casein is dissolved; this is further demonstrated by adding an acid to the clear solution, which at once precipitates the dissolved casein. The filtered pea-soup sets to a stiff jelly on cooling, and promises to be a special food of some value, but for the reasons above stated I am not yet able to speak positively as to its practical value. The experience of any one person is not sufficient for this, the question being, not whether it contains nutritive material—this is unquestionable—but whether it is easily digested and assimilated. As we all know, a food of this kind may "agree" with some persons and not with others—*i.e.*, it may be digested and assimilated with ease or with difficulty according to personal idiosyncrasies. The cheesy character of the abundant precipitate which I obtain by acidulating this solution is very interesting and instructive, regarded from a chemical point of view. The solubility of the casein is increased by soaking the peas for some hours, or, better still, a few days, in the solution of bicarbonate of potash.

Another question is opened by these experiments, viz., what is the character and the value of the fibrous solid matter remaining behind after filtering out the clear pea-soup? Has the alkali acted in an opposite manner to the acid in the ripening pear? Is it merely a fibrous refuse only fit for pig-food, or is it deserving of further attention in the kitchen? Should it be treated with dilute acid—say a little vinegar—to break up the fibre, and thereby be made into good porridge? Other questions crop up here as they have been cropping continually since I committed myself to the writing of these papers, and so abundantly that if I could afford to set up a special laboratory, and endow it with a staff of assistants, there would be some years' work for myself and staff before I could answer them exhaustively, and, doubtless, the answers would suggest new questions, and so on *ad infinitum*. I state this in apology for the merely suggestive crudity of many of the ideas that I throw out in the course of these papers.

Before leaving the subject of peas, I must here repeat a practical suggestion that I published in *The Birmingham Journal* about twenty years ago, viz., that the water in

which green peas are boiled should not be thrown away. It contains much of the saline constituents of the peas, some soluble casein, and has a fine flavour, the very essence of the peas. If to this, as it comes from the saucepan, be added a little stock, or some Liebig's Extract, a delicious soup is at once produced, requiring nothing more than ordinary seasoning. With care, it may form a clear soup such as just now is in fashion among the fastidious, but prepared however roughly, it is a very economical, wholesome, and appetising soup and costs a minimum of trouble.

I must here add a few words in advocacy of the further adoption in this country of the French practice of using as *potage* the water in which vegetables generally (excepting potatoes) have been boiled. When we boil cabbages, turnips, carrots, &c., we dissolve out of them a very large proportion of their saline constituents; salts which are absolutely necessary for the maintenance of health; salts, without which we become victims of gout, rheumatism, lumbago, neuralgia, gravel, and all the ills that human flesh with a lithic acid diathesis is heir to, *i.e.*, about the most painful series of all its inheritances. The potash of these salts existing therein in combination with organic acids is separated from these acids by organic combustion, and is then and there presented to the baneful lithic acid of the blood and tissues, the stony torture-particles of which it converts into soluble lithate of potash, and thus enables them to be carried out of the system.

I know not which of the Fathers of the Church invented fast-days and *soup maigre*, but could almost suppose that he was a scientific monk, a profound alchemist, like Basil Valentine, who in his seekings for the *aurum potabile*, the elixir of life, had learned the beneficent action of organic potash salts on the blood, and therefore used the authority of the Church to enforce their frequent use among the faithful.

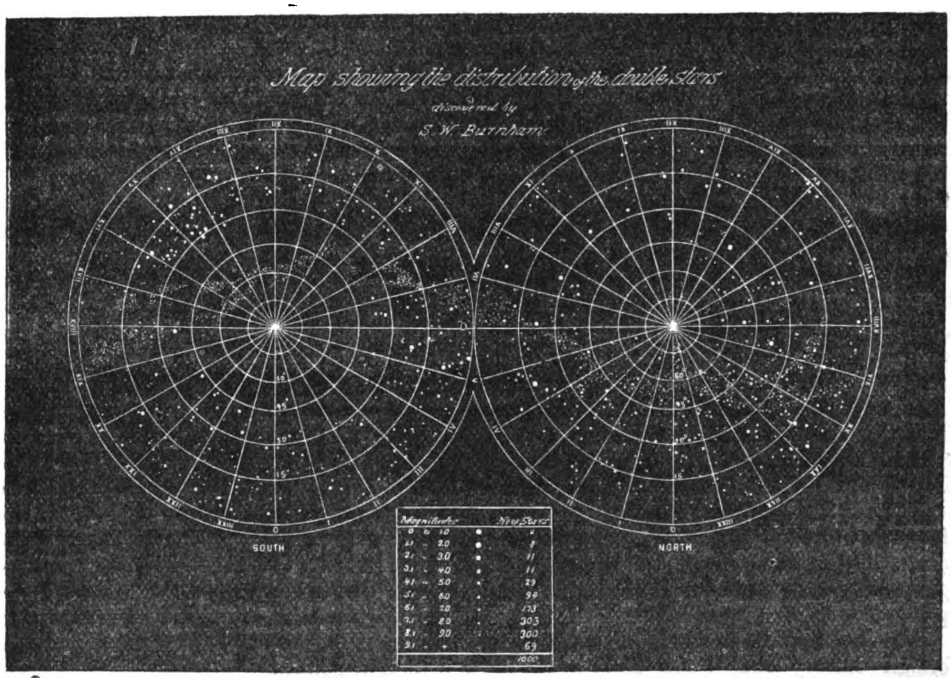
Destruction of Telegraph Wire.—The Indians in Alaska are said to have used, in making salmon nets, the bulk of 900 miles of telegraph wire, which was laid by the Overland Telegraph Company many years ago, at a cost of 3,000,000 dols.—*Electrical Review.*

DOUBLE STARS.

By Mr. S. W. Burnham.

(*Micrometrical Measures of* 748 *Double Stars made with the* 18½-*inch Refractor of Dearborn Observatory.**)

THE accompanying diagram was prepared in the first instance for my own information, concerning the distribution in space of the new double stars contained in this and the preceding catalogues. It then seemed that a reproduction of it might properly find a place at the close of the series. These double stars are as uniformly scattered over the surface of the sky as could be expected when due allowance is made for the unequal distribution of stars generally, and the difference in observing weather at different seasons of the year. The Milky Way, of course, furnishes a greater number of new double stars than the same area elsewhere; and that part of the heavens on the Meridian in the fall and summer months was more carefully examined, and with correspondingly better results.



No systematic search has been made for the discovery of new pairs. At this time, when comparatively so little has been done in the detection of really difficult objects, it is hardly worth while taking the necessary time to follow any special plan in looking for new pairs. No appreciable amount of time can be lost by the repeated examination of stars when the telescope is directed at random. A hundred years hence it may be desirable to make a careful systematic study of all the stars, not known to be double, down to the eleventh or twelfth magnitude, with the great refractors of that time, and especially if some location shall be found where a steady air and good definition can be had most of the time, at certain seasons of the year at least. Under such circumstances a thorough examination by an experienced observer of stars not catalogued as double would lead to valuable results, and would practically exhaust the field of discovery so far as the moderate or smaller apertures used in observatories now established are concerned.

* From the appendix of Mr. Burnham's fourteenth catalogue of double stars, recently published by the Royal Astronomical Society.

The Southern Hemisphere, however, is an almost untried field, particularly that portion within 40° of the South Pole. Very few close pairs have been discovered, and in the region mentioned there is probably not one double star known that would be called difficult with the telescope of this observatory. Assuming a uniform distribution of double stars in the Northern and Southern Hemispheres, several hundred fairly close pairs could be picked up with a small aperture of first-class definition without going below the eighth magnitude. A large number of close and unequal pairs will be found in the naked-eye stars.

The whole number of stars represented in the accompanying map is 1,000, of the following orders of brightness:—

Magnitudes.	No. of Stars.	Magnitudes.	No. of Stars.
0 to 1.0	2	5.1 to 6.0	94
1.1 „ 2.0	8	6.1 „ 7.0	173
2.1 „ 3.0	11	7.1 „ 8.0	303
3.1 „ 4.0	11	8.1 „ 9.0	300
4.1 „ 5.0	29	9.1 „ +	69
Total	—	—	1000

LIMITING DISTANCE OF NEW PAIRS.

My catalogues, particularly the earlier ones, contain a few stars which, from the distance of the components, it would have been better to reject. It may sometimes be desirable to record faint companions to bright stars and previously known pairs when they are beyond the ordinary limits of distance, but it is rare that the maximum distance of the Pulkowa Catalogue, 16″, should be exceeded, and in stars from 8 or 8.5 magnitude down, the companion should generally be within 5″ at the most. Pairs of this distance will rarely prove of much interest. All the rapid and interesting binaries are much closer.

Omitting some of the wider pairs, and also the close pairs which have not been measured, there are left 743 double stars out of 1,000 which are represented by the following mean distances:—

Magnitude.	No. of Stars.	Mean Distances. ″
3.1 to 4.0	6	1.88
4.1 „ 5.0	16	1.89
5.1 „ 6.0	61	1.80
6.1 „ 7.0	119	1.92
7.1 „ 8.0	240	1.36
8.1 „ 9.0	247	1.57
9.1 „ +	54	1.63
Mean distance of 743 stars =		1.58

In the early history of double-star astronomy, when for this work instruments were inferior as well as smaller, there might have been some excuse for including in a catalogue wide pairs, and those where both components were faint; but at this time there is no justification for ever calling two tenth-magnitude stars which happen to be 5″ or 10″ apart a double-star in the proper sense of the term. At any time since the publication of the work of Struve the time spent in observing such objects would be little less than lost, and this will necessarily be the case for all time to come. Of course it is very easy to make with a telescope very moderate in size and indifferent in definition, an imposing catalogue of so-called discoveries, so far as numbers go, if these faint and wide couples are included; but the value of such a list now, and in the future, will depend solely upon the number of first-class pairs it contains. The Pulkowa Catalogue, as given in Vol. IX. of the publications of that observatory, will serve as a model for this class of work so far as the element of distance is concerned. A close pair—that is, where the distance does not exceed 1″ should be recorded, however faint the components may be, and, of course, the magnitude of the companion to any star should have no weight in retaining or measuring it as a double-star.

An examination of the various double-star catalogues of original entry with reference to the proportion of close pairs is instructive as illustrating the relative number of systems of this kind in each, as well as showing the greater perfection of modern telescopes for this kind of work. Those of the Struves are so well known that further comment is unnecessary.

The seven catalogues of Sir John Herschel contain nine stars where the estimated distance is 1″ or less. Of this number I have examined and measured seven, the distances being 2″.5, 1″.81 ("violent suspicion," H., 1″.02 ("almost certain," H.), 2″.27, 3″.26, 1″.2 ("not verified," H.), and 1″.80. The remaining two could not be found, one being noted as doubtful by Herschel. So that at most we have only two pairs of Class I. Herschel gives 68 stars of Class II., estimated at $1\frac{1}{2}$″ to 2″, all of which, with, perhaps, three or four exceptions, are below the ordinary limits of magnitude, most of them being from 10 to 12 magnitude. In all, about 24 of these stars have recently been observed, mostly at Cincinnati and Chicago, of which 16 exceed 2″ in distance, and four were not found as described, four pairs only coming within the limits of Class II. It is practically certain that of the remaining 44 stars there would not be more than a dozen that would come within the required distance. I have assumed the number, however, to be 20.

The following table gives all the principal original double-star catalogues published, and the number of pairs in each of Class I. (distance from 0″ to 1″) and Class II. (distance from 1″ to 2″). In the last column is given the ratio of stars of these classes to each thousand double-stars catalogued by the discoverer:—

	Class I.	Class II.	Total.	Ratio.
Burnham, Catalogue of 1,000 stars	226	254	520	520:1000
O. Struve „ „ 547 „	154	63	217	400:1000
Struve „ „ 2,640 „	91	314	405	150:1000
Herschel I. „ „ 812 „	12	24	36	45:1000
Herschel II. „ „ 3,429 „	2	20	22	7:1000
Alvan G. Clark	14	1	15	
All other observers	40	75	129	
			1344	

From this investigation we find the pairs having a distance not exceeding 2″ are less than 1,400; and it is safe to say that the total number of stars now known which can be properly called double is but little if any greater than 3,000, and that at least three-fourths of the remainder are not worth observing for any reason.

THE PATENT ACT OF 1883.

BY POLYGLOT.

(Continued from page 369.)

BE careful not to make your claims too wide; because, if they embrace more than was new at the time you filed your application, even if the old part be of but minor importance, your whole patent is vitiated. Where an old part is used in a novel manner, and in connection with others that are new, it is often desirable to preface the claim with what is known as a "disclaiming clause," as in the following example, still referring to our supposed focussing gear:—"I am aware that racks have been previously applied to the focussing gear of photographic cameras, and I therefore do not claim such rack in itself, but what I claim is: The focussing gear for a photographic camera, consisting of a rack, a slotted guide, and a toothed wheel

with a thumb-screw, all substantially as described and shown in the drawings."

I have already mentioned that drawings must accompany the complete specification. As I have, however, given no particulars about mere formalities relating to the specification, I will follow the same rule with reference to the drawings.

Where no provisional specification is to be filed, the declaration previously spoken of will be accompanied by the complete specification, together with drawings where these are desirable.

Whether the complete specification has been filed in the first instance or after the provisional, it will be examined by the patent office. If it appears defective, the applicant is required to amend it; if it is found satisfactory, he is notified and the acceptance advertised. It is now open to inspection, and any one who has any objection to the grant of letters patent for the invention, or to the applicant, may oppose the issue of the patent. The grounds on which an opposition may be based are stated in the Act in these words:—

". . . . On the ground of the applicant having obtained the invention from him (*viz.*, *the opposing party*, *P.*) or of a person of whom he is the legal representative, or on the ground that the invention has been patented in this country on an application of prior date, or on the ground of an examiner having reported to the comptroller that the specification appears to him to comprise the same invention as is comprised in a specification bearing the same or a similar title, and accompanying a previous application *but on no other ground* (the Italics are mine, P.)."

Oppositions must be lodged within two months from the advertisement in the "*Patent Office Journal*" of the acceptance of a complete specification. If no opposition arises, or if the opposition is unsuccessful, the patent issues.

The patentee now has a title which he holds without any guarantee from the Government (or any one else for that matter) of its validity. The examination above spoken of is merely a comparison of the applicant's specification with those of others to whom patents have not been issued yet. As soon as the patent is granted, the invention is supposed to be known to the public at large, and the examiners cease to take any note of it. It therefore follows that holding of a patent is by no means proof of either the novelty, the utility, or the practicability of the invention. These must, perforce, be left to the judgment of the patentee, or to that of the courts of justice; but, in determining whether or not a patent is good, I think the courts will, as a rule, be inclined rather to give the patentee the benefit of a doubt than otherwise, unless his *bona fides* be questionable.

It may, however, be found that an invention, though embracing the whole or parts of another previously patented one, is yet a distinct advance upon such prior patent. Then, though the earlier patent bars the use of the second invention without the first patentee's consent until the first patent expires and thus becomes public property, yet the first patentee must equally refrain from using the second one's improvement.

If the first patent has still a considerable time to run, it is probably best if the two agree—sad to say, poverty often puts one man at the mercy of his richer opponent. If, however, the first patent has nearly run out, it may pay the second man to hold out and wait till it expires, as he then perchance may make money by his better apparatus or process.

The patentee, if his rights do not conflict with those of others, has the absolute and sole privilege to "make use, exercise, and vend" his invention to the entire exclusion of all others (with the exception of inventions for warfare, with regard to which certain special reservations have been made in favour of the Crown), and no one is entitled to counterfeit his invention or to interfere with his use thereof unless he employs it in a manner which is, *per se*, contrary to law. It is a very common error to suppose that any man may for his private use lawfully make a patented article. Whosoever does so without leave acts in contravention of the patentee's rights, is an infringer, and, as such, liable to be sued. The new Patent Act, however, contains a proviso according to which the Board of Trade may, under certain conditions, compel the patentee to grant licences for the use of his invention upon a fair consideration. Whether this section of the Act will ever be more than a dead letter, or whether it will work well or ill, remains to be seen. It is to be feared that the clause may lead to much trouble, and may sorely perplex inventors, poor ones especially, when they are attacked by rich and unscrupulous opponents. It has furthermore to be borne in mind that a patentee in selling an article made in accordance with his invention is held to have sold it with full licence for the free use thereof, unless there is an express understanding to the contrary.

Another point to be remembered is, that any one purchasing a counterfeit of a patented article, or an article infringing a patent, though he may have acted in perfect good faith, is yet liable to pay royalty for the use of such article; but he can, I think, seek redress at the hands of the vendor.

The privilege of a patentee lasts for fourteen years from the date of the application; during the first four years unconditionally, for the remaining term on condition that certain taxes, amounting in the aggregate to £150, be paid in such sums (from £10 to £20 per annum) as the Act directs.

I think I have now said as much on the subject as can conveniently be comprised within the limits prescribed by the nature of an essay like this. I have abstained from describing any of the procedure in oppositions and appeals, as no man ought to meddle with these without proper assistance, and can say nothing about law-suits connected with patent matters, which require to be dealt with by a better man, by far, than I am. In connection with this, I would only caution readers that all "law" is expensive, but suits in patent law are, by their very nature, doubly so, because in most cases they not only necessitate the assistance of first-class counsel — specialists in patent matters—but, moreover, almost invariably call for a great deal of costly expert evidence, and experts very frequently hold conflicting views on the questions submitted for their opinion.

I may summarise my experience about patents by saying that, though on the one hand there is no more valuable property than a good patent for a good invention, yet much caution is required before a man builds great hopes upon an invention. In every field of industry there are yet vast uncultivated spaces ready to yield a golden crop, but for its fruition business capacity, a sure insight into the wants of the public, and an untiring energy are needed quite as much as ingenuity and constructive skill.

I may conclude with the wish that what I have writ may prove instructive to some of the multitude of readers of KNOWLEDGE; and having begun, like the monks of old, with a salute to the reader, let me like them conclude with the greeting, "*Lector, vale.*"

GERMAN STEAM NAVIGATION.—A Bill has been submitted to the German Parliament proposing to grant State subventions to the extent of £204,750 to a line of steamers to Eastern Asia and Australia.

SENT TO THE BOTTOM.

BY RICHARD A. PROCTOR.

(*Continued from page* 389.)

IT remains that I should touch on a form of danger to which ocean-going steamers are exposed on certain well-frequented routes,—a danger more subtle and terrible even than the risk of collision. I refer to the danger of running on an iceberg in the night time. It can hardly be doubted that among those splendid steamers which have started under seemingly favourable conditions on the journey between America and Europe, well manned, well provisioned, and well formed in all respects, and have never more been heard of, some have been lost through collision with an iceberg. The stories of narrow escapes, of cases where an iceburg has been passed within a stone's throw, or of even narrow escapes where an iceberg has been actually struck, but the strength of the ship's compartments have saved her from foundering, show how reasonable is this explanation of the loss of ships which could have outridden almost any storm that has ever raged in the North Atlantic. On a dark night there may be no warning even when there is no haze. The keenest watchman cannot detect the outline of one of these long, low icebergs which have been approached even in full daylight, with bare time to escapo their formidable sides. When there is the least fog a low-lying iceberg is a still more treacherous enemy.

If we consider the case of the *Arizona*, in October, 1879, we shall see how subtle are the risks which icebergs and icefields cause, even when a ship is in the hands of the most careful commander. I was a passenger by her on the journey to New York preceding the accident, which occurred on the return voyage. I remember discussing with one of the officers (not when he was on duty, courteous reader) the risks from icebergs, and his admission that there was no way of ensuring safety from a danger so subtle, especially in the case of so swift a ship as the *Arizona*. On the return voyage, she was steaming gaily along at the rate of more than fifteen knots an hour; it was night, and there was no thought of danger, still less of disaster; some of the passengers were engaged in the saloon in the dignified employment of speculating on the day's run (to the following noon); when a shock which threw most of the speculators to the floor showed that whatever run she might make would certainly be far short of the high numbers on which they had been wagering. In a few moments every one was on deck, watching the dim outlines of two towering ice pinacles, which seemed to threaten the ship's immediate destruction. But when she had been backed from her dangerous proximity to the iceberg, it was seen that she was exposed to a still more serious danger,—she was settling down (apparently) by the head, with a list to starboard, which showed that a side compartment as well as her forward one had been broken into by the force of the shock. Fortunately the builders had done honestly by the owners. The stout framework and bulwarks of the compartments held firmly, and though crippled she remained afloat. She did not, however, complete her journey to the old country, but bore up to St. John's, N.B., to refit.

Now seeing that the most careful watch and the keenest eyes do not suffice to save a ship from the danger of collision with an iceberg, the question arises whether science cannot provide a keener sense than human eyesight to provide safety against this terrible danger. The only sense which seems available, failing the sense of sight, is the sense by which heat is perceived (properly distinguished long ago by Stewart Reid from the sense of touch, and undoubtedly to be regarded as our sixth sense). The fall of temperature, however, as an iceberg is approached, though very obvious near by, is not recognisable at a sufficient distance or with sufficient quickness, by human sense-organs. What is wanting is a scientific heat-feeler or temperature-noter, by which an iceberg half a mile away or more could be recognised. There are some heat measurers which are so delicate that they respond to the heat of a cigar burning ten or twelve yards away, and to even slighter sources either of heat or of cold, that is either of rise or fall of temperature. Now imagine a suitably-constructed instrument of this sort, adjusted for the actual temperature of the region through which the ship was passing; then the approach of the ship to an iceberg even some half-mile or so away would cause the index to move to the side showing cold, through an appreciable angle, the deviation increasing every second as the ship advanced. Nothing could be easier than to make the index antomatically start suitable alarm signals,—horn, whistle, or the like; and on moving further over to the cold side, causing the engineers to receive warning to slow or stop their engines; nay the index could easily, if it were thought desirable, be made to set mechanism in motion for stopping and reversing the engines.

An instrument such as this might also be so constructed as to show the direction of the source of danger—for if there were (say) a triple indicator one directed forwards, another towards the port quarter, and the third towards the starboard quarter, the first to signal an iceberg would be the one whose face was directed nearest to the point where the iceberg lay. The two quarter indicators would in this case rather show the neighbourhood of ice than the presence of any actually dangerous ice mass. But whenever the side indicators told of icebergs, it would be seen that there was occasion for very careful watch, and cautious progress.

Certainly, when we remember with what zeal science is applied to the invention of contrivances by which life and property may be destroyed—the long range guns, mitrailleuses, torpedoes, and the like which afford such striking evidence of our advance beyond the savage (who can only smash a single head by main force with his club, or pierce one body with his spear)—it seems as though science might with advantage devote a small portion of her ingenuity to the invention of measures whereby life and property may be preserved. The law of the survival of the fittest would still operate as effectively when science is applied to preservation as when it is applied to destruction; and Science could have afforded an answer to those who ask whether, despite the improvement in our appliances for travel, transit of goods, manufactures, and so forth, the influence of increased knowledge has always been such as to justify the aspiration

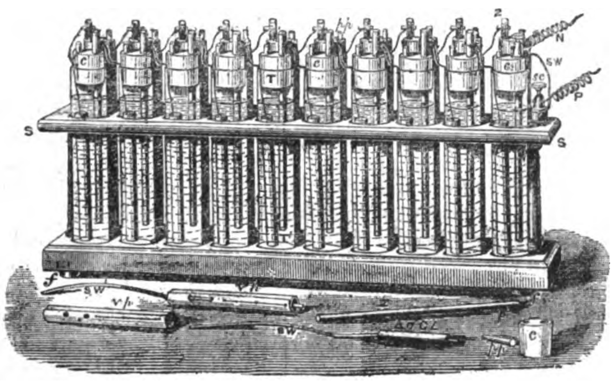
Let Knowledge grow from more to more.

THE AMATEUR ELECTRICIAN.

BATTERIES.—X.

THE only other form of battery to which we deem it desirable to draw attention is the one introduced by Messrs. Warren de la Rue, and Hugo Müller. Although reserved till the conclusion of this series of papers, the cell is not by any means the least important or useful of those referred to. On the contrary, there is every probability that it is destined to be extensively adopted by experimentalists, and for certain species of practical applications.

In the accompanying illustration is depicted a tray containing ten cells. There are also illustrated the various parts of which each cell is composed. The outer, or containing vessel, is a glass tube about 1 inch in diameter, and $5\frac{1}{2}$ inches long. A paraffin stopper (c) is made to fit the tube, and is provided with two holes, through one of which the positive plate, consisting of a zinc rod (z) $\frac{3}{16}$ inch in diameter, is passed and fixed by means of a little melted paraffin. The negative plate is pressed or wedged in



between the stopper and the glass, and consists of a flattened silver wire (SW) around which is cast a quantity of chloride of silver (AgCl) until a thickness of five-sixteenths of an inch is attained. Through the second hole in the stopper a solution of ammonic chloride (containing $2\frac{1}{2}$ per cent. of salt) is poured. The cell is then complete. When the circuit is made by joining the silver and zinc, a current is produced of great constancy. The zinc acting with a double equivalent of the ammonic chloride ($2NH_4Cl$) produces chloride of zinc ($ZnCl_2$), and sets free two equivalents of ammonia ($2NH_3$). The remaining hydrogen (H_2) acts on two equivalents of the silver chloride ($2AgCl$) and producing two equivalents of hydrochloric acid ($2HCl$) releases the silver (Ag_2). The action may be represented by equation thus :—

$$Zn + 2NH_4Cl + 2AgCl = ZnCl_2 + 2NH_3 + 2HCl + Ag_2.$$

The ammonia and hydrochloric acid thus produced combine to form two new equivalents of ammonic chloride, so keeping the quantity of that salt constant. The net result, in the absence of secondary reactions, is the gradual decomposition of the silver chloride, the solution of the zinc, and the accumulation of chloride of zinc. The silver is separated in a spongy form in the neighbourhood of the silver plate or wire, the available surface of which is thus being constantly increased. It will be seen that the principles involved are almost identical with those pertaining to the Daniell cell, the fused and insoluble chloride of silver taking the place of the soluble sulphate of copper. The insolubility of the silver salt obviates the necessity for using a porous pot, and therefore the internal resistance of the cell is very materially affected for the better. The use of so small a zinc rod is warranted by the absence of chemical action during the time the cell is not producing a current. Our readers will doubtless remember that the Leclanché cell was described as one capable of lasting a year or more from a similar cause. Mr. De la Rue stated, in 1882, that he had a battery made up six years previously, and which was still in action, loss of the fluid by evaporation having been from time to time made up. The electromotive force of the cell is 1·03 volts, or nearly that of a Daniell cell, which is 1·079.

In consequence of the small compass which the cell occupies and its non-wasteful properties, it lends itself remarkably to the requirements of the experimentalist who wishes at a moment's notice to produce a reliable current of considerable force. Mr. De la Rue had the cells grouped in trays containing twenty (sometimes more), and the trays placed in cabinets containing in some instances 1,200 cells, in other 2,160 cells. His entire battery thus made upon comprised 14,400 cells, which joined "in series" gave an electro-motive force of 14,832 volts. With the aid of such a battery (the largest ever employed in one continuous circuit) many most interesting experiments were performed bearing on the characteristic features of lightning, the aurora, and other phases of atmospheric electricity. Occasion may present itself later on to dilate upon this most important and little-known topic, but for the present we can only mention that the results of Mr. De la Rue's and other investigators' experiments demonstrate that, to produce a lightning discharge one mile long, a potential equal to that derived from upwards of 3,000,000 cells would be required.

It may be mentioned that one of the applications of the chloride of silver battery (the name by which the De la Rue cell is generally known) is in the "electric gas lighter." In this ingenious piece of apparatus, which is certainly unpretentious in appearance, a small ebonite tube, containing a miniature induction coil, is screwed on to another similar tube containing the cell. On pressing a little button, the current passes through the primary circuit of the induction coil, and induces a current in the secondary coil. The induced spark passes between two points, and, if the end of the apparatus carrying the points is placed in the escaping gas, ignition takes place.

Summing up what has been said on the various forms of battery in use, their several applications may be thus briefly noticed :—

1. Where a continuous steady current is required, more particularly if not of unusually high electro-motive force, the Daniell or one of its modifications stands pre-eminent, and its continued use in telegraphy and electro-chemistry is fully justified.

2. Where a current of high electro-motive force is necessary, and where its duration is required for a few hours (as in electric-light experiments) the Grove, Bunsen, or double-liquid Bichromate should be employed, preference being given to the Grove if the battery has to be carried about, or if space is a consideration. Its compact construction gives it this advantage. The Bunsen lasts longer than the Grove because its cylindrical form, &c., involves the use of larger quantities of acids, and which, therefore, are less rapidly decomposed or diluted. The objection to the Bichromate is the greater trouble involved in the preparation of its solution, but it has the advantage of not producing fumes of nitric peroxide, the evolution of which renders the adoption of the Grove or Bunsen in a room highly objectionable.

3. Where currents are only required at intervals, more particularly if the intervals are long and the time during which the current is called for short, none can beat the Leclanché.

The chloride of silver cell may be used to suit requirement 1, but is perhaps more applicable to requirement No. 3.

The frequent demands on our space have necessitated the omission of a few forms of battery to which we should have liked to refer in order to make the series more complete; but it is hoped that, notwithstanding these unavoidable ostracisms from our original programme, our readers will have seen much that is useful and instructive, and, furthermore, that they will be enabled to decide upon the battery most suited to any particular work. To so enable them was our primary object in writing.

FOUCAULT'S PENDULUM EXPERIMENTS.

By RICHARD A. PROCTOR.

SCIENCE owes to M. Foucault the suggestion that the motions of a pendulum so suspended as to be free to swing in any vertical plane might be made to give ocular demonstration of the earth's rotation. The principle of proof may be easily exhibited, though, like nearly all the evidences of the earth's rotation, the complete theory of the matter can only be mastered by the aid of mathematical researches of considerable complexity. Suppose A B (Fig. 1) to be a straight rod in a horizontal position bearing the

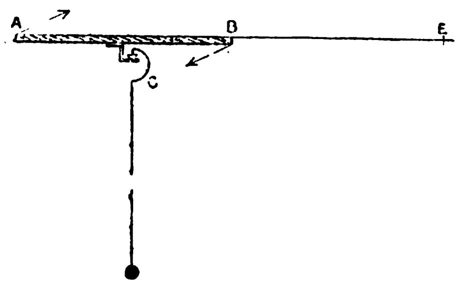


Fig. 1.

free pendulum C D suspended in some such manner as is indicated at C; and suppose the pendulum to be set swinging in the direction of the length of the rod A B, so that the bob D remains throughout the oscillations vertically under the rod A B. Now, if A B be shifted in the manner indicated by the arrows, its horizontality being preserved, it will be found that the pendulum does not partake in this motion. Thus, if the direction of A B was north and south at first, so that the pendulum was set swinging in a north-and-south direction, it will be found that the pendulum will still swing in that direction, even though the rod be made to take up an east-and-west position.

Nor will it matter if we suppose B (say) fixed and the rod shifted by moving the end A horizontally round B. Further, as this is true whatever the length of the rod, it is clear that the same fixity of the plane of swing will be observed if the rod be shifted horizontally, as though forming part of a radial line from a point E in its length. In these cases the plane of the pendulum's swing will indeed be shifted *bodily*, but the direction of swing will still continue to be from north to south.

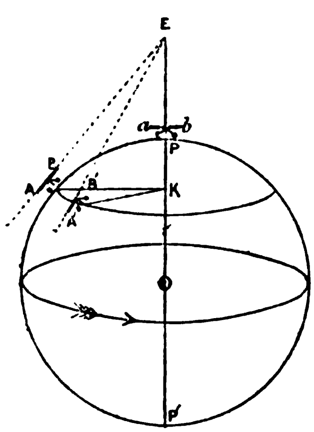


Fig. 2.

Now, let P O P' represent the polar axis of the earth; *a b* a horizontal rod at the pole bearing a pendulum, as in Fig. 1. It is clear that if the earth is rotating about P O P' in the direction shown by the arrow, the rod *a b* is being shifted round, precisely as in the case first considered. The swinging pendulum below it will not partake in its motion; and thus, through whatever arc the earth rotates from west to east, through the same arc will the plane of swing of the pendulum appear to travel from east to west under *a b*.

But we cannot set up a pendulum to swing at the pole of the earth. Let us inquire, then, whether the experiment ought to have similar results if carried out elsewhere.

Suppose A B to be our pendulum-bearing rod, placed (for convenience of description merely) in a north-and-south position. Then it is clear that A B produced meets the polar axis produced (in E, suppose), and when, owing to the earth's rotation, the rod has been carried to the position, A' B', it still passes through the point E. Hence it has shifted through the angle A E A', a motion which corresponds to the case of the motion of A B (in Fig. 1) about the point E,* and the plane of the pendulum's swing will therefore show a displacement equal to the angle A E A'. It will be at once seen that for a given arc of rotation the displacement is smaller in this case than in the former, since the angle A E A' is obviously less than the angle A K A'.† In our latitude a free pendulum should seem to shift through one degree in about five minutes.

It is obvious that a great deal depends on the mode of suspension. What is needed is that the pendulum should be as little affected as possible by its connection with the rotating earth. It will surprise many, perhaps, to learn that in Foucault's original mode of suspension the upper end of the wire bearing the pendulum-bob was fastened to a metal plate by means of a screw. It might be supposed that the torsion of the wire would appreciably effect the result. In reality, however, the torsion was very small.

Still, other modes of suspension are obviously suggested by the requirements of the problem. Hansen made use of the mode of suspension exhibited in Fig. 3. Mr. Worms,

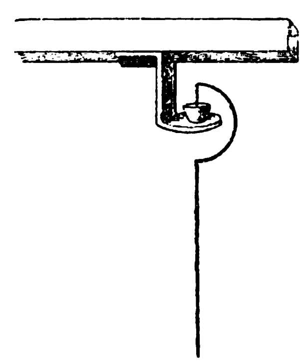


Fig. 3.

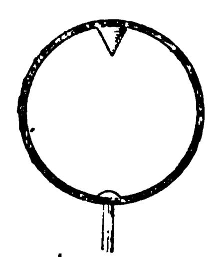


Fig. 4.

in a series of experiments carried out at King's College, London, adopted a somewhat similar arrangement, but in place of the hemispherical segment, he employed a conoid, as shown in Fig. 4, and a socket was provided in which the conoid could work freely. From some experiments I made myself a score of years ago, I am inclined to prefer a

* In reality A E moves to the position A' E over the surface of a cone having E P' as axis, and E as vertext; but for any small part of its motion, the effect is the same as though it travelled in a plane through E, touching this cone; and the sum of the effects should clearly be proportioned to the sum of the angular displacements.

† In fact, the former angle is less than the latter, in the same proportion that A K is less than A E, or in the proportion of the sine of the angle A E P, which is obviously the same as the sine of the latitude.

plane surface for the conoid to work upon. Care must be taken that the first swing of the pendulum may take place truly in one plane. The mode of liberation is also a matter of importance.

Many interesting experiments have been made upon the motions of a free pendulum, regarded as a proof of the earth's rotation; and when carefully conducted, the experiments have never failed to afford the most satisfactory results. Space, however, will only permit me to dwell on a single series of experiments. I select those made by Mr. Worms in the Hall of King's College, London, in the year 1859:—

"The bob was a truly turned ball of brass weighing 40 lb., the suspending medium was a thick steel wire; the length of the pendulum was 17 ft. 9 in. The amplitude of the first oscillation was 6° 42′, and during the time of the experiment—about half-an-hour—the arcs were not much diminished. As I had to demonstrate to a large number of spectators, I encountered considerable difficulty," says Mr. Worms, "in rendering the small deviations of the plane of oscillation visible to all. I accomplished it in three different ways." These he proceeds to describe. He had first a set of small cones set up, which were successively knocked down as the change in the plane of the pendulum slowly brought the pointer under the bob to bear on cone after cone. Secondly, a small cannon was so placed that the first touch of the pendulum-pointer against a platinum-wire across the touch-hole, completed a galvanic circuit, and so fired the cannon. Lastly, a candle was placed so as to throw the shadow of the pendulum-bob upon a ground-glass screen, and so to exhibit the gradual change of the plane of swing.

The results accorded most satisfactorily with the deductions from the theory of the earth's rotation.

THE ENTOMOLOGY OF A POND.

By E. A. Butler.

THE SURFACE (*continued*).

A LITTLE creature called *Velia currens*, something like a small *Gerris*, but rather stouter and still more spider-like, is one of the commonest objects on every piece of water, seeming, however, to prefer parts where the surface is not too placid. It is about one-third of an inch in length, of a dark brownish colour, with orange markings. Dozens of them, consisting of individuals of various ages, may often be seen congregating in a quiet corner, or even sometimes just under a spot where a miniature waterfall supplies sufficiently rapid streams to call forth all their energies in order to keep themselves from being carried

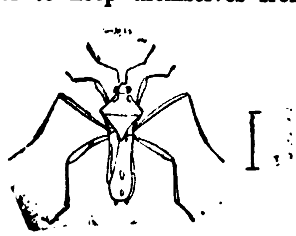


Fig. 1.—Velia Currens. Winged.

away by the current. This insect is almost always destitute of wings, but very occasionally a few examples are found fully developed, with dark-brown wings, adorned with a few snow-white spots. (Fig. 1.) A minute relative of Velia, *Microvelia pygmæa*, occurs sometimes on the surface of streams, but being only one-sixteenth of an inch long, it needs close looking for. It is black, marked with grey.

The other section of the surface fauna is Coleopterous, and its strangest representatives are, unquestionably, the beetles called "Whirligigs, (Fig. 2,) which constitute the

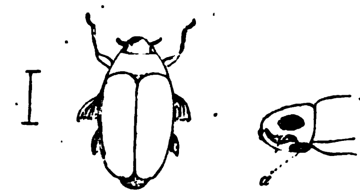


Fig. 2.—Gyrinus Natator, and side view of head. *a*.—Eyes.

family *Gyrinidæ*. They are oval, blackish insects, highly-polished, and with more or less of a metallic reflection. Like the Hemiptera, they are gregarious, occurring in little troops which are incessantly executing the most intricate gyrations, crossing one another's paths again and again, apparently in the most aimless manner, but really with that supreme end in view, the obtaining of food. When watching a company, one sometimes becomes conscious of a sudden diminution in their numbers, some individuals seeming to vanish; a careful scrutiny, however, detects the missing members at some depth in the water, whither they have temporarily dived, in consequence of some sudden scare. Presently, one by one they rejoin their comrades and at once begin again to take part in the mazy evolutions which seem the chief business of their life. Suppose, now, we have managed to secure some specimens from the lively company. Our first observation will undoubtedly be that they smell most disgustingly; in fact, they exude a fœtid milky fluid from various parts of their bodies, which must make them uncommonly nasty morsels to any creature that has the temerity to try to make a meal of them. Now these Gyrini are wonderfully well protected against possible foes; besides their horrid smell, which one would think might be almost enough by itself, there is the hard armour in which they are encased, the highly polished and well rounded surface of which renders them extremely difficult to hold. Then there is a most remarkable arrangement of the eyes, which gives them full command of both elements, air and water: the two customary eye-masses are each sub-divided into two sets by a pit, in which the attachment of the antenna is lodged: this throws one set on to the upper part of the head and the other completely beneath (Fig. 2a), a most excellent distribution, the reason for which is obvious. As the insect rests on the water, its eyes would, if normally placed, just come in line with the surface, the instability of which would considerably interfere with distinctness of vision: to obviate this difficulty, therefore, one is placed sufficiently above, and the other sufficiently below, to be out of the reach of the disturbing effects of surface irregularities, the upper being used for the vision of terrestrial and aërial, and the lower for that of sub-aqueous objects. Creatures like this, active, hard, malodorous, and possessed, so to speak, of quadruple vision, cannot but be well able to hold their own in the struggle for existence.

The legs have undergone great modification; the two hinder pairs are much shortened and flattened, the terminal joints reminding one somewhat of the fin of a fish. The front pair are of ordinary form, and considerably longer than the others, and are used for seizing the prey, which consists of small insects. The elytra are strongly arched and cover a pair of ample wings, whereby the creature is enabled to migrate from pond to pond when the water gets

low, a feat which, owing to the peculiar formation of the legs, would be impracticable in any other way. In their earlier stages, also, these insects are peculiar. The eggs are laid in rows on water-plants, and the larvæ proceeding from them look something like aquatic centipedes, for they are flattened and elongate, and behind the six legs, on the anterior segments, carry on each successive segment a pair of filamentous appendages, one on each side, not locomotive, however, but respiratory in function. After a predatory life below the surface the larva climbs some water-plant, and, having found a comfortable resting-place *out* of the water, spins there a paper-like oval cocoon, whence in due time the beetle issues, ready to descend again to its native element.

We may also class with the surface fauna certain beetles which, though not strictly aquatic, take up their abode on water-plants, such as water-lilies, the broad leaves of which form so many floating islands. They are chiefly stout-legged, large-footed insects, belonging to the genus *Donacia*, which contains nineteen British species, all vegetable-feeders, and attached to water-plants. They are clothed with hard integuments adorned with gorgeous colours, which glisten in the sunshine like gems, and as they are gregarious, the little companies that may sometimes be seen assembled on one of their leaf-islands, form a picture the beauty of which cannot be realised by those who have only seen the insects, bereft of all the sparkling glow of life, arranged in orderly rows on little pieces of cardboard in the collector's cabinet. Their larvæ live in the stems of water-plants, and the pupæ are enclosed in transparent silken cocoons. These beetles are essentially children of the sun, and are found principally during the three summer months.

Sometimes the leaves of water-lilies are found to be pierced with numerous holes; these are marks of the presence of the larvæ of another beetle, of no very handsome appearance, named after the plant, *Galeruca nymphea*; its whole life is spent upon the leaf, which forms its food as well as resting-place; the larvæ flourish on their succulent diet, and soon acquire considerable obesity, and they and the compact pupæ may frequently be seen from a distance dotting the surface of the leaf as so many dark specks.

From the point of view of the entomological collector, no account of the surface fauna would be considered complete without a reference to those hapless insects whose floating corpses testify to the frequency of tragedies which terminate in "accidental death by drowning." High winds, driving before them frail creatures whose feeble wings have betrayed the trust too readily reposed in them, or blowing less venturesome ones off the trees and plants to which they have vainly endeavoured to cling, are the chief cause of these catastrophes; but when such tempting feasts as are to be found in the honey of sallow blossom hang over a pool, the surface beneath is often strewn with insects, chiefly moths, which, having imbibed too freely of the intoxicating juice have fallen off their perches in drunken inanity, and paid for their intemperance with their lives. Delicate insects such as these are, of course, pretty sure to be completely spoilt by the water, but those of stouter and stronger build are often none the worse for many hours' immersion; any surface of water, therefore, is always regarded by the collector as a good trap during a high wind.

(*To be continued.*)

THE Congress on electrical units which met in Paris last week, has decided unanimously that the length of the column of mercury of one square millimètre section which shall represent the legal ohm., shall be 106 centimètres. This is equal to 1·0114 B.A. units (the standard at present in use).

THE INTERNATIONAL HEALTH EXHIBITION.

BY JOHN ERNEST ADY.

II.

["John! Did you shake that bottle of beer? No, Sir, but I *will*." (*Shakes the bottle.*)—*Explosion and John's exit accelerated.*]

TO enter into the philosophy of the question why John should have retired precipitately would seem to be ridiculous to most persons, yet, nevertheless, there must have been some weighty reason for such a procedure. Of course, everybody knows *why* the bottle ought not to have been "shaken before taken," but few are aware of the fact, that in the sediment at the bottom lies the secret of the excellence of bottled beer,—its sparkling property and sharpness are due to the presence within it of carbonic acid gas (carbon dioxide).

It has hitherto been the practice of beer-bottlers to allow the process of fermentation to go on within the bottle after it has been sealed, or, in other words, to let nature supply the carbonic acid gas. But how is this effected?

Fig. 1.—*Saccharomyces Cerevisiæ.* The Yeast plant × 1,200 diameters. *a* shows a cluster of yeast cells developed from each other and remaining attached. The central spot is a space or vacuole, and it may be observed that in the oldest cell (the lowermost) it is largest, nearly all the protoplasm having dwindled away. The younger the cell the richer is it in protoplasm; *b* a cell crushed to show that it contains protoplasm (p), and cell-wall (w); *c* a single cell, stained magenta, to show that *v* is in reality a space, and not a denser portion or nucleus; *d* treated with caustic potash, dissolves the protoplasm, hence the vacuole vanishes. All the above may be taken as a condensed ordinary life-history of the yeast of beer.

The fermentative process is due to minute plants which constitute the yeast or "barm" of the brewer. Fig. 1 shows some of these plants magnified 1,200 diameters; they are each only about $\frac{1}{2000}$th of an inch in size. Their action upon the prepared malt or "wort" is to convert its cane into grape sugar, and to induce alcoholic fermentation in the latter; this they can only do *whilst living.** Moreover, all living things, whether plant or animal, require oxygen for their respiration, and eliminate carbonic acid gas. Now, when the beer is ready for consumption, some of the yeast yet remains to be got rid of. Those yeast cells which have lived their little day have performed their functions and decayed; their bodies sink to the bottom, and, if allowed to remain there, would eventually rot away; decomposition brings in a crowd of other forms of life, invariably heralded by the *mycoderma*, which turns the beer sour, and *bacteria* soon follow with putrescence in their wake. Let us not anticipate the forms of bacteria

* Hence the alcoholic ferment is called an *organised* ferment, to distinguish it from that other ferment which yeast possesses, and which can convert cane into grape sugar independently of the *life* of the plant. According to *Pasteur*, about ninety-eight per cent. of the sugar is resolved into alcohol, carbonic acid gas, and a very small proportion of glycerine and succinic acid. The remaining two per cent. of the sugar is appropriated by the plant in the form of nutriment.—J. E. A.

called *bacilli*, some of which bring with them all the forms of malignant disease, from consumption to splenic fever and leprosy, which our physicians lay to their charge. But to return to our subject, the bottled beer, with its small percentage of yeast, gains carbonic acid gas, and the yeast plants, no longer able to live without oxygen and sugar, die; their bodies subside and form the undesirable sediment alike unpalatable and deleterious.

When we promised to visit the Western Gallery to view the process of beer aëration, we did so with the sincere purpose of bringing to the notice of our readers a machine which has of late caused some stir in the scientific world, inasmuch as through its employment all harmful sub-

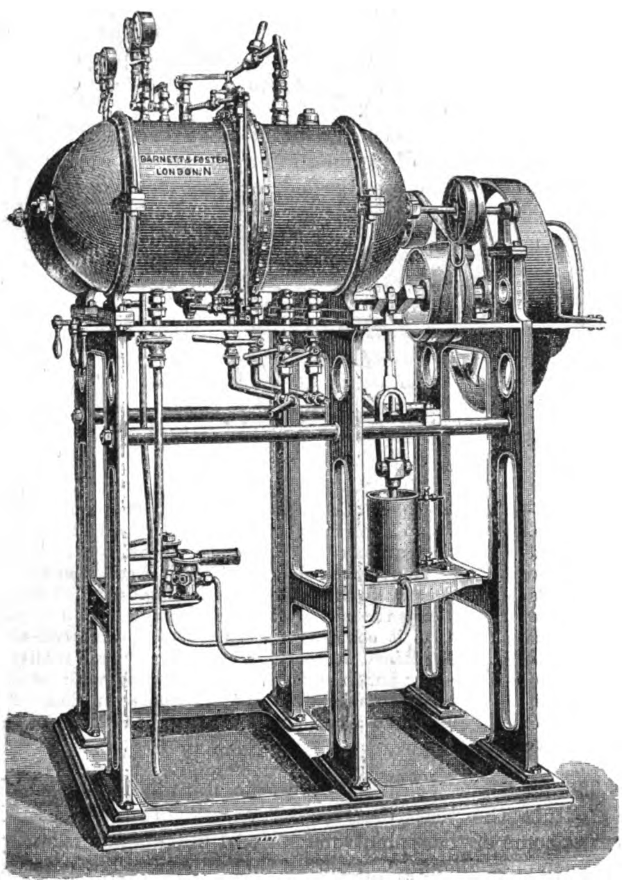


Fig. 2.—Messrs. Barnett & Foster's Beer Aërating Machine.

stances are expelled from the beer, and in consequence a beverage of great value secured.* To our mind, as to that of all rational physiologists, the human frame cannot be sustained "on bread alone;" proteids, starches, fats, salts, and *a small quantity of alcohol* are necessary for the maintenance of the body in the highest degree of vigour, especially when it is called upon to exercise manual functions. Most of the alcoholic drinks vended in the market are teeming with impurities, such as toxic oils and adulterations in the spirits and wines, and disagreeable sediments in the beers. To these causes are to be ascribed the nausea, headaches, and general debility which follow upon moderate potions.

We therefore hail with delight any improvement which is calculated to mitigate such evils, and must look upon the inventors in the light of public benefactors. To Messrs. Barnett and Foster, of the "Niagara" Works, Eagle Wharf-road, London, N., belongs the honour of having introduced an antidote in the shape of their beer aërating machine, and we cannot do better in this place, than, without any further preamble, give some account of their admirable contrivance.

The beer aërating machine of Messrs. Barnett and Foster is depicted at Fig. 2. Through its means all the air contained in any ordinary beer is first of all exhausted (pity the poor yeast plant). Fermentation thus becomes almost impossible, whilst greater scope is allowed for the incorporation of the carbonic acid gas than would otherwise, as in previously adopted methods, be the case.

There are two large cylinders, each capable of holding a barrel of beer, and these are made to work alternately, the one preparing, whilst bottling is carried on from the other (an obvious saving in time, *ergo* expenditure, the criterion of a well-devised mechanism). The air is first of all exhausted from the beer, and carbonic acid gas then forced into its place, until about 20 lbs. on the square inch of pressure is indicated. The bottling of the liquid is thence assumed.

The process of bottling the beer thus charged is so novel and excellent that it deserves some mention here. Although it does not strictly come within our area to treat of such things, we think that the effective "caging of the bird when caught" *is* of some importance, and hence offer the plan we have been made cognisant of to our readers as something of general practical interest. The great thing about it is that a sparkling liquid can be bottled securely without any frothing up, or, as it is technically called, "fobbing."

Two pipes fitted with stops-cocks are in communication with the orifice of outlet, the one to admit the beer, the other a return pipe, in connection with the upper portion of the beer-containing cylinder. The operation is conducted thus:—The cock of the return-pipe is opened, and *the pressure within the cylinder and the bottle to be filled are thus equalised.* The other cock is then turned on, and the beer flows into the bottle by gravitation, thus effectually preventing the so-called "fobbing." These processes may be watched at Messrs. Barnett & Foster's stand, No. 1,156 in the western gallery, where Messrs. Ashby & Co.'s, of Staines' ales are being bottled by Messrs. Chapman & Wells, of Tanner's-hill, Deptford.

Another great stride in the aëration of drinking liquids may also be viewed in operation at the same stand, where the "Niagara" machine of Messrs. Barnett & Foster is being exhibited. We would draw special attention to this machine, because we have of late been made painfully aware of the great danger which sometimes accrues through metallic contamination and consequent poisoning from such beverages, which are usually consumed in vast quantities in this country, and looked upon by the innocent imbibers as harmless, because they are called non-intoxicating. The vast majority of sodawater and other apparatus are provided with agitating gear, for the purpose of forcing a maximum of carbonic-acid into the liquid. It stands to reason that these appliances, especially as they are necessarily made of copper, lead, &c., must undergo a certain amount of wear and tear, and this, in instances which have recently come under our notice, has occasionally gone on to an alarming extent, so that the aërated drinks produced have not fallen far short of rank poisons.

We are therefore constrained once more to congratulate Messrs. Barnett and Foster on their invention, the "Niagara" condenser, and in giving publicity to their design, we rest assured that we are doing a service of some

* Especially for invalids, ladies nursing, and others who require a moderate amount of alcohol in a light harmless pabulum, which shall be inexpensive yet palatable.—J. E. A.

importance to our readers, by asking them to participate in the knowledge we have gained through careful inspection.

Fig. 3 is a sectional diagram of one of these "Niagara" condensors. The water impregnated with the gas is pumped into a compartment, E, formed between an inner and an outer casing; and here the liquid rises as the pumping is continued, and goes on absorbing gas which passes through the water to fill the whole body of the condensor. This goes on until the now partially-charged liquid reaches the level, D, over a perforated plate there situated, through which it rains down, like a waterfall (Niagara), into the body of the cylinder. During this descent it absorbs a further quantity of the compressed gas which fills the cylinder, and finally accumulates in the lower part of the condensor, highly charged, and ready to be conveyed by pipes to the bottling-machine. An attached water-gauge indicates the quantity of aërated water within the body of the cylinder.

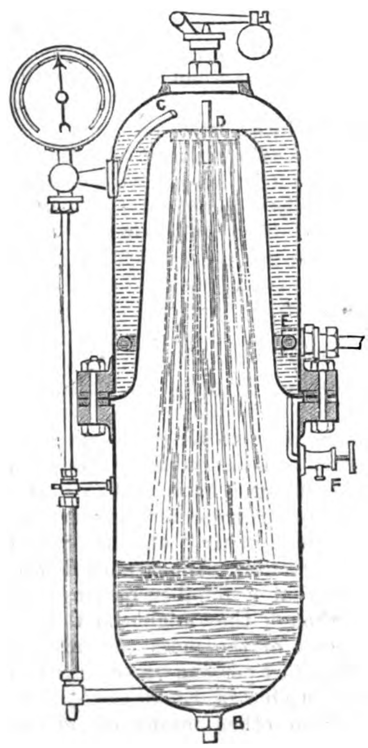


Fig. 3.—The "Niagara" Condenser, of Messrs. Barnett & Foster, for the Aëration of Soda, Potash, and other Waters.
(Diagrammatic Section.)

We can now easily understand how all metallic contamination is provided against in the slow rise of the water in the outer casing, and its downpour in a torrent through an atmosphere of compressed carbonic acid gas, by gravitation. These two simple operations dispense with agitators of all kinds, and produce the desired effect far more satisfactorily—viz., a maximum absorption, by the liquid, of carbonic acid gas.

Before leaving Messrs. Barnett and Foster's stand, we may briefly allude to the simple but effective contrivance employed in their bottling department. It will be seen that their improvements have been carried out in detail, and that the little as well as the great things have received due attention. Mr. Foster's improved bottle, called the "Intermittent," is provided with a screw-neck; the cap of stopper is somewhat larger than the neck of the bottle, and can thus be unscrewed with the utmost ease, thereby dispensing with the annoyance of corkscrews, whilst the stopper can be reinserted securely.

The plan adopted for bottling is ingenious. Fig. 4 shows the instrument employed. The stopper of the bottle is placed in a gripping holder inside the cup, under which the neck of the bottle is inserted, and, after the stopper has been drawn up by means of a winch handle, with which it is afterwards screwed into the bottle, hydraulic pressure is

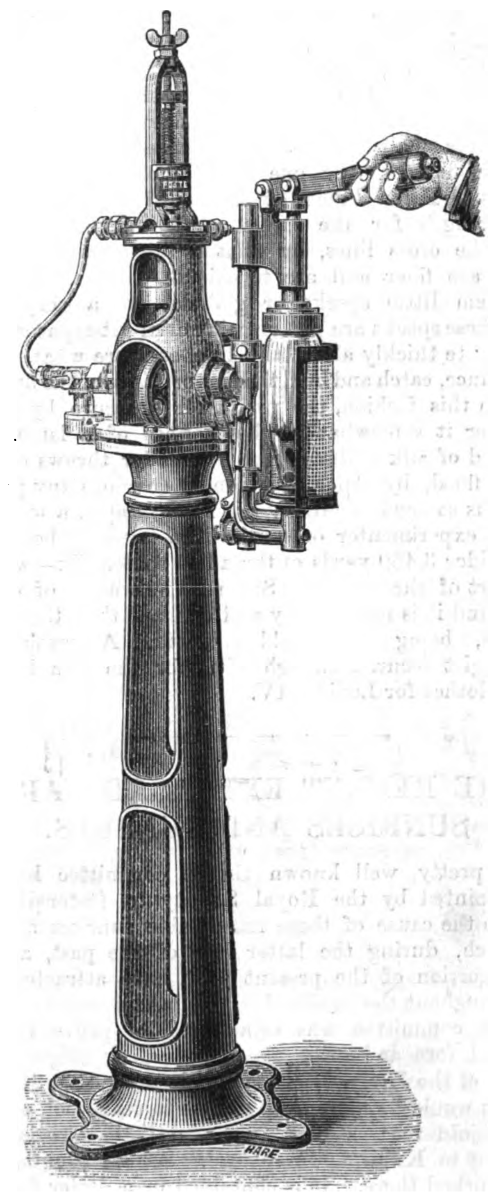


Fig. 4.—Barnett & Foster's "Excelsior" Bottling Machine for Aërated Waters, &c.

applied to an indiarubber collar, which makes a joint around the neck of the bottle and holds its firmly. The aërated liquid is then turned on to fill the bottle from a lateral orifice between the stopper and the mouth of the bottle. The stopper is then lowered and screwed on, and the bottling thus securely and neatly accomplished.

It is with regret that we are compelled to leave this interesting series of exhibits. We trust, however, that we have touched upon some of its more salient features, items which must always be of the first importance in any sanitary inquiry, and that what we have thus briefly noticed

may attract the attention of all those who are interested in the manufacture of aërated liquids in general, and the well-being of their fellow-creatures in particular.

SPIDER-LIFE WONDERS.

IN a lecture at Boston, Massachusetts, Professor Wood dealt with the phenomena of spider life. The female is larger and much fiercer than the male, who, while paying his addresses, is in constant peril, frequently losing some of his legs. In one tribe the female is 1,300 times as large as the male. The spider's thread is made up of innumerable small threads or fibres, one of these threads being estimated to be one two-millionth of a hair in thickness. Three kinds of thread are spun:—One of great strength for the radiating or spoke lines of the web. The cross lines, or what a sailor might call the ratlines, are finer and are tenacious—that is, they have upon them little specks or globules of a very sticky gum. These specks are put on with even interspaces. They are set quite thickly along the line, and are what, in the first instance, catch and hold the legs or wings of the fly. Once caught in this fashion, the prey is held secure by threads flung over it somewhat in the manner of a lasso. The third kind of silk is that which the spider throws out in a mass of flood, by which it suddenly envelops any prey of which it is somewhat afraid, as, for example, a wasp. A scientific experimenter once drew out from the body of a single spider 3,480 yards of thread or spider silk—a length little short of three miles. Silk may be woven of spider's thread, and it is more glossy and brilliant than that of the silkworm, being of a golden colour. An enthusiastic entomologist secured enough of it for the weaving of a suit of clothes for Louis XIV.

THE RECENT EXTRAORDINARY SUNRISES AND SUNSETS.

IT is pretty well known that a committee has been appointed by the Royal Society to (ostensibly) investigate the cause of those remarkable sunrises and sunsets which, during the latter half of the past, and the earlier portion of the present year, have attracted attention throughout the civilised world. As a matter of fact, this said committee was established to prove that the wonderful fore and after glows had their origin in the eruption of the Javan volcano Krakatoa, on Aug. 27, 1883; and as it would be more than useless to send such evidence as I subjoin to this nice, impartial little association, I forward it to KNOWLEDGE instead. At all events, it will not be burked there. It is contained in a letter from Mr. E. Neison, F.R.A.S., F.C.S., &c., the Director of the Government Observatory at Natal (so well known as the author of our classical work on "The Moon"), and only reached me yesterday morning:—

"In England," says Mr. Neison, "you seem all busy over discussing the extraordinary sunsets. They began in Natal *in February*, 1883, but on a less grand scale, but gradually became more marked until *June*. Then for two months nothing was noticed. In the latter end of August they became most vivid. On the 21st and 22nd they were noticeable, but not vivid. The next five days were stormy, with much rain and lightning. On the 28th and 29th the sunsets were most vivid. The 30th was rainy. August 31 and September 1, 2, 3, and 5 were fine, and the vivid redness of the sky was most remarkable, fading away as it did into green and purple in the east. Then came a week of much rain, and the sunsets vanished, not to return for nearly four months, except in a very faint degree. In February and March of this year they again became very noticeable, but did not last so long. Now (April 2) they have gone again. Now for a remarkable point. In the Transvaal they were first noticed in the beginning of September—the 2nd, I think—and were most vivid until the end of January, though here, only some 250 miles off, nothing was seen. They disappeared, as far as I can gather, from the Transvaal in January. . . . . I am inclined to believe the sunsets to have been purely meteorological. Those in February, 1883, were sufficiently marked to induce me to try a water-colour sketch on February 8. It was spoilt next day by two visitors to the observatory, who upset a glass of water over it, smashing the glass and making general havoc. I got a specimen of the fine dust which fell on the ships in the Indian Ocean some days after the eruption of *Krakatoa*. *It was absolutely free from any metallic iron or mineral containing iron decomposed by hydrochloric acid*—pumice-stone pure and simple, I believe. I tested it expressly for iron in both these forms. It was a very fine greyish-white dust."

So, here was the Government Astronomer at Natal actually drawing one of these marvellous sunsets between six and seven months before there was any eruption of Krakatoa whatever. The italics in the quotation above are Mr. Neison's, not mine. WILLIAM NOBLE.

THE DYNAMITE OUTRAGES.

By RICHARD A. PROCTOR.

CAN a grosser insult to the Irish race be imagined, or an expression of more utter hopelessness, than what "a number of Irishmen" have said in Paris of the dynamite outrages? They represent that "Ireland is fighting in this way for independence," and has "no other means of combatting England." What should we Englishmen feel if we heard that a number of Englishmen in, say, America—even though they might be the offscourings of our gaols—had said of the most cowardly villanies conceivable, perpetrated by persons who chanced to be English, that they were excusable in Englishmen, and indeed that Englishmen had no other means of maintaining their position?

It would be a bad look out for the friends of Ireland if the advance of Ireland depended on the effect produced by these outrages, which are only less contemptible than they are villanous. Once in two or three months certain cowardly hounds succeed in destroying a certain amount of property, and in maiming a few unfortunate persons, all of the poorer classes,—and they imagine, or pretend to imagine, that they are likely to scare Great Britain! Why, every week half-a-dozen persons, chiefly of the poorer classes, are killed by being run over in London alone, and more persons are maimed daily by accidents in London than these wretches succeed in hurting in a year in all England. They may be well assured that no scare is likely to be produced here. I doubt if the timidest old lady has felt a trace of alarm about these dynamite explosions.

The feeling actually produced in the mind of every Briton is akin to that felt when some loathsome crawling creature has stung one and then scuttled off into its foul haunt,—a sense of sheer disgust mixed with the feeling that when next the noxious beast makes one of its contemptible

attacks it must be more promptly pursued and trodden out of existence.

How are we to deal with these pests? I should say offer a reward ten times larger than has yet been proffered, or larger still if need be. When a number of villains have been thus caught and hanged, the informers will very likely be shot or stabbed by others of the gang, and that will be another riddance: their murderers can then be brought to justice and also eliminated. Thus a triple clearance will be effected, and humanity to some degree redeemed from the deep disgrace which the presence of these wretches involves.

For surely this is the deadliest part of the mischief the dynamite villains have done,—to have shown us that the human race is capable of these so much more contemptible villanies than we had supposed possible. The pirates and buccaneers of old seemed to give fearful evidence of what the human race is capable of doing in the way of villany; some kings, rulers, and warrior chiefs have been still more terrible examples of wickedness, having had no such incitements to evil courses; but the villanies of no preceding miscreants were quite so utterly black as those which the dynamiters have attempted. If these villains had a decent amount of courage or even of brains they would not fall so far below the level of pirates and like murderous ruffians as they do. If some among them would come in our midst, boldly bearing the dynamite they mean to explode, and would so disappear in atomic portions, we could even regard them as representatives of an insane sort of patriotism,—merely murderous madmen, not *otherwise* objectionable. Or if, being as unutterably villanous and cowardly as they are, they had a respectable allowance of brains, and laid plots likely to be in some real sense effective, we might think them worthy of a more attentive hatred. But being utter idiots, as well as craven cowards, the atrocity of their murderous designs excites a sickening sense of disgust suggestive rather of the feeling with which the more loathsome types of vermin are viewed than of the horror with which such less contemptible creatures as the cobra and rattlesnake are regarded.

As for the way in which Americans should view and treat the dynamiters in their midst, I would remark that, apart from the weakness of the extradition laws, the dynamiters are in *our* midst too, and we have not yet caught them. We cannot so much blame the Americans for not doing for us what we ourselves are unable to accomplish for ourselves. Yet I think the office of the *Irish World* might be rather peremptorily closed without unduly tyrannous exercise of power. When men openly proclaim that they are collecting money for attempts at murder* and outrage, they should be silenced (nay they cannot be left unsilenced without disgrace)—even though it be perfectly well known that the money so collected is expended on whisky, not on other explosives.

Some paper-making statistics have recently been compiled on the Continent by some busy figure lover, from which it appears that there are 3,985 paper mills on the face of the earth, in which annually 1,904 million pounds of paper are manufactured. Half of this paper is used for printing; 600 million pounds only for newspapers, the consumption of which has risen by 200 million pounds during the last ten years. As to the use of paper by individuals, an average of 11½ lb. is used by an Englishman, 10¼ lb. by an American, 8 lb. by a German, 7½ lb. by a Frenchmen, 3½ lb. by an Italian or Austrian, 1½ lb. by a Spaniard, 1 lb. by a Russian, and 2 lb. by a Mexican.—*Engineer.*

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* In the article in which the *Irish World* honoured me by a column of abuse there was enough said in the way of incitement to villany to justify respectable men in insisting that such a paper should be summarily silenced.

Editorial Gossip.

Another Bank-holiday has brought with it the customary interruption to business, the customary amount of discomfort, and the usual sequels of a disorderly and in large part drunken holiday. To see and hear the returning holiday-makers, on Monday night, on many of the chief roads into London, was to learn what are the fruits of that beneficent system by which the fifty-two holidays really available for the working people are closed (save for the public-houses) and in their place four days have been substituted for the entire year, on which the people are told to make holiday or to go without any. Yet it was said, on rather high authority, once, that the seventh day rest was made for man. Truly our legistative system in this matter is a singular caricature of Christianity.

I have seen the specimens of dress at the Health Exhibition, and noted what the dress reformers want ladies to wear instead of the customary costume. With all respect for the intentions of Mrs. King and Viscountess Harberton, I venture to tell them that they will *never* persuade ladies to follow them; and that it is well they should have no chance of success. The divided skirt, even as they recommend it, is of course something very different from the absurd costume drawn in *Punch*, *Fun*, and other such papers. But it is obtrusive enough to be unwearable by any lady who, respecting herself, desires to pass through our streets without attracting that sort of attention which is offensive and objectionable. In my own circle the divided skirt has been welcomed as lighter, warmer, more comfortable, than the underclothing before used,—and has been adopted (corsets being rejected) for nearly two years. In a quiet sort of way, again, the ladies of my household have converted many to the use of the reformed skirt. Yet no one in drawing-room or ball-room, in the street or on the tennis lawn, could ever guess that any change had been made in the manner in which these ladies dress,—unless perhaps the greater freedom and grace of their movements would suggest that they could not possibly be pinched by corsets or loaded with multitudinous skirts. Thus and thus only, I venture to predict, can any reform be usefully attempted. I myself would not as a rule offer any suggestions to the ladies of my household on such matters, knowing they have sense enough to judge for themselves. But I must confess that if anything would cause me to interfere, it would be the wearing of a form of dress such as the so-called Dress Reformers recommend,—a style which would attract the objectionable notice of every street lout (and many who think themselves respectable people have manners in the street which mark them as street louts).

I find that the idea entertained by many that the sign ♎ in my orbit maps should have been ♈, was occasioned by my own mistake in writing ♈ instead of ♎ in the description of the maps. This should be corrected by readers.

Fire Alarms in London.—According to the report of the Metropolitan Board of Works for 1883 just issued, the past year has seen an extension of the system of electric fire-alarm circuits, which are now an important feature in the machinery of the Metropolitan Fire Brigade. There are 23 fire-alarm circuits in operation, the number of points at which an alarm can be given being 151. There are 64 lines of communication by telegraph or telephone between fire-brigade stations, 17 between fire brigade and police stations, and 41 between fire-brigade stations and public or other buildings.

Reviews.

SOME BOOKS ON OUR TABLE.

The Early Days of Christianity. By F. W. FARRAR, D.D., F.R.S. Part I. (London: Cassell & Co.)—This is the first part of a reprint of Archdeacon Farrar's delightful history, and is characterised by all that charm of style and truly catholic spirit which distinguish his writings. In these days of bitter polemics it is a treat to meet with a quasi-theological work in which historical truth is transparently the first object of its author's attainment, and in which no dogmatic exigencies are allowed to operate in a distortion of the facts as they actually occurred.

Fuel and Water. A Manual for Users of Steam and Water. From the German of Franz Schwackhöfer. Edited by WALTER R. BROWNE, M.A. (London: Chas. Griffin & Company.)—Like all German work, this is thoroughly and exhaustively done, and any possessor of a furnace or steam-boiler, who wishes to utilise to the utmost the means at his disposal, can scarcely do better than procure the book whose title heads this notice. What has been, to a considerable extent, heretofore effected in stoking and boiler-feeding by rule of thumb, is reduced by Herr Schwackhöfer to practice, founded upon sound and intelligible theory; and the steam or water user is taught not only exactly what to do, but also why to do it. Highly as we are able to speak of the main portion of the work, we can afford, at least, an equal meed of praise to the introductory chapter on Heat and Combustion, by the Editor. We are not sure that we have yet met with so clear, intelligible, and, in the best sense of the word, popular an account of the mechanical theory of heat, as that contained in this really excellent essay. It might, we think, be reproduced in a separate form; greatly to the advantage of that numerous class of students of elementary physics, to whom mere algebraical formulæ convey but dim and indistinct ideas.

Science Gleanings in many Fields, Studies in Natural History. By JOHN GIBSON. (London: T. Nelson & Sons.)—This charming series of essays appears to be a reprint (with a few additions and emendations) of a series of articles contributed to the *Scotsman* newspaper. It is the very book to put into the hands of young people in whom it is proposed to implant or foster a taste for Natural History. Mr. Gibson writes from the point of view of the evolutionist, and gives the results of the very latest researches in biology. His papers on palæontology are as interesting and readable as are those on recent forms of life; albeit, his conclusions as to the age of pre-historic man in Britain may excite the wrath and indignation of the orthodox. The last glacial epoch in these latitudes may be placed approximately 210,000 years ago, and our author regards man as having been pre-glacial in these Islands!

The Young Collector's Handbook of British Birds—of Shells—of Orders of Insects—of Butterflies—of Beetles—and of Postage Stamps. By various authors. (London: W. Swan Sonnenschein & Co.)—Here is a series of the most marvellous pennyworths that we have ever seen. They are written, one and all, by officials of the British Museum, are well illustrated, and contain just the kind and amount of information required by the beginner, conveyed in a pleasing and easy style. It is not easy to understand how works like these, written by men of eminence in their various departments, can be made a commercial success at the almost nominal price at which they are published. Certainly nothing but the enormous circulation which they well deserve can suffice to render them so.

A Handbook to the Fernery and Aquarium. By J. H. MARTIN and JAS. WESTON. (London: T. Fisher Unwin.)—These are really two separate handbooks, printed on different paper and in different type, but bound in one cover. The author of the Fern moiety of the work makes some funny statements (for example, about the prevalence of the *Osmunda regalis* on p. 25), but his notice of the histology of ferns is not without interest. The second half of the volume is more practical, and will be found useful by those about to start aquaria.

The Weather of 1883, *as Observed in the Neighbourhood of London.* By EDWARD MAWLEY, F.R.Met.S. (London: Ed. Stanford.)—This is a tolerably exhaustive account of the atmospheric conditions prevailing at Addiscombe during the past year. For ourselves, when we glance over table after table of temperature, pressure, humidity, and wind, we are tempted to ask *cui bono?* Perhaps, though, some day a meteorological Newton will arise who will co-ordinate the stupendous existing mass of undigested records and turn them to some account.

The English Language: Its Sources, Growth, and Literature. By THOS. PAGE. (London: Moffatt & Page.)—Here is an early result of the New Code issued by the Education Department. If we admit that it is necessary for the tillers of the soil, and the labouring classes generally, to understand the structure of the language they employ, and to spend their school-time in philological study, then will Mr. Page's book answer its purpose, and, we are bound to say, answer it well. Whether, though, an intimate knowledge of Greek affixes and Saxon suffixes will benefit the class to whom it is proposed to impart it, may form a legitimate subject for discussion. A person may be able to read the time from a chronometer to a fraction of a second, who does not even know what its "escapement" is.

Aids to Botany. By ARMAND SEMPLE. (London: Baillière, Tindall, & Co. 1883.)—Such works as this are the direct and immediate product of the detestable system of cram developed by the examinations of the Science and Art Department. The competitor who wishes to pass in a botanical examination as at present conducted, has nothing to do but to read up Mr. Semple's treatise, to do so with flying colours. As for identifying a buttercup or a foxglove by its aid, though, the incipient student might as well try to master the theorems of pneumatics by watching the weathercock on a church steeple.

Great Industries of Great Britain. Part I. (London: Cassell & Co.).—This is the first part of a serial publication, in which it is proposed to describe the various industries which have made England the foremost manufacturing nation in the world. It contains a chapter on Iron and Steel, by Mr. W. D. Scott Moncrieff; another on Cotton, by Mr. Bremner; one on Shipbuilding; one on Hemp, Flax, and Jute; besides a biography of Sir Thos. Bazley, by Smiles; and essays on Industrial Legislation, and Health and Disease in Industrial Occupations, by Mr. Henderson and Dr. Gordon Hogg respectively. The names of the authors afford sufficient guarantee of the value of their contributions. The illustrations leave nothing to be desired.

The Electrician's Directory, with Handbook for 1884 (London: *The Electrician* Office.)—What it professes to be—a thoroughly complete list of every one engaged in the pursuit of Electrical Science and Art, Theoretical Electricians, Telegraph Engineers, and makers of Electrical Apparatus of every sort and description. In addition to this, there are the fullest details as to the various Electric Light Companies, of the British Cable Fleet, of the Chief Officers of all the Local Authorities in England and Wales, of the

various Railways and their Officials; in fact, the book contains everything that justifies its title.

Tait's Improved Arithmometer. (London: Chas. and Edwin Layton.).—This is a description of an improved and simplified form of the calculating machine originally devised by M. de Colmar, but which has been modified by Mr. Tait. Either addition, subtraction, multiplication, or division may be performed with the arithmometer with unerring accuracy; but the price of the machine is necessarily comparatively high, fifty guineas being asked for one giving sixteen figures in the product. It is, however, easy to understand that in cases in which numerous intricate and repeated calculations are involved such an outlay might prove a really good investment.

THE FACE OF THE SKY.

From June 6 to June 20.

By F.R.A.S.

The usual daily examination of the Sun for spots, &c., will be made with the telescope. The night sky is delineated in Map VI. of "The Stars in their Seasons," but twilight now persists all night long. Mercury is still a morning star, attaining his greatest elongation, west of the Sun (23° 3′), at 3 a.m. on the 13th. About this date will be the most favourable time for catching him, somewhat to the north of east, above the horizon, before sunrise. Venus is still, by far, the most conspicuous object in the sky; her crescent continues to diminish as her diameter increases, and she forms at once a beautiful object for the observer with a telescope, and a severe test of the excellence of his instrument. She sets somewhat sooner now, and should therefore be looked at as soon as she is visible—supposing, of course, that the student possesses no means of picking her up in daylight. She is, though, quite visible to the naked eye on a sufficiently clear day, when she crosses the meridian in brilliant sunshine, and, about the time these notes begin, may be picked up in the following way:—She souths on June 7, about a quarter to three o'clock in the afternoon, and about two minutes sooner on each succeeding day. Let the reader then cut a right-angled triangle out of cardboard, whereof we will call the apex A, the left-hand corner B, and the right-hand corner C. The base, B C, must measure 3 in. long, the upright side, A C, ⅞ in., and the slant side, A B, 6·5 in. in length. Then at or about the instant of Venus's meridian passage the base B C must be placed truly horizontal and pointing due north and south, with the end B towards the north and C towards the south. The observer must now place his eye at B, and looking along B A he will, after a little attention, pick up the planet shining in the sunlit sky with a lustre which will astonish him. It must be noted that her meridian altitude, though, decreases slightly daily. At night she casts quite a perceptible shadow of any opaque object interposed in front of a sheet of white paper. The rest of the planets are now beyond the ken of the observer, though possibly Uranus may be picked up, when the twilight has deepened sufficiently, by the aid of the Zodiacal Map on p. 165. No occultations of stars by the moon occur during the period covered by these notes. The moon is in Libra to-day (the 6th), but at six o'clock to-morrow morning she will enter the narrow strip of Scorpio, which forms the northernmost part of that constellation. This she takes twelve hours or so to cross, emerging in Ophiuchus at six in the evening. In Ophiuchus she remains until 3 p.m. on the 9th, when she enters Sagittarius. She takes until 5 a.m. on the 12th to traverse this, and then passes into Capricornus, where she remains until 11 p.m., crossing then into Aquarius. Her passage across Aquarius occupies until 11 p.m. on the 15th, at which hour she crosses the boundary into Pisces, which great constellation she does not quit until 10 p.m. on the 18th to enter Aries. She is in Aries until noon on the 20th, when she enters Taurus. There we leave her.

French Subterranean Telegraphy.—The first proposal for laying down an underground telegraph network for France was made in 1880, by the Minister of War, who at that time asked for a credit of £320,000 for that purpose. The aggregate length of the network is to be 3,435 miles. At present the eastern lines, which are most urgent, are finished, and a good deal of progress has been made with others in the north and north-east. Nearly 2,500 miles are now completed, and it is hoped that by 1886 the whole network will be finished.

"Let Knowledge grow from more to more."—Alfred Tennyson.

Only a small proportion of Letters received can possibly be inserted. Correspondents must not be offended, therefore, should their letters not appear.

All Editorial communications should be addressed to the Editor of Knowledge; *all Business communications to the* Publishers, *at the Office, 74, Great Queen-street, W.C.* If this is not attended to delays arise for which the Editor is not responsible.

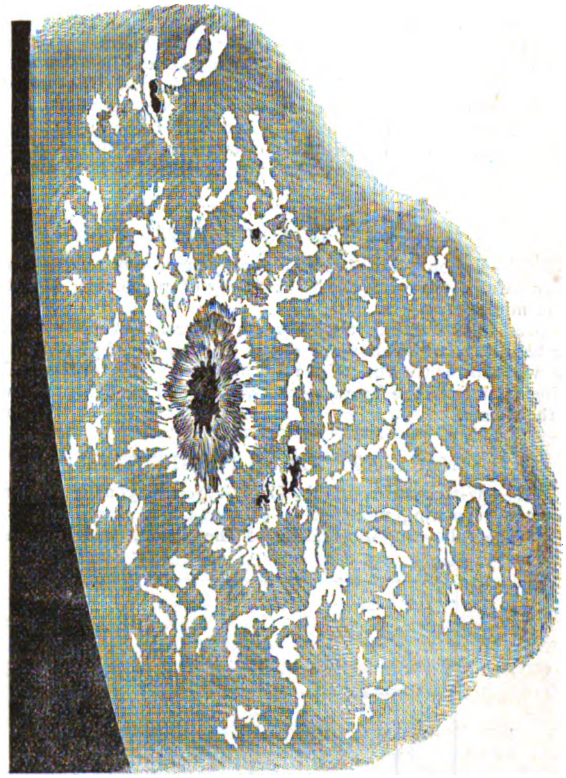
All Remittances, Cheques, and Post Office Orders should be made payable to Messrs. Wyman & Sons.

The Editor is not responsible for the opinions of correspondents.

No communications are answered by post, even though stamped and directed envelope be enclosed.

SUN-SPOT.

[1284]—As the sun's disc is at present showing some large and interesting spots, it may please the readers of Knowledge to see the fine wreaths of faculæ that surrounded the large spot that has just disappeared round the rim. I have enclosed a careful copy



May 20, 8.30 a.m. 3½ in. Wray, power 120.
Drawn from the screen.

from my drawings. The spot as seen on the 15th was really a striking object, with its many tongues running into the dark umbra, which had on the left part of it a thin veil of faculæ that stretched across it, giving the dark part the effect of greater depth.

E. L. Brown.

"TWINKLE, TWINKLE."

[1285]—I trust to the proverbial justice and courtesy with which your name is inseparably connected [Oh, sir.—R.P.] to find space for the following:—1. If G. G. H. will take the trouble to refer to "Rhymes for the Nursery" (not "Original Poems for Infant Minds"), by Ann and Jane Taylor (which he can see at the British Museum, or, failing this, procure for a comparatively

trifling sum, of T. Nelson & Sons, Paternoster-row, London, E.C.), and turn to page sixteen of the same, he will there find the poem, the authority of which he disputes. 2. Nothing bearing the *faintest* resemblance to the poem in question appears in the "Ryght Pithy Pleasaunt and Merie Comedie intytuled Gammer Gurton's Neele." (See Dodsley's "A Select Collection of Old Plays," Vol. II.) Dr. Drury, as a "Dr.," should have known better. (*Vide* KNOWLEDGE, No. 132.)

The question is, why did not G. G. H. consult the play itself, rather than take the authority of Dr. Drury? And (3) G. G. H. refers me to "Notes and Queries," for "the original Latin version." I said nothing about this. Why did he not rather turn to page 45, of the same volume, which he erroneously quotes from (for he writes* "*Mira, Mira,*" for "*Mica, Mica*") where the following 'note' (I quote a part of it) is given:—"Twinkle, Twinkle, Little Star! Done into Latin by Spirits. . . . Several Latin quotations were given; among the rest the following translation of a stanza from *Jane* [and Ann, of course] *Taylor's* little nursery poem, beginning "Twinkle, twinkle, little star," &c. And now for *hic finis fandis*, so far as I am concerned, at any rate, for I have neither the time nor the inclination to haggle over the authorship of a poem never before disputed. I beg to thank the Editor of KNOWLEDGE for his forbearance in allowing the discussion to go thus far, to, I should honestly think, the satisfaction of all parties. *Valete ac plaudite? Heu!* J. W. HOWELL.

DUALITY OF THE BRAIN.

[1826]—I had been dreaming one morning of a scene in a drawing-room in which the figure of one of my friends stood clearly out, and awaking in the daylight, I saw distinctly, at the same moment, the figure of my friend as with one eye and the top of the bed with the other! I awoke in a state of considerable excitement, thinking what a strange thing that one eye should be asleep and the other awake at the same time! Will not the fact that one eyelid happened to open before the other, and your correspondent, Leonard H. Rudd's suggestion (KNOWLEDGE, p. 250) respecting the duality of the brain, help to explain this circumstance, whilst the circumstance itself tends to confirm the view expressed in the foot-note.

R.

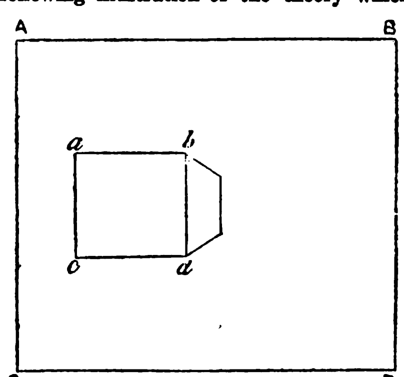
PRESERVING INSECTS.

[1287]—I should be glad if one of your correspondents would kindly inform me what is the best way of preserving insects for *future* microscopic use.

If pressed and dried, they become so brittle that they will not bear handling, while they are out of all shape. If placed in fluid, how will this leave the wings of moths and butterflies? I ask, having friends in the Tropics who would send me things if I would tell them how. S. BOYD.

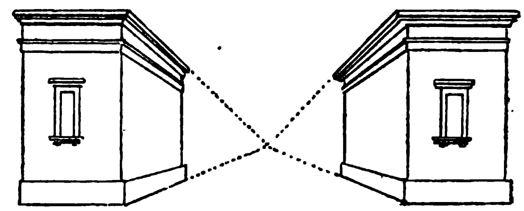
IMAGINARY FALSE PERSPECTIVE.

[1288]—With reference to the difficulty felt by your correspondent R. Jones, regarding perspective (KNOWLEDGE, May 16, p. 358), I submit the following illustration of the theory which is usually

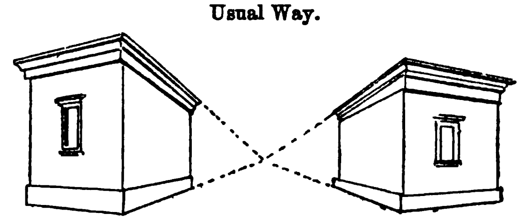


acted on. Imagine a plate of glass A B C D to be the plane of a picture. Imagine a log of wood with a square end, to be placed behind it, and at right angles to it, and with the square end butting against the glass as if about to crush it. Then I say that if the outline of this square end A B C D be traced with a diamond on the glass, it will equally be square whether the log of wood be at one side or in the middle of the glass; the sides of the square end will not converge as your correspondent thinks, though the log of wood be so much on one side of the centre that the length of the log may be seen in perspective behind the middle of the glass.

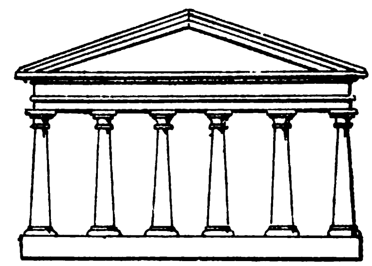
AN OLD DRAUGHTSMAN.



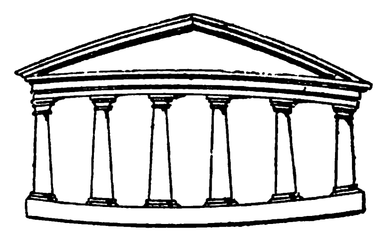
Usual Way.



Unusual Way.



Usual Way.



Unusual Way. N.B.—Something like this might be called for, if the plane of the picture were a curved surface, as in a Panorama.

[Very neat. Here is a question for consideration. In the usual way of picturing the axes in co-ordinate geometry of these dimensions the axes of x and y are at right angles. Is this right?—R. P.]

A STRANGE COINCIDENCE.

[1289]—An American lady—Mrs. B.—on seeing the article headed thus in KNOWLEDGE, of May 16 (No. 1,237), told me of a singular incident which happened to herself a few years ago.

Her mother, living in New Jersey, had suddenly become an invalid from a slight stroke of paralysis. She and her sister were called to the spot, and when examining the contents of some old boxes in a lumber-room, they came upon a packet of old letters, amongst which was one which had been written to their deceased father thirty years before, by his old friend M. J. S., whose home was in the West, a long distance off. The two younger ladies took the letters down to their mother in order to ask whether she wished them destroyed. Just as their mother was occupied in reading this particular one, a knock was heard at the house door, and M. J. S., the very man who had written this letter, was ushered into the appartment, a totally unexpected visitor. COSMOPOLITAN.

COINCIDENCE WITH CAWS.

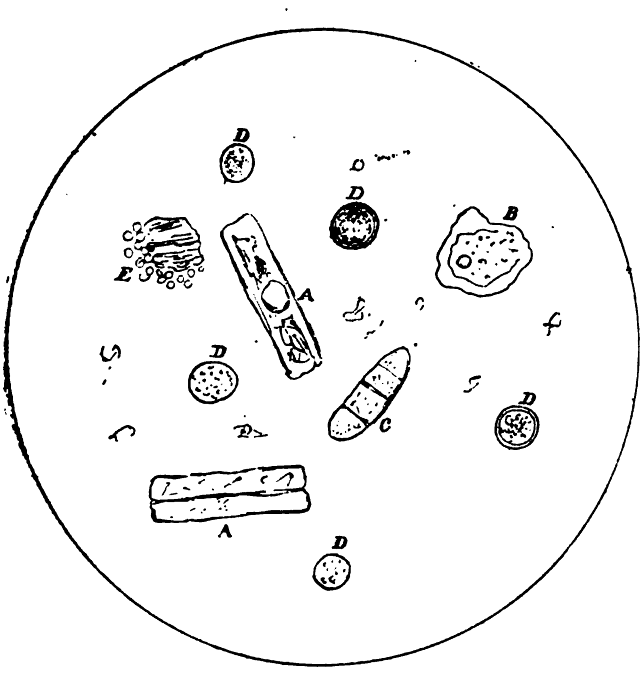
[1290]—On Sunday afternoon, March 13, 1881, about 3.30, p.m., I was taking a quiet stroll on the lawn in front of my house, having my after-dinner smoke, when suddenly two rooks appeared upon the scene. They circled about over my head, within easy gunshot,

* He wrote the words rightly; they were mis-printed.—ED.

at the same time uttering all the while their discordant "caws." So persistent were they that I involuntary looked up, exclaiming, "What ill omen do you black rascals bode." Now, it was not the fact of the rooks being where they were which arrested my attention, as there is a large rookery close by, and the birds daily visit these grounds. It was their persistent "Caw, caw!" and the birds themselves wheeling in circles not twenty yards above my head. At that moment the late Emperor Alexander was assassinated in the streets of St. Petersburg. YOHAMITA.

ORGANISMS IN HAILSTONES.

[1291]—I have seen occasionally some reference made to the fact that snowflakes and hailstones when examined under the microscope, were found to contain organised forms, but I doubt if it is known that they are met with to such an extent as I have lately noticed. I have on two or three occasions during the last twelvemonth collected a few hailstones in a conical glass, so that anything contained in them subsided to the bottom as they melted, and have always found organised remains, but I never had any idea of the quantity of these till the hailstorm of the 5th inst., and I now send drawing of the contents of a single hailstone (about ¼ in. diam.) which I placed with every precaution as to cleanliness, between the glasses of a live box. The figures are carefully drawn in proportion to each other on the understanding that the bodies marked D were of about three or four times the diameter of a human red blood corpuscle. The only respect in which the drawing does not truly represent the exact contents of the hailstone is that there were, at least, forty of the spheres D, which appear to me very like ova of some of the oviparous Rotiferæ. The amœba and one diatom were in active movement. The spore (?), C, I would call the attention of some of your microscopists to, and should be glad to hear if they are acquainted with it, as it is one of several of the same kind which I discovered among the fibres of the heart of animals dead from cattle disease in India, in 1870, and described in the monthly *Microscopical Journal* for December of that year, p. 312. B. M., F.R.C.S.



CONTENTS OF SINGLE HAILSTONE.

A. Diatoms—one of them in active motion.
B. Living amœba.
C. Spore (probably of fungus), olive brown.
D. Pale yellowish bodies, like ova in size, about three to four times diameter of human red blood corpuscle.
E. Dark brown mass, with small bright spherules

THE ORIGIN OF THE WORD "CAUCUS."

[1292]—Mr. Bulley's query as to the origin of the above is thus explained by "The Oracle" of Feb. 24, 1883:—"Caucus is a word of American origin. The oldest use of the word is often supposed to be in the following passage in John Adams's diary, dated Boston, February, 1763:—'This day learnt that the Caucus Club meets at certain times in the garret of Tom Dawes, the adjutant of the Boston regiment. He has a large house, and he has a moveable (*sic*, J. W. H.) partition in his garret, which he takes down, and the whole club meets in one room. . . . . There they choose a moderator, who puts questions to the vote regularly, and select men, overseers, collectors, wardens, fire-wards, and representatives are regularly chosen by the town. They send committees to wait upon the merchants' club, and to propose and join in the choice of men and measures. Gordon, in his "History of the American Revolution," traces this practice to even an earlier date. It has been conjectured that caucus is a corruption of calkers. Very possibly the Caucus Club which met in Tom Dawes's garret was originally a mechanics' club, called from the leading trade in it the Calkers' Club, which name, with a variation, is still retained after it passed into the hands of politicians. Mr. J. H. Trumbull derives the term from an aboriginal word, meaning to speak, encourage, instigate. The singular of the Indian noun is said to have been kaw-kaw-was, plural kaw-wus-sough, "councillors, which the Virginians changed into cockarouse, designating a pretty chieftain; and it has been supposed that thence come caucusers, and caucus." It would, I venture to think, be interesting to know what Dr. J. A. H. Murray, the President of the Philological Society, and editor of the new English Dictionary, has to say about the etymology of the word under consideration.

J. W. HOWELL.

THE MYSTERY OF GRAVITY.

[1293]—If Mr. Aynsley [Letter 1239] will reconsider the question, I think he will be led to change his opinion regarding the source of the energy derived from falling water.

If we consider the case of a cork constrained to remain beneath the surface of a liquid, it is obvious that in transferring it to that position, *the water which it displaces* has been raised through a certain distance *against the force of gravity*, and, therefore, possesses potential energy. If now the cork be liberated, the water is free to fall, and its potential energy becomes kinetic, part of which is consumed in raising the cork to the surface again. Now, the source of energy here can no more be traced to gravity than can the source of energy in a catapult be traced to the elastic of which it is made. When the catapult is drawn, energy is stored up by doing work against the elastic, which becomes kinetic when the missile is released. The elastic does work it is true, but only by virtue of work done upon it previously. So, in the case of the cork, gravity does work in drawing down the water, and so raising the cork; but only by virtue of work done against it previously in raising that water, which work must have been derived from some other source.

Now, to turn to the main point, which is much more complicated. In raising water, the sun does two kinds of work. Firstly, *internal work*, which is expended in raising the temperature and changing the state of the liquid. But secondly it does *external work* against gravity, by lifting that portion of the atmosphere which is displaced consequent on the increase in the volume of the water on passing from the liquid to the vapour state. Thus we may suppose that *for an instant* this raised portion of the atmosphere has potential energy, which is immediately converted into kinetic energy as it falls, and part of this is consumed in raising the aqueous vapour. Now, if this vapour, from any cause whatever, should at any time return to the liquid state, it will again reach the level of the sea in virtue of that energy which was stored up when it was raised, and may be made to do work in its fall, which work is clearly done indirectly by the sun, and has not the force of gravity as its ultimate source.

Mr. Aynsley says:—"Nor is there any connection or commensurability between the energy of falling water on the earth and that energy of the sun which had previously vapourised it." The cause is not far to seek. The energy which is consumed in actually raising the aqueous vapour bears no comparison to that consumed in changing its state, and the greater part, if not all, of this *internal work* is again converted into heat upon condensation. C. P.

THE PATENT ACT.

[1294]—I am pleased to see that you are treating of such a widely interesting subject in KNOWLEDGE as the "New Patent Act," and as it is of practical interest to me, I would like to ask a question or two on the subject, which, perhaps, the writer of the article will kindly answer. The following are the questions:—

1. After a *Provisional Specification* has been sent in with a full set of drawings, is it necessary, in case there be no alteration in them, to send in a duplicate of the drawings with the *Complete Specification?* Will not the one set of drawings which has

been sent with the Provisional be sufficient for the Complete Specification?

2. In the case of taking out two patents for inventions which are somewhat allied, and which might be used in conjunction with each other, will the sketch required for the *Illustrated Journal* be sufficient if it illustrate the two inventions combined, provided the two complete specifications are sent to the Patent Office together?

FRANK WEST.

PROPERTY OF NUMBERS.

[1295]—The following, which is due, I believe, to Bishop Cotterill, is a neat problem, and may exercise the ingenuity of your readers:—

"Given the remainder when a number of three digits is divided respectively by 3, 5, 7, 11, to find the number."

It would also be interesting to trace the modifications of the solution if we introduce another divisor, as for instance 13.

C. W. BOURNE.

SQUINTING.

[1296]—The remarks lately made by Mr. W. Cave Thomas on the subject of vision by means of an instrument called the diascope, intended to present to the right eye the view ordinarily falling to the left and *vice versa*, encourages me to ask space for one or two notes on a kindred subject—namely, squinting. This power, which can be easily acquired, but must be cautiously practised to avoid injury to the eyes, is, I think, of some psychological interest.

I note then, first, that the difference of colour tint in the fields of the two eyes, referred to by Mr. Thomas, though sometimes very marked, is not permanent. The natural delicacy of the organ would lead us to expect some difference. It could hardly be otherwise, seeing that the eye is so readily dazed by lights, and has its vision after staring at a colour so soon tinted with the complementary. Is this last, by the way, anything more than an application of the law of relativity, which renders us deaf to a continuous and continued sound, and makes the one hand, coming from cold water, feel lukewarm water hot, while the other from hot water feels it cold?

It is to be noted, also, that the focus of the eyes, usually coinciding with the point whither their vision converges, can readily, with a little practice, be made adjustable to any object towards which we squint, however near the point of convergence is to the face, and that the two eyes may be focused differently.

If a stereoscopic slide is cut in two, and the parts interchanged, the apparent relief of the objects, as is well known, can be seen by squinting quite as readily as with the stereoscope. Four pictures in all are seen by the two eyes, and it is only necessary to make the middle two overlap and coincide. This fact shows the correctness of the above remarks, and likewise brings out one or two interesting facts. Why does the slide appear smaller when so treated? This effect is marked and even startling. It is, I think, an optical delusion, depending on the fact that the eyes, converging to a point nearer the face, unconsciously conclude that the object is there, and at that distance expect it, of course, to present a larger appearance. Hence the convergence of the eyes must have more effect in determining the distance of an object than the adjustment of the focus has. An object more and more squinted at diminishes in size if retained in focus the while.

Even if the slide is not cut, the eye still interprets the appearances as indicating relief, and does not usually make the concaves convex, and *vice versâ*, though one would expect this. Accordingly, it must be inferred that almost any slight difference in the pictures of the two eyes will, within limits, be taken as an evidence of solidity, *provided that the nature of the object under inspection is such as to lead us to expect it to be in relief.* The two stereoscopic photos, however, do not combine *equally* well when the photo on the right is seen by left eye and *vice-versa*. When the relief is very great, the eye becomes confused and at a loss. This will be seen by examining the slide with the stereoscope after cutting it and arranging it as above. If the relief is very great the eye then becomes confused and at a loss. A slight difference in two pictures does not prevent coincidence in the view; it is well known that under the stereoscope a circle may be made to combine with an ellipse of small eccentricity and equivalent size. In the case of very simple figures, as a sphere, an interchange of concavity and convexity may occur.

It is possible, with a number of similar objects possessing cubical dimensions (say a row of hat-pegs), to make the picture of one peg in the right eye overlap another in the left. Thereupon the solidity of the peg, is exaggerated—they appear longer. A peg to the left hand is brought by the right eye more to the front, and the left eye brings one in the opposite direction to meet it. These being seen more sideways than a front peg would be are less foreshortened; hence, when they come to the front they appear longer.

I have never been able to effect a super-imposition by converging the eyes to a point behind the row of pegs. I believe it, nevertheless, to be practicable. For if we hold, say, three equal pencils at equal distances in the hands before the face, and gaze between them at a point in the opposite wall, we shall at length, by varying their distance, cause their duplicates to coincide. I have never thus succeeded in securing a focus. Has any reader effected it?

P. J. BEVERIDGE.

LETTERS RECEIVED.

T. COMMON. You are, of course, right; your meaning was sufficiently clear.—J. C. E. Thanks. Yet what a capital name Zetetic is for a medicine, such a one as "searcheth" the reins and the liver, for example.—A. B. C. The idea is rather of elliptical motion in a plane at right angles to the direction of transmission. But the whole subject is crowded with difficulties. The properties of the æther of space are contradictory—infinite elasticity and infinite rigidity.—C. P. Letter in hand.—EXCELSIOR. Many thanks. I had overlooked that mistake, which I will at once correct. There is, however, no reason for putting ♈ instead of ♎ as the spring sign in a heliocentric chart. Your later letter received. The misunderstanding is removed, is it not?—J. T. ROUTLEDGE. I am away from my books, and unable to point to a passage. I fancy every believer in evolution — which is nearly the same thing as saying every man of science—believes that at some time in the history of the earth there was spontaneous generation; but science knows nothing experimentally of any kind of life other than that which has sprung from former life.—E. L. G. Your interesting letter on Noah's Rainbow in hand; may be delayed, though, on account of length.—H. SNAIBURN. That would hardly suffice to explain why satellite looks sometimes as dark as its own shadow.—R. J. S. Neither am I a "betting man." Multiplying is right. Each event of one set may be combined with each event of the other set, giving product of the two numbers as total number of pairs of events.—E. Your interesting letter would unfornately be misunderstood by many. (I know this from experience). As you say, a deaf-mute would not give satisfactory evidence. Nor would a man brought up with full scientific knowledge but without religious ideas. Perhaps the strongest evidence is given by men of science who have *not* been brought up without religious ideas. But we must leave the subject alone, or many will misunderstand.—A. J. W. Your telescope seems a fair one. Nebulæ vary in appearance according to the atmospheric conditions. Pictures such as we draw of Jupiter say nothing as to power, because they can be held at different distances. Have you tried looking with one eye through telescope at Jupiter, with the other—unaided—at the moon, when she is pretty near the planet?—J. BINDON CARTER. Ah, well; for the sake of logic we might as well keep to the assumptions. Assuming otherwise you arrive at another inference—(alliteration artful aid affordeth).—JOS. CLAYTON. In the earlier numbers of KNOWLEDGE, papers on "Nights with a 3-inch Telescope—the construction of an astronomical telescope was described. Have not back numbers with me.—W. LEWIS. Perhaps Mr. Allen will write about that matter. You say "we have taken a lively interest and am still very sceptical,"—joker of jokes might ask if you are something beside yourself: every editor is, of course,—officially.—J. M. BROWN. Have sent your letter to publishers; if there is delay the fault is not ours. *You* evidently consider we are something beside ourselves.—O. B. That sort of rainbow ring round the moon is not very unusual. Glad you find so large a proportion of our contents interesting. We agree with you it would be a pity to pad with trash and charge fourpence. Know no reason for supposing our solar system more ancient than any other parts of the universe. Quite the contrary, I think.

E. W. PREVOST. There is no "advantageous form of generator" to supply two or three incandescent lamps, unless for occasional experiments, when large-sized Grove Bunsen or bichromate cells answer best. The number of cells needed depends upon the electromotive force required. Almost an equal number of "storage batteries" would be required, their E M F being very little higher than that of a good primary cell. Consequently they are not to be recommended.—H. D. W. The object in view in writing the earlier portion of the article referred to was to demonstrate the inefficiency of batteries for electric lighting. The fall of free potential, consequent on the insertion of low resistances, was neglected to simplify matters. A recognition of the fact would only have intensified the argument set forth. The difference in the case of the incandescent lamp is immaterial, although it is admitted that it would have been better to employ larger cells (of lower resistance), in order to render the depreciation of the free potential negligible. The question shall, however, be further dealt with.—T. H. CHRISTY.

The local features which determine a lightning discharge are so various, and the subject has received such a small amount of attention, that it would be unsafe to assign a cause for the varied effect upon the wire forwarded. We have seen many remarkable effects, each doubtless the consequence of a specific cause. Nevertheless, many of these causes are undiscovered.

Our Mathematical Column.

EASY LESSONS IN CO-ORDINATE GEOMETRY.

By Richard A. Proctor.

(*Continued from p.* 381.)

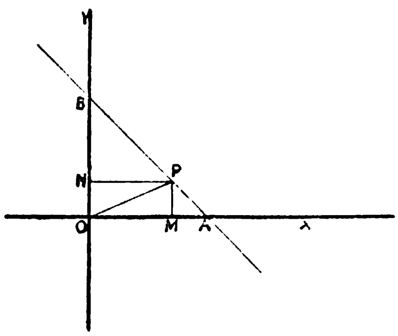
We proceed to discuss some other convenient forms of the equation to a straight line.

32. *Def.* The two lines intercepted between the points in which a straight line meets the axes of x and y, and the origin are called respectively, the *intercepts* on the axes of x and y and are considered positive or negative according as they are measured from O in the positive or negative direction along either axis.

Thus in the figure to Art. 31, O A is the intercept of the line A B on the axis of x and is negative, O C is the intercept of A B on the axis of y and is positive.

33. *To find the equation to a straight line in terms of the intercepts on the axes.*

Let A B be a straight line meeting O X and O Y in the points A and B respectively; and suppose O A $=a$ and O B $=b$. From any point P in A B draw the ordinates P M and P N, and join O P. Let the co-ordinates of P be x, y.



Then $$2\triangle APO + 2\triangle BPO = 2\triangle BOA$$
that is $$bx + ay = ab$$
the required equation.

Dividing by $a\,b$ the equation assumes the convenient form
$$\frac{x}{a} + \frac{y}{b} = 1. \qquad \text{(ii.)}$$
in which it is generally employed.

34. Conversely any equation of the form (ii.) is the equation to a straight line. For multiplying by $\frac{1}{2}\,a\,b$ it becomes
$$\tfrac{1}{2}\,bx + \tfrac{1}{2}\,ay = \tfrac{1}{2}\,ab$$
which shews that if we take O A $=a$ and O B $=b$, and join any point P on the locus of the equation with the points B, O, and A as in the figure, the sum of the triangles O P B and O P A is equal to the triangle included between the lines O A, O B, and A B, which it would obviously not be if P did not fall on A B. Hence the locus of the equation is the straight line A B.

35. To show that the general equation of the first degree
$$Ax + By + C = 0 \qquad \text{(i.)}$$
represents a straight line.

Dividing by C (i.) becomes
$$\frac{Ax}{C} + \frac{By}{C} + 1 = 0$$
which may be written
$$\frac{x}{-\frac{C}{A}} + \frac{y}{-\frac{C}{B}} = 1;$$
hence by 34 it is the equation to a straight line whose intercepts on the axes of x and y are $-\frac{C}{A}$ and $\frac{C}{B}$ respectively.

36. If A $=0$, the intercept on the axis of x becomes infinite, and the straight line is therefore parallel to the axis of x. This is also obvious from (i.) which in that case becomes
$$By + C = 0, \text{ that is } y = -\frac{C}{B}$$
and this by Art. 30 represents a straight line parallel to the axis of x, and whose intercept on the axis of $y = -\frac{C}{B}$.

37. Similarly, if B $=0$ (i.) represents a straight line parallel to the axis of y, and whose intercept on the axis of $x = -\frac{C}{A}$.

38. If C $=0$ the equation (i.) becomes
$$Ax + By = 0 \qquad (2)$$
that is
$$\frac{y}{x} = -\frac{A}{B}$$
which by Art. 30 represents a straight line through the origin inclined to the axis of x at an angle whose tangent $= -\frac{A}{B}$.

39. We can also show that the last-named angle is the angle which the straight line represented by (i.) makes with the axis of x whether C vanish or not. For if a is the angle that any straight line makes with O X estimated in the manner described in 31, then
$$\tan a = -\frac{\frac{C}{B}}{\frac{C}{A}} \text{ in the case of the line represented by (i.).}$$

Lastly,* if C $=0$, and also A or B $=0$, the equation (i.) becomes $y=0$, or $x=0$ respectively; that is the equation represents O X or O Y respectively.

(*To be continued.*)

EASY RIDERS ON EUCLID'S FIRST BOOK,

With Suggestions.

Prop. 32 (*continued*).

109. Show that the interior angle of a regular figure of n sides exceeds a right-angle by $\frac{n-4}{n}$ths of a right-angle.

110. On the sides of any triangle A B C equilateral triangles B C D, C A E, A B F are described, all external to A B C. Show that the lines A D, B E, C F are all equal.

Establish the equality of the triangles A C D, B C E, *&c.*

111. In the triangle A B C, the lines B D, C E are drawn perpendicular to A C, A B, respectively. Show that the angle A B D is equal to angle A C E.

112. With the same construction show that the angle E F D exceeds the angle E A D by twice the angle A B D.

113. Show, also, that the angle D F C is equal to the angle B A C.

114. Show, also, that the angles B F C, B A C are together equal to two right angles.

115. If the straight lines bisecting the angles at the base of an isosceles triangle be produced to meet, they will contain an angle equal to an exterior angle of the triangle.

116. Show that every right angle may be divided into two isosceles triangles.

117. If A B C be a straight line, bisected in B, and any line B D equal to A B, or B C be drawn from B, show that A D C is a right angled triangle.

118. Trisect a right angle.

119. Construct an isosceles triangle, having the vertical angle equal to four times the angle at the base.

120. One of the acute angles of a right angled triangle is three times as great as the other; trisect the smaller of these.

121. On a given straight line A B an equilateral triangle A C B is described, the angles A and B are bisected by lines meeting in D, and lines D E, D F are drawn parallel to the lines A C and B C respectively. Show that the line A B is trisected in the points E and F.

(*To be continued.*)

* If A $=0$, and B $=0$, C remaining finite, equation (i.) assumes the form C $=0$, a form apparently expressing an absurdity. In reality, however, the equation in this form admits of interpretation; for if, instead of being made equal to 0, A and B were made to become indefinitely small in actual value, we see that $-\frac{C}{B}$, and $-\frac{C}{A}$, that is the intercepts on the axes, become indefinitely great, and $\left(-\frac{C}{B}\right)+\left(-\frac{C}{A}\right)$ assumes the indefinite form $\frac{\infty}{\infty}$; hence C $=0$ may be assumed to represent a line at an infinite distance from the origin and indefinite in direction.

Our Chess Column.

By Mephisto.

SOLUTION OF PROBLEM, p. 381.

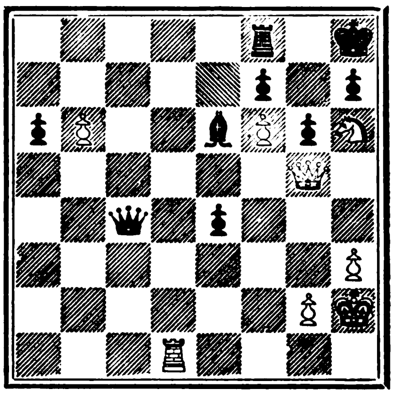
1. Q takes P on Q5 (ch.) The Queen can now be taken by seven pieces, each capture giving a separate mate, *i.e.*,

1. K takes Q	2. R to QR4 mate
1. P takes Q	2. R to K6 mate
1. B takes Q	2. B takes R mate
1. R takes Q	2. R takes B mate
1. Q takes Q	2. B to Kt2 mate
1. Kt(Kt3) takes Q	2. Kt to B5 mate
1. Kt(Kt5) takes Q	2. Kt to Q6 mate

A most beautifully constructed problem, and difficult, in spite of the first move being a check.

POSITION IN A GAME PLAYED IN THE DIVAN TOURNAMENT.

Black.



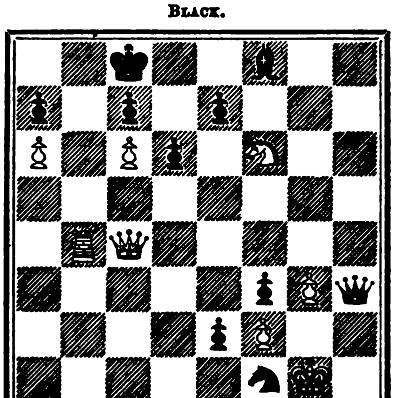
White.

White wins by

Kt takes P	B takes Kt	or	K to Kt sq
Q to R6	R to Kt sq	Q to R6	K takes Kt
R to Q8	B to K sq	Q to R7(ch)	K to K sq
R takes B	Q to KB2	Q to K7 mate	
R to K7 and wins			

The following extraordinary ending was sent to us by a correspondent, and is supposed to have occurred in actual play. It certainly looks more like a very difficult problem how to mate in five moves:—

Black.



White.

White is threatened with a mate on the move, and the P on K7 can go to Queen. In this desperate position White played:—

1. Q to K6(ch)! — Q takes Q

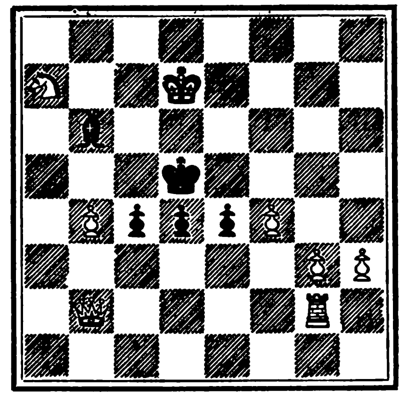
(1. R to Kt8(ch) is of no use, as K takes R. 2. Q to Kt4(ch), K to B sq. 3. Q to Kt7(ch), K to Q sq, and the Q can interpose next move and win.)

2. Kt to Q7 — Q takes Kt (forced)
3. R to Kt8(ch)! — K takes R
4. P takes Q, and mates next move.

PROBLEM (SELECTED.)

By W. Grimshaw.

Black.



White.

White to play and mate in three moves.

ANSWERS TO CORRESPONDENTS.

*** Please address Chess Editor.*

W. Hamahan.—We should be much obliged if you would kindly send us a copy of the Problem published in the "Irish Fireside" of April 7.

H. M. W.—Solutions are published a fortnight after publication of Problem. Correct solutions received of H. M. W., S. Osborne.

W. Hamahan, Donna.—Solutions incorrect.

Contents of No. 135.

KNOWLEDGE
AN ILLUSTRATED
MAGAZINE OF SCIENCE
PLAINLY WORDED-EXACTLY DESCRIBED

LONDON: FRIDAY, JUNE 13, 1884.

CONTENTS OF No. 137.

THE EXTINCTION OF SPECIES.

By GRANT ALLEN.

ON Lundy Island, in the Bristol Channel, there lingered on till lately, and perhaps there still lingers on even now, almost the last surviving colony of the old British black rat. In the mainland of England itself that aboriginal indigenous rodent is by this time probably all but quite extinct. True, one sees every now and again, among the paragraphs of gossip in the naturalists' journals, a story to the effect that a black rat has been caught and killed by a farm labourer in some out-of-the-way cottage or shieling in the heart of the country; but if the very last specimen has not yet gone finally to his rest beside the last wolf, the last beaver, and the last wild boar, in Mr. Harting's historical collection, it is at any rate quite clear that the black rats have nearly disappeared from the face of the earth, and that only single solitary individuals still drag out a miserable and lonely existence in the holes and corners of this rat-haunted land. Nothing more wretched can be conceived than the position of such last hunted-down representatives of a dying race. Living alone in a world which knows them not; cut off from all social intercourse with their own kind; probably denied the companionship of mates or young—these poor last rats, and last Thames otters, and last Welsh pine martens, have something of the same pathos for the naturalist as ethnologists find in the last of the Tasmanians, dying in the midst of the intrusive white-faces on his lonely rocky Pacific isle.

There is, however, one great point of difference between the extinction of the black rat and the extinction of the wolf and the bear in England, or of the moa, the dodo, and the rhytina or sea-cow elsewhere. These other animals have all been destroyed and annihilated by the hand of man alone, or very nearly so. The wolf, we know, was deliberately hunted down and extirpated in these islands, first in England and Wales, then in Scotland, and finally in Ireland. A similar dead set had probably been made at the British bear at a still earlier period. The moa, in spite of its gigantic size, was eaten down steadily by the first Maori settlers of New Zealand; the dodo was ruthlessly shot off by the early European visitors to Mauritius; and the sea-cow was regarded as a sort of natural larder by the original navigators of the Behring's Straits. Now, extinction of a species in this way could never exactly have occurred before the advent of man upon the world; and therefore it may be regarded to some extent as an exceptional and almost abnormal process of nature. It does not seem probable that any other carnivore except man has ever really succeeded in extinguishing any one of the species upon which it preyed. There appears to be usually a certain natural see-saw between the numbers of the carnivores and of their victims: as the antelopes decrease, the lions necessarily die off of starvation; as the lions die off, the antelopes increase again; as the antelopes grow more numerous, more young lions can find a living for themselves by preying upon them; and so on *ad infinitum*. But it cannot often happen that the carnivore actually kills off an entire species; man alone seems to be cunning enough and persistent enough to carry out a whole system of extinction, deliberate or otherwise, upon that large and regular scale.

In nature at large, before the advent of man, it seems more likely that species generally became extinct by the competition of other closely allied forms. Competition is always keenest and fiercest between those of the same trade. The grocer's deadliest foe is not the soldier or the sailor, but the rival grocer. So it is not the lion that kills out the particular antelope type, but some other and better adapted competing antelope. This is a point which is often misunderstood by the unscientific public in the interpretation of such phrases as "the struggle for existence," and "the survival of the fittest." Strange as it sounds to say so, the worst enemies of each species are not its natural foes, but those of its own household. We could hardly have a better instance of this than the history of the old black rat of England. Throughout the whole of the Middle Ages, the black rat flourished vigorously in our midst, living upon the fat of the land, and having no competitor in its own field within the limits of the British Islands. Now and then, a man might set a trap for a particular black rat, and catch him and kill him; but against the species as a whole, man was practically powerless, for it is a noteworthy fact that while we can easily exterminate a very large animal like the elephant, the bear, or even the wolf, we are unable to cope successfully with smaller creatures like mice or rats, and absolutely helpless to annihilate the Colorado beetle, the common bed-bug, or, still more, the almost microscopic phylloxera. The whole race has been engaged in catching fleas (off and on) for the last hundred centuries, and it is doubtful whether there is one flea the less in the world to-day than there was at the first beginning of that ceaseless crusade. No, it was not man or the dog, his ostensible enemies, that exterminated the black rat; it was an invader of his own kind that eat him finally out of house and home.

Where the brown rat came from nobody exactly knows, but probably from the east, the home of early civilisation, with all its attendant pests. There is some reason to believe, indeed, that the black rat is the older animal of the two—the most primitive house-haunting member of the genus—for it is found even in New Zealand, where it must have been taken over (accidentally, of course) by the ancestors of the Maories, as there are no other indigenous mammals in those great islands. However, the brown rat, developing afterwards its specific peculiarities in India, apparently, acquired a set of qualities which enabled it far better to exist in the midst of a highly-civilised community. For many years it was known in Asia, and, at last, in 1727 (frightened by an earthquake, according to the Russian naturalist, Pallas) it swam the Volga, and invaded Europe.

At once, the old black rat began to find that the new competitor was an enemy of quite another kidney from man or the dog, with their desultory warfare. The big brown rat met the older inhabitant at every point of the compass; competed with him all along the line; eat up everything on which he used to feast in perfect security; and, in short, played toward the black rat the part of the white man toward the Red Indian of America. About the middle of the last century it took ship for England (from Hanover, said the Tory jokers of that day), and, having a most vigorous constitution, with great ferocity, and a wonderful faculty for breeding fast, it soon overran the whole land. The smaller black rat was quickly driven out before it, not, in all probability, by actual hard fighting (though rats are not entirely devoid of cannibal tastes), but by stern competition—the new-comers shortly filling up all the posts in our houses, barns, and sewers, once so securely occupied by the original and old-established British rats. As the brown rat breeds several times in a year, and has sometimes as many as fourteen young in a litter, it is not remarkable that it spreads rapidly wherever it has once acquired a firm foothold.

It is by just such severe competition between closely-allied species and varieties, in all probability, or at least between creatures adapted to fill exactly the same place in nature, that most extinct plants and animals have been really exterminated. One could easily give several other examples of similar extinction taking place beneath our very eyes. I shall be satisfied with recording a single one. Buxbaum's veronica, a handsome, vigorous plant, with comparatively large blue flowers, abundant in Southern Europe, was practically unknown in England some forty years since. More than one local collector has put on record the delight with which he hailed its first appearance in his own garden, much as White of Selborne rejoiced ecstatically over the arrival of the earliest Oriental cockroach in his remote country parish. They would not raise any pæan over either creature at the present day. The southern veronica has rapidly overrun half the gardens in the south of England, till it has become one of our commonest and most persistent weeds; and in doing so, it has ousted the old native *Veronica agrestis*, which is clearly an earlier inferior form of the same type, less healthy, less attractive, and less pushing, with smaller inconspicuous flowers, and a weaker physique in every respect. If Buxbaum's veronica continues to spread with its present aggressive jingoism, it will before long have exterminated its less vigorous competitor entirely, over all the southern portion of these islands. I know many spots where *agrestis* was once common, but where you may now look in vain for any veronica save only Buxbaum's.

THE DIVIDED SKIRT.

By a Lady.

When I wrote, some fifteen months ago, on this subject, I had only worn the divided skirt a few months. I felt even then, however, the good of the change and how much good it promised to do. I can now write after nearly two years' experience, and I think what I have to say may be of use to many.

In the first place I wish strongly to correct the mistaken idea which many have of the divided skirt,—first from pictures in the comic papers, supposed to represent it, and secondly (and chiefly) from the dresses with divided skirts shown at the Health Exhibition. The divided skirt is not and was never meant to be a divided *dress*. It is simply a divided *petticoat*, and practically assumes the form of a pair of trousers with deep frilling. In *this* respect, some of the divided skirts at the Health Exhibition are right enough; but even in their case, every chance of their being ever *generally* adopted by ladies is destroyed by their being only half hidden by the skirt of the dress. They should be fully hidden, precisely as a petticoat would be. In saying this I am expressing no opinion as to the dress being more or less artistic, or more or less hygienic, when the few inches necessary to hide the divided skirt are added to the shortened dress-skirts of Mrs. King. I assert simply, what I am sure to be the case, that ladies never can be persuaded, and ought not to be willing, to wear the dress advocated by Mrs. King and Viscountess Harberton. Mrs. King is doubtless right enough in saying that ladies, and expecially young ladies, do in fact dress with the wish to be, in a sense, conspicuous; there is certainly no harm in a lady desiring to be attractive (which is really what Mrs. King meant) by the elegance of her dress, its grace and style, its well-chosen combinations of colours, and so forth. But not one lady in a thousand will suffer any peculiarity in her dress to appear which will excite ridicule or rude remarks, and the thousandth, who imagines she is helping to introduce improvements in dress when she exposes herself to insult in the streets, is quite mistaken. I believe (from my own experience and the experience of others) that thousands would now be wearing the divided skirt if they had not been deterred by the conspicuous appearance of the arrangement recommended by Mrs. King. (The Turkish trouser form exhibited at the Health Exhibition is simply *impossible.*)

Now to give some results of my own experience.

I first tried the divided skirt for the sake of warmth and lightness. I travelled a great deal, and no matter how many skirts I might wear, I found my lower limbs, from the boot-tops to the knees, suffered always from cold. My husband never suffered in that way, though he had not half so many clothes around him. It seemed to me that I might, without any one noticing the difference, adopt, under the dress (unchanged in length and appearance) the same plan for keeping me warm that he and other gentlemen follow. Mrs. King very kindly let me have one of her divided skirts as a pattern. I made a skirt for myself. I found it just what I had wanted,—warm, light, and comfortable in every way. I was able to leave off all heavy petticoats, and felt at once relieved from a great burden, able to walk farther and more easily, and much more active.

Shortly before this, I had tried the experiment of discarding stays. Like most ladies who have been accustomed to wear stays since girlhood, I found it very difficult to do without them. When I walked my skirts were such a drag—now I had left off stays—that unless I made some change with the petticoats, I must soon have put my stays on again. I was very reluctant to do this, for though I had never laced tightly, or even closely, I felt much freer in many ways, could bend or stoop without fear of whalebones breaking, besides being able (which is perhaps rather unimportant) to *really* breathe,—for the first time (I suppose) since I had begun to wear stays. The change to the divided skirt came to my rescue. With divided skirt and without stays, I was perfectly free. I found, rather to my surprise, that whereas when I wore stays I often felt inclined to depend upon them for support, and so would not always sit or stand quite upright, it now seemed to come naturally to me to hold myself well up. And in passing, I may remark that this is one of the first points which ladies who have left off stays should attend to, if they wish their dresses to

set properly; they cannot expect this if they "double up;" but I can answer for it the fault does not lie with the removal of their stays, though it may have been in part caused by the constant wearing of very tight stays.

In the case of fat persons of the mile-stone persuasion, I know not what to say. I really cannot expect them to go without stays, when I think how they would look at first. But possibly they might find that, relieved of their burden, they would be inclined for more exercise and in that way get a figure considerably better than they now get from stays.

And now, after nearly two years, I can answer with great confidence the assertion, very confidently made by many ladies, that it is impossible to do without stays. At the end of the first week I was convinced that I could do very well without them. Other members of my family who soon after tried the experiment had the same experience. Among the ladies I have persuaded to try it, have been some well advanced in life. With children the effect is delightful, and pleases every one. Nothing would ever induce me (or my friends who have tried the change) to go back to the imprisonment of stays or to resume the burden of petticoats.

DREAMS:

THEIR PLACE IN THE GROWTH OF PRIMITIVE BELIEFS.

By EDWARD CLODD.

II.

ILLUSTRATIONS of the low intellectual stage of some extant races not quite at the bottom of the scale, drawn from simple matters, will make clearer how they will interpret matters of a more complex order, and interpret them only in one way.

Of the beginning of thought we can know nothing. For numberless ages man was marked out from the animals most nearly allied to him by that power of more readily adapting means to ends which gave him mastery over nature. Through that dim and dateless time he thought without knowing that he thought. "His senses made him conversant only with things externally existing and with his own body, and he transcended his senses only far enough to draw concrete inferences respecting the action of these things." He is human only when the thought reaches us through articulate speech. Language, as a means of communication between him and his fellows, denotes the existence of the social state, the play of the social evolution which gives the impetus to ideas. Language is the outcome of man's social needs and nature; he speaks not so much because he thinks and feels, as because he must perforce tell his thoughts and feelings to others. And by the richness or poverty of his speech we may assess the richness or poverty of his ideas, since language cannot transcend the thought of which it is the vehicle.

By what tones and gestures, by what signs and grimaces, the beginnings of speech were made, we know not. Countless processions of races appeared and vanished before language had reached a stage when the elements of which words are built up could be separated, and the reason which governed the choice of this and that sound or symbol discovered. Now and again, when a correspondence is found between the roots of terms in use amongst the higher races and the names given by lower races to the same thing, we get nearer primitive thought, the correspondence being not always in sound or spelling, which may be delusive, but in physical or sensible meaning. But the inquiry is speculative, and we are concerned with matters of fact.

Language proves the limited range of ideas among barbaric peoples in the absence of their capacity to generalise. They have a word for every familiar thing, sound, and colour, but no word for animal, plant, sound, or colour as abstract terms. It is the concrete, the special shape and feature and action of a thing which strike the senses at the outset; to strip it of these accidents, as we call them, and merge it in the general, and realise its relation to what is common to the class to which it belongs, is an effort of which the untutored mind is incapable. Many of the northern non-Aryan tribes, as among the Mongols, have names for the smallest rivulet, but no word for river; names for each finger, but no word for finger. The Society Islanders have a separate name for the tails of various animals, but no name for tail. The Mohicans have verbs for every kind of cutting, but no verb "to cut." The Australians and other southern aborigines have no generic term for tree, neither have the Malays, yet they have words for the several parts—the root, stem, twig, &c. When the Tasmanians wished to express qualities of things, as hard, soft, warm, long, round, they would say for hard, "like a stone"; for tall, "long legs"; for round, "like the moon," and so on. Certain hill-tribes of India give names to sunshine, candle, and flames of fire, but "light" is a high abstraction which they are unable to grasp. Some of the Red Race languages have separate verbs for "I wish to eat meat," or "I wish to eat soup," but no verb for "I wish." Of course, the verb "to be," which, as Adam Smith remarked long ago, is the most abstract and metaphysical of all words, and therefore of no early coinage, is absent from a large number of barbaric languages. Abstract though it be, it is, as Prof. Whitney points out, made up of the relics of several verbs which once had, like all elements and parts of speech, a distinct physical meaning. As in "be" and "been" the idea of "growing" is contained, so in "am," "art," "is," and "are" the idea of "sitting" (or, as some think, of "breathing") is embodied. As an example of its absence, the Abipones cannot say "I am an Abipone," only "I Abipone." Turning to another class of illustration, we have proof what a far cry it is from the savage to the Senior Wrangler in the powerlessness of the former to count beyond his fingers—indeed, he cannot always count as far as that, any number beyond two bewildering him. One of the best stories to the point is given in Mr. Galton's "Tropical South Africa."

"When the Dammaras wish to express four, they take to their fingers, which are to them as formidable instruments of calculation as a sliding rule is to an English schoolboy. They puzzle very much after five, because no spare hand remains to grasp and secure the fingers that are required for units. Yet they seldom lose oxen; the way in which they discover the loss of one is not by the number of the herd being diminished, but by the absence of a face they know. When bartering is going on, each sheep must be paid for separately. Thus, suppose two sticks of tobacco to be the rate of exchange for one sheep, it would sorely puzzle a Dammara to take two sheep and give him four sticks. I have done so, and seen a man put two of the sticks apart, and take a sight over them at one of the sheep he was about to sell. Having satisfied himself that that one was honestly paid for, and finding to his surprise that exactly two sticks remained in hand to settle the account for the other sheep, he would be afflicted with doubts; the transaction seemed to come out too "pat" to be correct, and he would refer back to the first couple of sticks; and then his mind got hazy and confused, and wandered from one sheep to the other, and he broke off the transaction until two sticks were put into his hand and one sheep driven away,

and then the other two sticks given him, and the second sheep driven away. Once while I watched a Dammara floundering hopelessly in a calculation on one side of me, I observed Dinah, my spaniel, equally embarrassed on the other. She was overlooking half a dozen of her new-born puppies, which had been removed two or three times from her, and her anxiety was excessive, as she tried to find out if they were all present, or if any were still missing. She kept puzzling and running her eyes over them, backwards and forwards, but could not satisfy herself. She evidently had a vague notion of counting, but the figure was too large for her brain. Taking the two as they stood, dog and Dammara, the comparison reflected no great honour on the man."

Dr. Rae says that if an Esquimaux is asked the number of his children, he is generally puzzled. After counting some time on his fingers he will probably consult his wife, and the two often differ, even though they may not have more than four or five in family. Of the languages of the Australian savages, who are the lowest extant, examined by Mr. Crawfurd, thirty were found to have no number beyond four, all beyond this being spoken of as "many," whilst the Brazilian Indians got confused in trying to reckon beyond three. The list of such cases might be largely extended, and although exceptions occur where savages are found with a fairly wide range of numbers, notably where barter prevails, the larger proportion of uncivilised people are bewildered at any effort to count beyond three or five. The fingers have, in most cases, determined the limits, for men counted on these before they gave words to the numbers, the words being at last borrowed from the fingers, as in our "five," which is cognate with the Greek "pente," and the Persian "pendji" (said to be derived from the word for "hand"), and "digits," from Latin "digitus," a finger. This limited power of numeration thus shown to be possessed by the savage, justifies the remark made in a former paper, that he is nearer to the ape than to the average civilised man, nearer, as the extract from Mr. Galton shows, to the spaniel than to the mathematician. What conception of the succession of time, still less of it as a confluence of the eternities, can he have whose feeble brain cannot grasp a to-morrow! And yet the difference is not one of kind, but of degree, which separates the aborigines of Victoria or Papua from the astronomer who is led by certain irregularities in the motion of a planet to calculate the position of the disturbing cause, and thereby to discover it nearly a thousand million miles beyond in the planet Neptune.

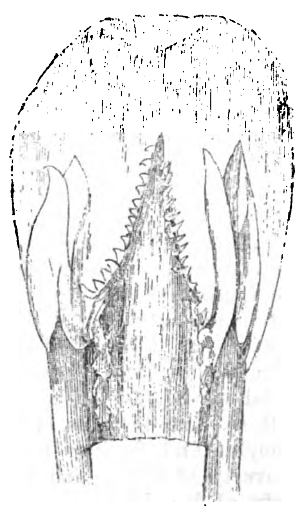
PLEASANT HOURS WITH THE MICROSCOPE.

By HENRY J. SLACK, F.G.S., F.R.M.S.

AT this time of the year there are numbers of interesting facts to be observed in the structure of various garden and field flowers, and it is well to walk about with a pocket magnifier, and examine all that are found in bloom. Look, for example, at an Azalea or a Rhododendron. Both belong to the Heath family, and the anthers of both can be seen with a low power to be formed like little bottles with open mouths, out of which comes the pollen when it is ripe. Many insects frequent the rhododendrons, and big ones, such as humble bees, rout its flowers about very roughly. They can, however, do no harm to the pollen while it is maturing inside the bottle, and when it is ripe they carry it off on their hairs, and convey it to another flower on the same or on another bush.

The pretty Borage, so striking for its exquisite blue tint, and its relative, the Comfrey (*Symphytum*), exhibit other curious arrangements. The stamens of the Borage press round the pistil, enclosing it as in a tube. Down below, at the base of the stamens, is the honey-drop the bees wish to get at; and, to do this, they must push the anthers on one side, as they thrust in their tongues. The pollen, when ripe, is discharged inwardly, and falls into the space between the filaments and the style. The bee's tongue comes into contact with it, and carries it away. Then the insect retires, the anthers spring back to the style, and the pollen is again shut up, and safe from other attacks. Bees, on any one excursion, usually proceed from flower to flower of the same sort, and many insects only frequent particular flowers, so that when any kind of pollen adheres to them, it is likely to be taken to a plant of the same species. There are numerous ways in which self-fertilisation is avoided, and the pollen of one flower conveyed to another by the agency of insects, or by the wind. In the latter case, the supply of pollen is large enough to allow for much loss, and the pistil is feathered or sticky, so as to have a better chance of catching some of it.

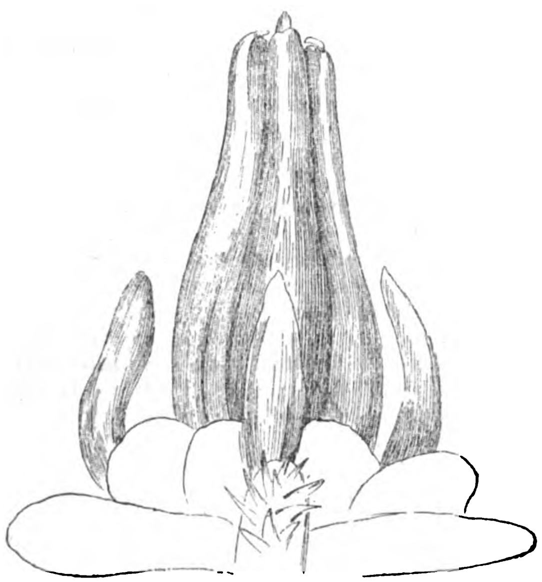
By the way side and in the meadows there is now abundance of sorrel (*Rumex acetosa*). It often supplies a pretty red tint to grass fields that contrasts well with their green; but the ordinary wayfarer or tourist, though he may chew a leaf or stalf for its refreshing acid, is heedless of its beauty. A hand-magnifier shows that elegant little tufts of pink and white hue and glassy aspect protrude from its imperfect flowers. They are like sea polypes with their tentacles displayed, and are fringed appendages to the three short styles. The flowers are generally diœcious, the two sexes being on separate plants, but sometimes they are monœcious and diclinous, on the same plant, but on different flowers.



Segment of Comfrey Petal: two anthers and one scale. Tip of segment recurved. × about 12.

London Pride, now in free bloom, is a very fine object for the microscope and a low magnification. The white petals are elegantly decorated with yellow spots below groups of pink ones. The stigmas are supported upon a round cushion, from beneath which the anther filaments radiate in star pattern. The stems are covered with glandular hairs, like minikin pins with ruby heads. The plant belongs to the Saxifrage family (*Saxifraga umbrosa*), and besides its London appellation is called None-so-Pretty, and, for some reason a student of folklore may be able to explain, St. Patrick's Cabbage.

To return to the Comfrey, we find its flowers in pendulous groups (*exuves*). Each flower hangs from a pedicel, and its tubular corolla is five-cleft. The humble-bees are very fond of it, and pierce holes near the base of the corolla to get out some nectar-juice. Looking over a considerable number of flowers I found all with two or more perforations, often so close as to be nearly continuous. Is this done because the juice of the corolla is what the bees want? In other cases, as in the flower of the arbutus, the insects generally content themselves with one hole, and the hive-bee avails itself of the opening made by his bigger and stronger-mandibled relative. Humble-bees do not mind being closely watched, and will allow a hand-magnifier to be brought near them without disturbing their proceedings. In many instances I noticed that they thrust their long tongues into the mouth of the flower as well as into the lower part of the corolla. This brought them into contact with its pollen.



Portion of Borage-flower, showing the position of anthers. × about 12.

The annexed sketches show a portion of the Comfrey corolla split open and flattened. Next to the anthers are ornamental scales, with glandular bulbs at their edges. These group themselves round the style, which is long, and projects beyond the corolla. In the common Borage, to which the bees' visits have been described, the style is shorter than in Comfrey, and the stigma is hidden by the anther group. The five stamens have short-forked filaments, and the anthers are dark purple, almost black. The narrow sepals, which are of a brown tint, add five intermediate rays to the broader-rayed star of the five petals, which they alternate with. Each petal of this flower has a glassy-looking scale at its base, composed of glittering cells, and with five hairs inside. The five scales form a cup, and next to them are dark purple, finger-like projections, shorter than the anthers. The Borage family may be generally known from all but the Labiates—which they in no other respect resemble—by their ovaries presenting the appearance of four little nuts.

Besides the arrangement mentioned, which facilitates the fertilisation of the flowers by bees, the opposite practice of self-fertilisation is hindered by the flower being *protandrous*, that is, having the anthers ripened before the stigma. As the Borage is in flower a long time, the bees, in passing from one plant to another, and from one flower to another, are pretty sure to find ripe pollen in some and stigmas fit to receive it in others. Lungwort (*Pulmonaria*) is a Borage which flowers in the spring, and is recognisable now by its pretty spotted leaves. It has quite a different method of obtaining cross-fertilisation. Its nectar is very attractive, and it produces two sorts of flower, one with long styles and the other with short. Insects carry the pollen of one form to the stigma of the other, and it was supposed no seed could be produced otherwise; but Lubbock cites Darwin as having raised seedlings from long-styled plants which were fertilised by their own pollen.

Pig-nuts are now abundant in grass-fields, road-sides, and gardens. They may be known by their long, thin stems, from one to two feet high, slender leaves, and heads of very small white flowers. They belong to the umbeliferous or parsley family, and get little notice except from schoolboys, who dig up their edible roots; but they deserve the attention of the microscopist, as their flowers are beautiful little things, and both in their partially and fully open states, suggest patterns for the designer.

The above plants are mentioned merely as specimens to excite attention. It may be added that such objects as the stigma-fringes of the sorrel may be mounted in Canada Balsam, well thinned with benzine, having first soaked them for a few minutes in carbolic acid; the colour is lost, but the form well preserved.

THE UNIVERSE OF SUNS.

By RICHARD A. PROCTOR.

THE method of star-gauging imagined such a degree of uniformity in stellar distribution within our star system,—assumed to have attainable limits—that the number of stars seen within any given field indicates fairly the extension of the system in the direction of that field. Taking fields equal in size and observing them with equal telescopic light-gathering power, we should have the same sort of estimate of extension that we should obtain of sea depth by letting down a tube to the sea-bottom and weighing all the water inclosed within the tube. All that is necessary to make the estimate satisfactory, is that we should really reach the limits of the system, and that the stars really should be spread with tolerable uniformity. The size of individual stars counts for nothing in this method, assuming only (as Herschel assumed) that a telescope powerful enough to reach easily to the limits of the system, would show every star within the system; in other words, that there are no *very* great discrepancies of magnitude.

It was as the result of this process and no other, that Herschel arrived at the stratum-theory of the sidereal system—or rather the cloven-stratum theory.

When this theory was formed, Herschel had not met with even the simplest cases of unequal star distribution; he had not yet learned that such objects as binary stars exist in the universe. Struve has well pointed out that the discovery of the physical association between certain double stars changed the whole aspect of the star system in Herschel's eyes. From this time he recognised the fact that there are different orders of stars, and different systems of star-grouping.

Herschel himself abandoned altogether, as well the principle of star-gauging as the results to which it had led him.

Herschel not only said that the Milky Way was not what he had supposed; but he stated what in his opinion was the actual shape and constitution of certain parts of the Milky Way. He found that certain of the brighter

parts of the Milky Way were not regions of the sidereal system owing their apparent richness to their vast extension, but true clustering aggregations—not parts of an extended stratum but globe shaped.

The *resolution-test* is very commonly confounded with star-gauging, but is quite distinct in principle. According to this plan the distance of any star-group was to be determined by the telescopic power necessary to resolve the group. To show the distinction between this method and the other, it is only necessary to point out that in star-gauging, the same telescope, or at least the same telescopic power, was necessarily employed throughout the inquiry; in the new method, the telescopic power was made variable, gradually increasing power being applied until a star-group was completely resolved, and the power required for this purpose was held to indicate the distance of the group. There was a partial return, in the adoption of this method, to the general idea of uniformity of distribution which had led Herschel to erroneous results in 1785; and in precise correspondence with this fault in the new method we recognise in the results obtained an obvious incorrectness, which would not have escaped the attention of Herschel in earlier years.

The new method was one to be tested, not *adopted*. The method of star-gauging had been adopted, and had eventually to be rejected. This experience should have suggested that no new method ought to be adopted until it had been subjected to some adequate test. The results obtained by Herschel showed sufficiently (and in his earlier years would have been regarded as showing) that the method was unsound.

Let one point suffice to show this :—

The fine double-cluster in the sword hand of Perseus was partly resolved with the lowest power Hershel employed, and this, according to the criterion he had adopted, showed that part of the cluster lay only just beyond the limits of naked-eye vision. But he was unable with the highest powers he employed to resolve this spot completely, and this, according to his criterion, showed that part of the cluster lay beyond the range of his highest powers, or some twenty times farther away than the limits of naked-eye vision. Thus, according to his criterion, the distances of the nearest and farthest parts of the cluster are as one to twenty, at least. But the apparent size of the cluster shows that the breadth of any part of the cluster is less than the hundredth part of the distance of that part. Combining these indications, we have for the shape of the space within which the cluster is included, something like A B (Fig. 8), where S is the place of the Solar system.

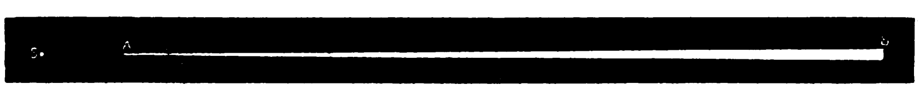


Fig. 8.

But no one can accept so monstrous a result as this; nor would Herschel for a moment have adopted it, had he noticed that his own figures led directly to it. Apart from all other objections, the circumstance that this long-projecting cluster had the Sun directly at its apex, would have sufficed to show that some other interpretation must be adopted. But no one who has observed this or any other star cluster can doubt for an instant that the portion of space occupied by the cluster is rounded—speaking roughly. The cluster is not a globular cluster, of course; it has outlying branches and streams (and appears, by the way, to be connected with the general stream of the Milky Way in this neighbourhood); but that, regarding it as a whole, we must consider it as a rounded nodule or double nodule of the galactic stream, and not as a projecting region directed exactly from the sun, no one can doubt who has ever examined the cluster.

Nor did Herschel question this, or fail to recognise the soundness of the method of reasoning, illustrated by Fig. 8, which I have used to dispose of the strange result to which Herschel's figures lead. But the real fact is that Herschel did not live to examine fully the facts he supposed he had accumulated in and after 1817. He did not do much, indeed, towards carrying out the scheme which he had indicated. The papers of 1817 and 1818 may be compared respectively with those of 1784 and 1785; and I venture to say, that if in 1818 Herschel had had before him as long an observing career as in 1785, we should have found him definitely abandoning the principle he indicated in 1817, precisely as we find him in 1802 definitely abandoning the principle he indicated in 1784.

The fact is, both principles were matters to be tested, not to be adopted as rules for guidance.

William Struve adopted a singular combination of the two methods, and interpreted the results he obtained in a most remarkable, and as I think, altogether unsatisfactory, manner.

Struve perceived that if the principle of star-gauging had been sound, the stars of the brighter orders ought to be strewn with general uniformity over the heavens; for the range of naked eye vision, or low telescopic power, for separate stars lies far within the limits of the stellar system, even in its narrowest parts—that is, measured in the direction of its thickness. He saw then that here was a new test applicable to the star-gauging principle. It was only necessary to take some complete catalogue of stars down to, say, the 7th or 8th magnitude, and to try whether the stars grew richer as the Milky Way was approached. If they did grow richer, then it would be manifest that the principle of star-gauging could not be trusted *in all its generality*. (The reader will presently see why I emphasise these four last words.)

Struve took a catalogue complete only for an equatorial zone thirty degree wide, and extending fifteen degrees from the equator on the north and on the south. The catalogue was tolerably complete down to the ninth magnitude inclusive. Like other catalogues, it was numbered and arranged in order of Right Ascension; and what Struve did was simply to ascertain how many stars it included in the different "hours." He found that the Milky Way "hours," the seventh and nineteenth, were the richest, and that there was a gradual increase up to the maximum of richness, and then a gradual decrease down to the minimum, so that there was all the evidence his method could afford to show that the nine brighter order of stars do increase in numbers as the Milky Way is approached.

If he had been satisfied with this result, Struve would simply have obtained rough evidence of those laws of stellar distribution which my methods have more particularly indicated.

But he was not so satisfied, and it was in analysing his result that he applied the novel considerations I have spoken of.

He first assumed that the law of distribution for an equatorial zone thirty degrees wide, might be regarded as the law of distribution for the circuit of the equator itself —a particularly daring assumption, when we remember that the galaxy does not cross the equator at right angles but slantwise, so that an "hour" of stars thirty degrees in declination range crosses parts of the star sphere of very different richness (assuming, as Struve did, that the mid-zone of the galaxy is the zone of greatest richness). This first step then—this substitution of the equator itself for an equatorial zone covering more than a quarter of the whole heavens—was directly calculated to mask the real features of stellar distribution.

But the next step was even more daring. Having found that there is a variation of star-richness round the equator, two opposite points of which circle mark the place of maximum richness, Struve next changed the circle into a disc, and in this way:—

Suppose A B, Fig. 9, to be one "hour" of the celestial equator, S the place of the solar system, *a b c d* one "hour" of the equatorial zone dealt with by Struve. Then he took the stars within the space *a b c d*, and having distributed them uniformly along the arc A B, he next spread them

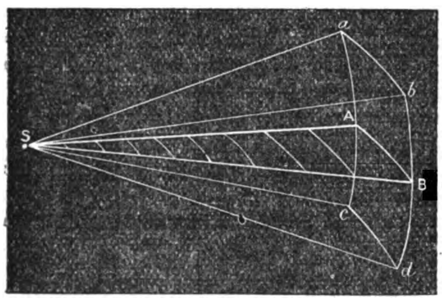


Fig. 9.

(according to the numerical distribution of their different orders of brightness) over the sectorial area A B S, Figs. 9 and 10.

Surely an amazing application of the laws of averages, to take the stars spread over the area *a b c d* of the celestial sphere, where A B is an arc of fifteen degrees, and *a b*, *c d*

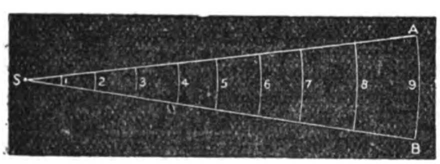


Fig. 10.

each of thirty degrees, and to conceive these stars spread, according to a special law of uniformity, over the sectorial plane A B S. Yet it was thus that Struve's celebrated *section* was obtained.

I hesitate to point out objections against this process, because I cannot conceive what arguments can possibly be urged in its favour.

Struve himself considered that his result agreed perfectly with the views of Herschel in 1817 and 1818; and it will be perceived that there is something in the formation of the sectorial star-area S A B, which bears a distant resemblance to Herschel's resolution test of star-distance. On the other hand, Struve perceived that his results were altogether opposed to the general theory of star gauging. He adopted, however, a modified form of the principle, since he assumed a very peculiar law of uniformity of distribution.

[In passing, I may remark that, in answering the first paper I read on this subject at the Royal Astronomical Society, Professor Pritchard made the somewhat remarkable statement that all Herschel's results confirmed each other, and that Struve's agreed with them. But it is certain that Professor Pritchard has never read Sir W. Herschel's papers through.]

The plan on which I have proceeded has been unlike any of those hitherto described, and the principle I have adopted is not an assumption, but one whose justice depends on manifest and sufficient considerations.

I have, in the first place, set equal surface charting as the only effective way of indicating laws of distribution, if any such exist; and charting *generally* as a necessary substitute for mere cataloguing, or other process of numerical estimation, whether as applied to the stars of various orders, to nebulæ, to clusters, to double, coloured, or variable stars, or lastly, to star motions. You can *see* a chart, study its relations, return again and again to its investigation, compare it with other charts, or combine it with them in a new chart, and so on. Any characteristics which charts are competent to exhibit, they show to all, and reveal independently of any special process. The case is altogether different with catalogues. Their capacity for concealing the truths really contained in them, and for mystifying the student of nature, and their adaptability for deceptive processes of manipulation, have been repeatedly illustrated in scientific researches.

But of course, when once a chart has been drawn, it suggests questions for statistical research; or rather it suggests really effective methods of dealing with numerical relations. And the construction of charts of distribution, has led me to notice two principles of analysis, which are manifestly sound, and have already been shown to be effective, while I believe very confidently that they will hereafter be found to be the only principles which can enable us to make any satisfactory advance in dealing with those portions of space which lie beyond our means of measurement.

They are these:—

A. If objects of one class are found to be spread *over certain parts of the celestial sphere* more richly than over others, and if objects of another class are found to exhibit corresponding peculiarities of distribution, being always more richly spread or more sparsely strewn where the same is observed with objects of the former class, then the two classes of objects form one system, and are intermixed within that system, the same *subordinate* region of *space* within that system including the aggregations of both classes of objects.

B. If, on the contrary, a law of contrast is observed in the distribution of objects of two different classes, so that the distribution of the objects of one class is *systematically* richer or poorer according as the distribution of objects of the other class is poorer or richer; then the two orders of objects form one system, but are separated from each other within that system, those subordinate regions of space which include many objects of one class having few or no objects of the other class, and *vice versâ*.

I will illustrate these principles before passing on to a third, which is only *probable* (not as these are *certain*,—where only the evidence described can be obtained in sufficient *amount*)

Suppose the whole sky were covered more or less richly with flying creatures; suppose it could be seen that there were two well-marked classes of these creatures, large ones and small ones: then if, wherever the large ones were

richly strewn over *the sky*, the small ones were likewise so strewn, while where there were few large ones there were few small ones, it would be a safe inference that the large and small ones were intermixed in the *aërial spaces*, travelling together in flights or groups. They might be old birds and their small youngsters, or birds pursuing their prey among flights of insects, or the like; but whatever the two classes might be, it would be certain (supposing always that the peculiarity in question were sufficiently marked) that the large and small objects were intermixed.

But, suppose the reverse held, and that where there were many large flying creatures, there were few or no small ones, and *vice versâ*, the peculiarity being very marked. Then it would be manifest that in this case also there was a law of association, but one which caused the objects of the two classes to separate into different groups. The large objects might be birds of prey, and the smaller might be weaker birds, banding together for mutual protection, or the like; but it would be certain that the peculiar law of arrangement observed among the flying creatures was due to some real relation between the two classes, this relation causing them to keep apart from each other.

In neither case (and let the reader notice specially, that what I am about to say applies equally to both cases), in neither case could we imagine for a moment that the large flying creatures were of the same class as the small ones, but much nearer; for it would be practically impossible that by some strange accident two strata of creatures, flying at different levels, should have their rich and poor regions so adjusted as to be seen, in all cases, in the same direction, by the observer on earth.

Thus we are led to the third principle, which only affords probable evidence, though, in some cases, such evidence may approach very nearly to demonstration.

C. If objects of two classes are spread over certain parts of the heavens more richly than over others, but the rich regions of one agree systematically neither with the rich nor with the poor regions of the other, then the probability is, that the two orders of objects lie at different distances, and are in no way connected with each other.

This probability rises into certainty where some markedly peculiar arrangement of one order is seen to be carried across some equally marked arrangement of the other, the two arrangements being manifestly independent of each other.

The principles thus enunciated, are those which I have followed in the interpretation of all my equal surface charts, whether of stars or nebulæ. Applied to my chart of 324,000 stars, principle A shows conclusively that neither the earlier nor later view of Sir W. Herschel, respecting the Milky Way, nor the theory of the elder Struve, is even an approach to the true theory; *but*, the *general* views indicated by Sir W. Herschel during the period of his observing career intermediate between the abandonment of the star-gauging principle and the adoption of the resolution-test, accord fairly with the observed facts. The point overlooked by him, by Struve, and by all other astronomers, except the younger Herschel (who, however, did not sufficiently dwell on its importance), is the evidence of the intermixture of stars of many orders of real magnitude within the same regions of space. And as principle A, applied to the teachings of my chart of 324,000 stars, shows the Milky Way to consist of a system of star-streams, including many orders of stars, from the highest down to what must be regarded relatively as mere star-dust, so principle B, applied to my charts of Nebulæ (and still more clearly when applied to the exact charts recently submitted by Mr. Sidney Walters to the Astronomical Society), shows that the star-cloudlets belong to the same system as the stars of the Galaxy, but are so distributed within that system as to be rich where stars are relatively few, and few where stars are relatively rich.

But much wider and more systematic research is still required. It will be manifest that the three principles, but especially the first, admit of being applied most effectively to stellar research—much more effectively than star-gauging as originally proposed, even if it had turned out that the principle of such star-gauging was sound. By applying telescopes of different power to systematic star-gauging, as distinguished from the mere selection of a few widely-scattered fields, and by employing for each survey only dark, clear, and moonless nights, instead of using (as was the case with Herschel's surveys) moonlight, twilight, and hazy nights, and even daylight, an estimate can be formed of the laws of distribution of the stars of various orders, proceeding onwards from those included in my chart of 324,000 stars, and thus extending and amplifying the teaching commenced in that chart. When that work has been accomplished, we shall begin to understand the real wonders of the star depths, the magnificence of the subordinate star-schemes which have been mistaken for the sidereal system itself, and something of the grandeur of that system, whose limits lie far beyond the range of our most powerful telescopes, while within them are included all the various orders of celestial objects which the telescope has yet revealed. Combining such lessons with what has been learned and yet remains to be learned of the movements taking place within the sidereal universe, we shall have a picture grander and more impressive by far than any yet presented to our contemplation; we shall learn that the true galaxy is infinitely more extended, infinitely more complex in structure, than we have supposed, and that the processes at work within its bounds are infinitely more stupendous.

THE INTERNATIONAL HEALTH EXHIBITION.

BY JOHN ERNEST ADY.

III.

THE subject-matter of our present report will be confined to a general review of one of the most striking series of exhibits which it has been our good fortune to inspect. Within the space called the Central Gallery, at its eastern extremity, a rectangular space has been allotted to Messrs. Doulton & Co., of the Lambeth Sanitary Engineering Works and Art Potteries. So judiciously have they utilised the area entrusted to them, that visitors to South Kensington will find it well worth their while to spend a few hours there to admire and to learn.

We propose to give an outline of what might with justice be called an exhibition of itself; for therein are to be found works of art which would furnish material for a library of treatises, harmoniously blended with the requirements of sanitary science. We are allured into a realm where all is lovely in form and beautiful in hue, and where the watchword at the portals is "health." We leave the Doulton trophy and its annexes with the inward satisfaction of having gleaned a vast amount of profitable information, in an easy and pleasant manner. Our lingering glances dwell upon the tiers of filters we have examined, and remind us that we are soon to attempt the mysteries of the "water supply question," a theme which deservedly takes the foremost place here, as bearing directly and most powerfully upon the sanitary condition of humanity at large.

The "Doulton Section," as we shall term it, may be approached from the East Gallery down a short flight of stairs, and most persons would be inclined to hurry on to view the details of the mosque-like edifice before them, which occupies the central portion of the pavilion; but we would divert their attention for a moment toe-wards. They there stand on ground of adamant, for the terra-cotta steps are fitted with the patent so-called "Silicon Tread," which is formed of a material more durable that the hardest of Nature's stone, and has been expressly placed in its present frequently traversed position in demonstration of its quality to resist wear and tear.

One more interruption and we shall go *in medias res* without further comment. To the left and right hand side of the descended staircase are two exhibits of vases on shelves, one on each side against their respective walls. Those to the left-hand side are peculiar to the Lambeth Art Potteries, and are known as the famous "Doulton ware." They are the result of a prize competition by assistants in training. The original decorative designs have been produced upon a piece of ware of previously unknown shape by the unaided skill of the students, within a limited period of four hours. We do not like to make unfavourable comments, but we cannot here refrain from thinking that this *time* plan is essentially pernicious to the cultivation of legitimate art; it is somewhat analogous to the wretched system known as "cram" amongst science teachers; pupils are apt to coach up striking designs which can be

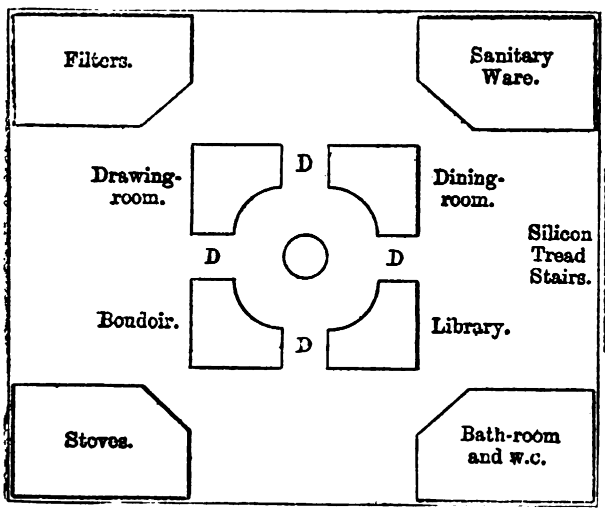


Fig. 5.—Plan of Messrs. Doulton's Pavilion.

executed in a short space of time, and finally veiled plagiarism takes the place of originality. Of course, from a purely commercial point of view, things are entirely different; nevertheless, the vases are excellent examples of the ware they are intended to illustrate. The glazes used are exceptionally pure and clear, and what seems to us to be an important advance from an artistic point of view, in Doulton ware, is, that although the colours chosen are brilliant, yet they are so happily intermingled as to leave in their totality a pleasurable impression rather than a sense of gaudy vulgarity. One can imagine a place for these ornaments in a room laid with Eastern carpets and lighted through delicately stained window frames. We were also led to observe that their glazes of splendent colours are free from unsightly cracks, or, as potters term it, they do not "crase." This crasing is a kind of bugbear to the potter who endeavours to produce rich hues: but Doulton ware evidently gets "scot free," probably because the coat of glaze penetrates into the "body" of their stone ware.

On the right-hand side of the terra-cotta Silicon-tread staircase there are a number of stunted urns of "Silicon ware," also the result of competition. These vases are unglazed, and although the colours are bright and pure, they are subdued, so that a singularly softened effect is produced. They are well worthy of careful examination, and do great credit to the students, especially to the lady students, whose names predominate among the prize-winners.

The general disposition of the exhibits may be gathered from a glance at the subjoined figure (Fig 5). A central square, access to the interior of which may be gained by four median, diametrically opposite, arched doorways (D. Fig. 5) gives support, upon elaborately-decorated columns of Doulton ware, to a graceful dome of about 30 ft. in height. This structure is almost unique in this country, and is based upon the Indian principle of dome architecture, where the arches of which it is formed are halved into every alternate arch, thereby securing a maximum of strength with a minimum of material. The superstructure is covered with tiles of coloured and glazed terra-cotta, beneath which an external facing of panels of mosaic in the new Silicon ware alternates with stained-glass windows.

The external decorations of the lower portion of the pavilion consist of sixteen elaborate panels of hand-painted tiles, each one of which is a masterpiece, and tells a tale of its own. An exhaustive description of these pictures would involve a labour so considerable, that these pages, magnified an hundredfold, could not contain a tithe of what might be said. They recall the patient experiments of generations of philosophers, artists, and workmen, and lead back even to the mystic ages of the Celestial Empire; they would necessitate an explanation of the vast subject of the rise and progress of the potter's art in all its intricate phases, with biographies of the pioneers who may be said to mark each epoch in its history. Palissy, Della Robbia, Wedgewood, Shon-Sui, all stand forth in living pictures to attest to the skill of their descendants—the Messrs. Doulton. Then we are brought face to face with pictures of old London, the "Bishop's Walk," "High-street, Lambeth," "Lambeth Palace," and, most modern and important of all, "The Albert Embankment," and birthplace of this magnificent pavilion. A brief titular description of the pictures —and, indeed, of the entire exhibit—may be obtained at the pavilion by asking for it. Around the columns, which support the dome, are grouped four small rooms, fitted respectively as a drawing-room, dining-room, library, and boudoir.

The annexed corners of the space occupied by Messrs. Doulton are devoted to the display of their recent inventions and improvements of articles which come more emphatically under the title of health exhibits. Several of the appliances shown are of such great importance that we shall devote our next communication to their consideration. They deal with drain-pipes, sanitary appliances, stoves, and filters; and although the names of these things conjure up a host of disagreeable fancies, we may assure our readers that an account of some of the ins-and-outs of these necessaries will not only be interesting, but of great practical value, and highly delectable, especially when it is borne in mind that they are the outcome of a toil which seeks to cope with the evils of modern civilisation, and to establish sanitary reform.

AN electric light installation has been completed at the Park Pit Ocean Collieries, South Wales. A 6-horse-power engine drives a Crompton-Bürgin self-regulating dynamo. Fifty 20-candle power Swan incandescent lamps are distributed underground and on the surface over the screens, workshops, and engine-houses.

OPTICAL RECREATIONS.

BY A FELLOW OF THE ROYAL ASTRONOMICAL SOCIETY.

(*Continued from p.* 391.)

IN the various devices for popularly illustrating the effects of reflection that we have so far described, we have assumed such reflection to take place from a silvered mirror or mirrors; but the surface of unsilvered glass reflects light too, and does so while other rays are passing through it from any object behind it. This supplies us with the means of producing that effective optical illusion so well known in this country as "Pepper's Ghost." Fig. 9 exhibits a mode by which this can be shown upon a drawing-room table.

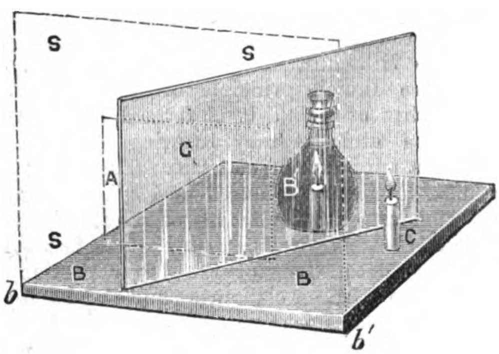


Fig. 9.

B in the figure above represents a slab of inch board, two feet long and one foot wide, across which, in a groove, fits tightly the sheet of carefully-cleaned plate-glass, G, at an angle of 45° with the length of the board. This should be fixed accurately upright. At B', behind it, we place a tall clear glass-bottle full of water, and in front of the glass, at C, a candle, which we shift about until its image, reflected in the glass, seems to fall in the middle of the water-bottle. A millboard screen, S, covered with black paper, and having a central rectangular aperture, A, should, prior to the exhibition, be fastened to the front edge, *b*, *b'* of the board, B, so as to hide the candle. The uninitiated spectator, seated at a suitable distance, and looking through the aperture, A, will imagine that he is viewing a candle burning in the very middle of a bottle-full of water. Numbers who will read these lines must have witnessed the spectral forms produced at the old Polytechnic Institution by a slight modification of this device on a larger scale.

The principle of reflecting rays from plane mirrors, at right angles to their original direction, has been utilised (among other ways) in the observation of an enemy from behind earthworks, where exposure in the embrasures was rendered dangerous by opposing marksmen. A pretty application of it, too, is seen in the capital conjuring trick of the Sphinx, in which a human head delivers oracular responses from a table on which it lies, the table being a tripod one, without any cloth on it, and (seemingly) absolutely void and detached from all surrounding objects, save the floor beneath. Enough, though, has probably been said on the subject of reflection from a plane. Let us now try to ascertain what happens when light falls upon a curved surface. Upon p. 390 we enunciated the law that "when a ray of light is incident upon a reflecting surface, the reflected ray is in the same plane with the incident ray, and the angles of incidence and reflection are equal;" and this we have shown to hold rigidly true where such reflecting surface is a plane. What, though, will happen if, instead of a plane surface, rays of light impinge upon a curved one? Will our law still hold good? It will. Let us take a concave mirror as an example, and track the course of the rays falling upon it from a luminous point in front of it.

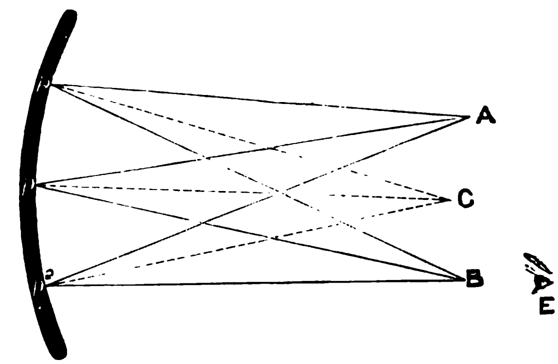


Fig. 10.

We may consider a curved mirror as made up of an infinite number of little planes, which are called, mathematically, its "elements;" and it is quite evident that a radius of curvature, drawn from the centre of that curvature to any point in the mirror, is perpendicular, or square to its surface at that point. Thus, in our figure above, Cp, Cp^1, Cp^2 are all perpendiculars to the mirror at the points p, p^1, and p^2; and, from what has preceded, it will be evident that light reaching the mirror in these lines will be reflected back along them to the centre of curvature C, whence the light must have emanated. Suppose, though, that instead of our luminous object being on the axis of the mirror at C, we remove it to A; let us trace, at an equal distance from the mirror, the course of the rays emanating from it under these conditions. Here, Ap is the incident ray; and C being the perpendicular at its point of incidence, the angle ApC will be the angle of incidence, to which BpC, the angle of reflection, will be equal. In the same way, Ap^1C and Bp^1C are equal, as are Ap^2C, Bp^2C—and so on; so that an eye at E, 10 inches or so from B, will see an image of the luminous object A, accurately reproduced in form, size, and colour at B, though inverted; or such image may be received upon a screen placed at that point. This is technically spoken of as a "Real image," in contradistinction to the "Virtual image" of an object produced by a plane or convex mirror, or by an object within the focus of a concave one. It will serve to test the progress of the student up to this point if he will construct a diagram for himself of a concave mirror, drawing as above radii of its curvature for perpendiculars, and ascertain what will be the course of parallel rays—or those from an infinite distance—falling upon it. He will find, as a matter of fact, that they will all meet at a point halfway between the middle of the mirror's surface and its centre of curvature. This point is called the "principal focus" of the mirror. Moreover, he will see that the rays from a luminous point situated in the principal focus will be reflected from the mirror in lines rigidly parallel to its axis. Concave mirrors are, unfortunately, not very cheap things to buy, and their construction is beyond those for whom these papers are intended. Should, however, the reader fortunately be able to obtain one, he may try the following simple experiments:—Let him first turn the face of his mirror towards a landscape seen through a widow in an otherwise darkened room; by then holding a white cardboard screen in front of the mirror between it and the window (the mirror being just so much inclined that the screen can be placed above or below its axis), he will get a

beautiful little image of the distant landscape on the cardboard, when it is held in the principal focus of the mirror. Measuring accurately the distance between the screen and the centre of the mirror under these circumstances, he will obtain the focal length of the latter. If now at night he will place a small flame in its focal point, he will find a bright circle of light cast on the opposite wall of the exact diameter of the mirror itself; showing obviously that the rays on leaving its surface are parallel. Here, again, is an illustration that rays of light go forth and return by precisely the same route. In the case of the landscape, the parallel rays from it met in the focus of the mirror; in that of the light in such focus, the rays diverging from it were rendered parallel by their reflection from it. The reason of this the student should have no difficulty in seeing if he will only construct the diagram we have recommended him to make above. Now let him look at himself in the mirror, taking care that his face is within its focus. Fig. 11 may suffice to illustrate what will happen.

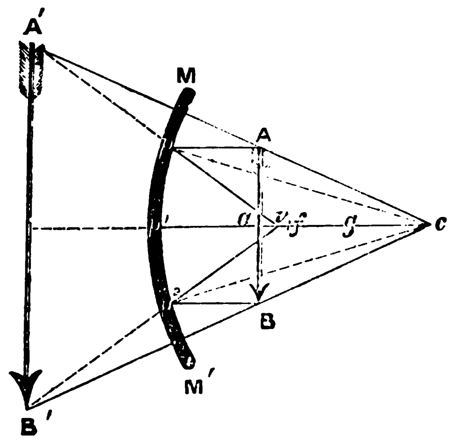


Fig. 11.

If for the reader's face we may be permitted to substitute the arrow A B, placed between the mirror M M′ and its principal focus *f*, then (remembering that, as before, radii of curvature $c\,p$, $c\,p^1$, $c\,p^2$, are perpendicular to the surface of the mirror), evidently the ray A *p* from the top of the arrow will be reflected in the direction *p v*, $a\,p^1$ from the middle of the arrow will return on itself, and B p^2, will be reflected in the direction $p^2\,v$, and will reach the eye (suppose at *a*), as though they proceeded from the very much larger arrow A′ B′ behind the mirror. Hence an observer regarding himself in a concave mirror with his eye within its principal focus, will see a considerably magnified erect (virtual) image of himself. If we further suppose an object to be placed at some point between the focus and the centre of curvature—say at *g* in Fig. 11—a *real* magnified inverted image of it will be formed somewhere on the other side of the centre of curvature, and may be received there on a screen. The truth and reason of this, the student who has attentively followed us thus far and has taken our advice just given about drawing his own diagrams may be fairly expected to now see for himself. Should he possess a concave mirror, he should carefully repeat every experiment of which he has graphically worked out the theory. Finally, it must be evident that if we place an object at more than twice the focal length of the mirror from it, an inverted image of it, real and smaller than the original, will be formed between the focus and the centre of curvature, which is only another way of saying that image and object in our immediately preceding experiment may be made to change places. The images produced by convex mirrors, can, pretty obviously, no more be real than those seen in plane mirrors. Rays falling on them are rendered divergent, or appear to emanate from a much smaller image behind the mirror. These mirrors were at one time common articles of furniture. They are now only seen in old-fashioned homes, and rarely on those wheel-barometers, by the aid of which certain credulous people still fondly believe they can foretell the weather. They (we mean the mirrors, although it is equal applicable to the barometers) are of no practical use. The subject of reflection from cylindrical surfaces would more properly pertain to that branch of our subject dealing with distorted images; but before proceeding to examine into some of the phenomena of refraction, or the bending of the rays of light, we may refer to a simple but very effective experiment, showing the effect of multiplied reflections from the interior of a cylinder.

Get a piece of glass tube, T, rather less than an inch in diameter, and about a foot long; cover the outside of it with a piece of the dead-black paper which we have previously advised the student to obtain, the black side next the glass, and round this paste two or three thicknesses of brown paper, so as to prevent all light penetrating it. Fit a pill-box over one end of the tube, having previously burnt a circular hole, *h*, in the bottom of it with a large

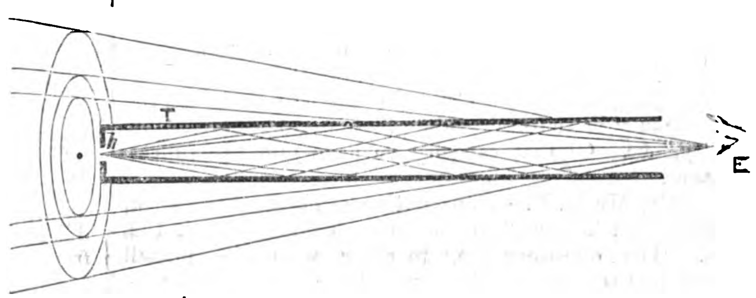


Fig. 12.

knitting-needle, made red hot. This burns the ragged edges of the hole and leaves it smooth and circular. If we now direct the hole towards a bright light, and look down the open end of the hole, we shall see a series of concentric circles of light. The course of the rays as they are reflected from the sides of the tube, until they all meet at the eye, E, is shown in our figure; a very little attention to which will enable the student, who has so far followed us, to see easily how the charming effect he will perceive in making the actual experiment is produced.

(*To be continued.*)

ELECTRICAL EDUCATION.

By W. Slingo.

THE science of electricity has now developed to such an extent that its applications are sufficiently numerous and important to afford lucrative employment to a host of men of all kinds and conditions. If, however, there is any profession that demands a sound scientific education it is that of an electrician. Time was, when to be acquainted with "an apparently incoherent series of facts" enabled the aspirant to claim the title. That time, however, is fast passing away, and the man who now desires to adopt any electrical application as the source of his income must acquaint himself more or less thoroughly with such laws as have so far been found to govern the science. It is not enough to know that a magnet is able to raise a piece of

iron, or that friction results in the production of electricity. He must now know how much iron, and be able to measure the electricity. Impressed with these facts, we some time since found occasion in an article on "Electrical Engineering" (Knowledge, No. 47) to speak somewhat fully of the appliances in use at the School of Telegraphy and Electrical Engineering, Hanover-square. It was there pointed out that the managers of the school were thoroughly awake to the requirements of the time, and had provided themselves with large premises, well stocked with apparatus "up to date." Since that article was penned, many important additions to the stock have been made, more especially in the electric lighting and telephone branches. Further developments are also intended, such as the introduction of mechanical engineering, so far as an electrician's requirements necessitate. For that purpose a room, light and airy, has been set apart. It may here be interesting to quote what was said on this point in No. 47:—"If there is any fault to find with the school itself, it is that it is too electrical and not sufficiently mechanical. We should much have liked to see these embryo engineers doing a little real engineering, making, repairing, and improving all kinds of apparatus, from the magnetic needle upwards." It is not, however, so much to the building and its contents that we would now direct attention as to the "education" itself, that is to say, the system of teaching and its results. Before, we could only speak of the school. We were not sufficiently acquainted with its teaching to refer to it either approvingly or disparagingly. All we could do was to venture a proviso such as "With (or, providing there were) good teaching capacity," &c. Of that capacity we have now the fullest assurance.

Recently, Mr. H. R. Kempe, author of *the* standard work on "Electrical Testing," conducted an examination of the pupils. The questions set, together with the marks awarded and the examiner's reports, have been laid before us, and most certainly show a state of things creditable alike to the school and its pupils. It appears that the system pursued is to place every new entrant into the elementary portion of the school, where his progress is checked day by day. If he fails to satisfy the tutors in this division, he is refused admittance to the advanced section, where the only thing he could do would be to impede the progress of the others. It is, however, a noteworthy fact that only the absolute dullards or positive idlers stay long in the lower class, and it is every teacher's experience that such troublesome pupils find their way into every educational establishment. The field traversed in the elementary section is a considerable one, and embraces the fundamental principles of telegraphy, telephony, electrical measurements, &c.; while the advanced class occupies itself with cable testing, the construction and principles of dynamos, motors, lamps, secondary batteries, &c., and the more advanced systems of electrical measurement. The syllabus is thus a very comprehensive one, and upon it Mr. Kempe based his questions.

We will quote one taken at random from each paper:—Elementary:—"Two batteries A and B are joined in circuit with a tangent galvanometer and a resistance x. When the two batteries assist one another, a deflection of D° is obtained; but when the batteries oppose one another, a deflection of $d°$ is obtained. Battery B is now removed, and the resistance x is altered first to R, so that the deflection $d°$ is reproduced, and then to r, so that the deflection D° is reproduced. Find the value of x in terms of R and r. The two internal resistances and the resistance of the galvanometer to be neglected." Advanced:—"How would you determine the current and difference of potential between the terminals of an electric lamp worked by alternating currents, the resistance of the lamp being unknown?" Mr. Kempe, reporting on the answers of the advanced pupils, says, "I must congratulate you on the most satisfactory replies which have, with few exceptions, been given to the various questions. The result is a highly creditable one to the School, and shows that the course of instruction is very complete." Speaking later of the elementary class, the examiner says:—"I must again express my satisfaction at the general excellence of the replies to the questions, and the credit which they reflect upon the School." There was a third class of new pupils whose examination was scarcely so creditable, but no importance could be attached thereto, because of their very brief training. These extracts will, we trust, speak for themselves, and warrant the eulogies which we have felt it our duty to bestow here, and in the previous article, on the School of Telegraphy and Electrical Engineering. The School is in every sense of the word commendable, and the results accomplished as the outcome of private enterprise undoubtedly present a remarkable contrast to the feeble results attained by certain endowed institutions not very far distant.

MOVED FROM AFAR.*

(*Continued from p. 396.*)

THE SENSATIONS.

Perhaps the transmission of *localised pains* is as purely sensory an instance of telepathy as can well be selected. We have occasionally obtained this phenomenon in the normal state; but it is in the hypnotic state that, though still rare, it is most markedly induced. Take a mesmerised "subject" who is sufficently *en rapport* with his mesmeriser; talk to him on some question which engrosses his attention; and, in the middle of your talk, suddenly pinch (for instance) the mesmeriser's right ear behind the subject's back. The sleep-waker will continue to listen and reply, but his hand will fly to his own right ear, which he will rub with manifest discomfort. Now here is a transferred impression which is as purely *sensory* as we can well obtain, which prompts to an action nearly or quite reflex, and is scarcely present in any conscious manner to the sleep-waker's beclouded intellect.

Now, according to our theory of a close parallelism between the *induced telepathy* of our experiments (on a small scale) and the *spontaneous telepathy* which nature offers on a much larger scale to our examination, we might fairly expect to find some cases where a localised pain has been transferred from one person to another at a distance, unaccompanied by any definite *idea* of the cause or source of the pain thus felt. To give force to an account of this kind, it is plainly important that the pain should be sudden, distinctly localised, and not easily referable to some mere ordinary cause. If Brown were to tell us that he got into such *rapport* with Smith at a friendly Greenwich dinner that, when Smith's head ached the next morning, Brown's ached also out of sympathy, we should hand over both headaches alike to a branch of science better established than our own. But when Louis Blanc feels a shock through one of his arms, as if it had been pierced through with a rapier, at the moment that Charles Blanc's arm is pierced in a duel,† we feel that any ordinary sort of common cause for the two events is excluded. The incident which

* From a paper on Apparitions, in the *Nineteenth Century*, by Messrs. Edm. Gurney and Fred. W. H. Myers.

† See "Memoirs of C. M. Young" (1871), pp. 341, 342.

we shall now quote (occurring to Mr. Arthur Severn, the distinguished landscape-painter, and his wife, and the account of which has been obtained for us through the kindness of Professor Ruskin) presents the requisite characteristics of suddenness, localisation, and unusualness of the pain in a very high degree.

"Brantwood, Coniston, October 27th, 1883.

"I woke up with a start, feeling I had had a hard blow on my mouth, and with a distinct sense that I had been cut, and was bleeding under my upper lip, and seized my pocket-handkerchief, and held it (in a little pushed lump) to the part, as I sat up in bed, and after a few seconds, when I removed it, I was astonished not to see any blood, and only then realised it was impossible anything could have struck me there, as I lay fast asleep in bed, and so I thought it was only a dream!—but I looked at my watch, and saw it was 7, and finding Arthur (my husband) was not in the room, I concluded (rightly) that he must have gone out on the lake for an early sail, as it was so fine.

"I then fell asleep. At breakfast (half-past 9), Arthur came in rather late, and I noticed he rather purposely sat farther away from me than usual, and every now and then put his pocket-handkerchief furtively up to his lip, in the very way I had done. I said, 'Arthur, why are you doing that?' and added a little anxiously, 'I know you've hurt yourself; but I'll tell you why afterwards.' He said, 'Well, when I was sailing, a sudden squall came, throwing the tiller suddenly round, and it struck me a bad blow in the mouth, under the upper lip, and it has been bleeding a good deal and won't stop.' I then said, 'Have you any idea what o'clock it was when it happened?' and he answered, 'It must have been about 7.'

"I then told him what had happened to *me*, much to *his* surprise, and all who were with us at breakfast.

"It happened here about three years ago at Brantwood. —Joan R. Severn."

"Brantwood, Coniston: November 15th, 1883.

"Early one summer's morning, I got up intending to go and sail on the lake. Whether my wife heard me going out of the room I don't know; she probably did, and in a half-dreamy state knew where I was going.

"I was left becalmed for half an hour or so, when, on looking up to the head of the lake, I saw a dark blue line on the water. At first I couldn't make it out, but soon saw that it must be small waves caused by a strong wind coming. I got my boat as ready as I could, in the short time, to receive this gust, but somehow or other she was taken aback, and seemed to spin round when the wind struck her, and in getting out of the way of the boom I got my head in the way of the tiller, which also swung round and gave me a nasty blow in the mouth, cutting my lip rather badly, and having become loose in the rudder it came out and went over-board. With my mouth bleeding, the mainsheet more or less round my neck, and the tiller gone, and the boat in confusion, I could not help smiling to think how suddenly I had been humbled almost to a wreck, just when I thought I was going to be so clever! However, I soon managed to get my tiller, and, with plenty of wind, tacked back to Brantwood, and, making my boat snug in the harbour, walked up to the house, anxious, of course, to hide as much as possible what had happened to my mouth, and, getting another handkerchief, walked into the breakfast-room, and managed to say something about having been out early. In an instant my wife said, 'You don't mean to say you have hurt your mouth?' or words that effect. I then explained what had happened, and was surprised to see some extra interest on her face, and still more surprised when she told me she had started out of her sleep thinking she had received a blow in the mouth; and that it was a few minutes past seven o'clock, and wondered if my accident had happened at the same time; but as I had no watch with me I couldn't tell, though, on comparing notes, it certainly looked as if it had been about the same time.—Arthur Severn."

It is fortunate that in this case the incident was bizarre enough to stamp itself at once cn the memory. For one main difficulty in collecting cases of this sort is that, even if they do occur, they are not likely to be observed or remembered. Their theoretical importance is (very naturally) not discerned; they are thought trivial and purposeless—merely incredible, without either pathos or dignity. In reality, no narratives are more significant, or cast a more searching ray on the obscure pervasive co-sentiency of man and man.

(*To be continued.*)

Editorial Gossip.

A Mr. T. W. Piper, presumably related to the famous Peter, and inheriting some of the effects of undue pepper-picking, picks occasion to tell me, somewhat pointedly, that every reader of Knowledge is sick and tired of my "eternal quarrels" (of which I had not myself heard), and that no one will continue to take in Knowledge, if the personal peculiarities of Mr. Proctor are thus pertinaciously paraded. I have no personal objection one way or the other. If Mr. Piper is right, and he implies that he knows hundreds of readers of Knowledge all without a single exception prepared to pick the same quarrel with its editor, a score or so of others will feel the effect of their secession before (and rather more than) I shall. But that is not my point at present. I have liked to chat with readers as with friends, and Mr. T. W. Piper is one of but about six or seven who have somewhat rudely objected to this departure from customary editorial solemnity. But just now the trouble is that nearly two-score of readers have written to ask me whether certain statements about myself, which have recently appeared in the papers, are correct,—and I am divided in my mind between the objurgations of the peppery Piper on the one hand, and the suave requests of many readers on the other. But for Mr. Piper I should at once, *more meo*, have told "the truth, the whole truth, and nothing but the truth:" but seeing he knows that *every* reader would object to that process, I will simply deny categorically the untrue.

I learn then, to my astonishment, that I am about to settle on my wife's property in Missouri. I learn also, and again the news takes me by surprise, the following particulars of my meeting with that lady. Her name it appears was Mrs. Sadie Crowley, her husband had died in Ireland, and "having fallen in love with Mrs. Crowley on her return journey to America from Ireland," I followed her to Missouri, and in the touching language permissible at half-a-crown per thirty lines, "there wooed and won the fair widow." This is to me as full of interest as a new chapter in Reade's last novel, and for the same reason, it is so completely new to me. Though it may seem sad, I never knew a lady who either in baptismal register or among her friends went by the sweet name of Sadie. Nor have I yet had the pleasure of meeting a Mrs. Crowley, though ladies of that name are I doubt not tolerably numerous. I do not think I ever consciously knew any lady whose husband had died (recently or otherwise) in

Ireland. I did not (it so chances) meet my wife within ten thousand miles of Ireland; and I never had occasion to follow any lady to Missouri, for the pleasing purpose indicated in the papers. Nor am I to the best of my knowledge and belief about to settle on my wife's property in Missouri.

I LEARN lastly and can contradict the statement emphatically, that I have expressed an intention of ending my days in Missouri. I have no present idea of ending my days anywhere in particular. All I can just now announce as settled, in that matter, is that, presumably, I shall end my days somewhere. I hope Mr. Piper will bear with me for obtruding this information upon a perhaps impatient public.

HERE is full copy of a letter (*pace* Piper & Co.) which I have just received :—

West Hartford, Conn., May, 1884.

Richard A. Proctor.

Dear Sir,—Having heard of you through friends, I write to ascertain if you still cast horoscope's (*sic*), and what are your prices for brief delineations, also whole life reading.

Yours respectively (*sic*)

MRS. W. B. OLMSTED.

P.S. Won't you please send me a catalogue or a few testimonials?

THAT "still" goes to my heart. Do you *still* cast horoscopes? Have you left off beating your grandmother? Who can answer "yes" or "no" to such questions?

TECHNICAL EDUCATION.—The report of the Royal Commission on Technical Education, which has just been issued, goes thoroughly into the results of the investigations made by the Commissioners, who assert that England is not so far in the rear of other countries in this matter as many persons have supposed, some of our institutions, having the object of an extension of practical teaching and scientific knowledge, exciting the wonder and admiration of nations where technical schools have long been established.

AMATEUR PHOTOGRAPHY.—Although the art of photography may be said to have been in existence for five-and-forty years (since M. Daguerre and Mr. Fox Talbot both introduced their processes in 1839), but little has been done till recently to popularise what may be regarded as one of the most useful, yet simple, of all the discoveries of science. When one considers the manifold ways in which photography is a necessary adjunct to successful study, it is matter for surprise that the endeavour to introduce that art for the amateur has been so long neglected. And not only to the student, indeed, but to the pleasure-seeker is photography an accomplishment that will well repay the expenditure of time necessary to its acquirement. For instance—now that "leafy June" is here, and the days of pleasure-touring have commenced,—what is there more enjoyable than to be enabled to produce an actual presentment of some enchanting picture painted by Nature herself—"the valleys, hills, and woods in rich array"? How often does one feel the desire to recount the beauties of a favourite scene, yet with the consciousness of the utter inadequacy of verbal description? Here photography steps in. The tourist can by its means easily and quickly preserve for after-contemplation his choicest impressions, and without adding appreciably to his travelling *impedimenta*. For many other reasons is the popularisation of the art of photography a consummation to be desired; and therefore we are glad to notice that much is being done in this direction by the London Stereoscopic Company, who are prepared, not only to supply a complete and remarkably compact set of apparatus for amateurs at a comparatively trifling cost, but also to give, at their premises, 110, Regent-street, W., gratuitous lessons in the art of photography to those desirous of learning. A good feature of these lessons lies in the fact that they are not given in classes, the privacy necessary to the diffident beginner being thus ensured, and proficiency the more readily obtained. By the introduction of the "dry process," the manipulation is greatly simplified, and cleanliness in working is secured. This at once renders the art more acceptable to the non-professional student. The free-instruction system is a decidedly new and liberal departure, which should meet with the success it certainly deserves.

NOAH'S RAINBOW.

[We publish the following letter, by our ingenious and erudite correspondent Mr. E. L. Garbett, without giving any opinion on the views expressed in it, other than this, that one who admits as much doubt as Mr. Garbett does respecting the accuracy of the account of the Flood in Genesis, might well feel free to admit other and wider doubts, even such ideas as Sir George Airy's, that "we cannot doubt the Flood to have been an overflow of the Nile." The Egyptian origin of the story, touched on by Mr. Garbett at the close of his letter, corresponds with my own suggestion that the original story of the Flood was an interpretation of the pictures on some ancient constellation sphere. Just as the whole story of the Fall is found fully recorded in ancient Persian star pictures, so the story of the Flood corresponds with the course of Osiris (the sun) through the watery constellations Capricornus, Aquarius, and Pisces, to conjunction with the ship Argo (the Ark), and the long sea-line indicated by Hydra, and so past Chiron the Centaur, (identified of old with Noah), represented as offering sacrifice on the altar (Ara),—

Ara ferens thuris, stellis imitantibus, ignem,

while above, in the smoke of incense was seen the Bow of Sagittarius. All these figures were seen in the stellar heavens (in fact, few of the constellation figurings are more easily recognised among the stars than the streams from the water-cans of Aquarius, the shape of the Poop of Argo, the long sea-snake (with the Raven on its back), the Altar and the Bow; and it is fully in accordance with what has repeatedly happened in such cases, that pictures associated with these imagined figures should afterwards have a story attached to them, and that this story should be very widely spread. It is at least remarkable that the story of the Flood corresponds closely, as to time intervals with the extension of the constellations named beside the course of the sun. It is also noteworthy that the Egyptian account, which is undoubtedly the oldest (older even than the Assyrian) is associated with what is unquestionably a solar myth.—R. P.]

FEW, I suppose, have ever doubted that the bow Noah considered set in the cloud for a covenant was a natural rainbow. Moreover, nothing in the story necessitates us to suppose either he or his young disciples were not already familiar with the sight. Nevertheless, I for one have always thought it likely that they, or even their aged chief, now beheld it for the first time; which is a widely different idea from that deduced and taught, I have heard, by some mediæval divines, of its being the *first rainbow;* though you will perceive, I hope, that the moral, spiritual, or historical effect would be exactly the same. If, as I suppose, none of the eight human spectators (or none except the eldest) had seen such a thing before, the impressiveness would be just as great and memorable as if the thing were then first created.

Now there are large regions wherein natives, if not travelling beyond them, have practically no chance of seeing a rainbow, even in the longest life. To say nothing of the rainless ones (not all deserts, but comprising the fertile thousand miles of the middle Nile) all tropical islands, and perhaps most parts of the tropical continents, receive all their rain in the midday hours or by night. No rainbow is possible with a sun higher than 42°, or noticeable as a phenomenon when he is higher than 30°. To produce any in equatorial latitudes then, there must be rain suddenly leaving a spot, within the two hours after sunrise or before sunset. I doubt if this ever happens within the range of the trade-winds. Accordingly, I never met with a Jamaica negro who had seen a rainbow, or had more conception of it than of snow.

Now the civilized art of those nearest in time and place to Noah, the Assyrians, gives this decisive mark of coming from an ancestry much nearer the equator. They never represent the horned moon otherwise than quite on her back, the horns on one horizontal line. This is how no northern artists place the moon; for in fact it is not possible five degrees outside the tropics (nor except by a rare chance outside them at all) for the moon to be so seen; whereas it is her regular aspect at all hours to everyone near the equator.

Next observe that we are absolutely driven by the diluvial monuments, drift-gravel, hundred-fathom land-relics, &c., to place Noah's youth, and his progenitors, if any, in *our* "glacial age,"—in times when, as Professor Dawkins infers from the dredgings, the general sea-level was full 100 fathoms lower than now, and the lands we now inhabit were bleak icy uplands, where a thinly scattered flint-folk, like modern Esquimo, hunted the reindeer, bear, mammoth, and woolly rhinoceros. This nowise implies absence elsewhere of temperate, rich, or even hot regions, or of the highest civilizations therein. Letting alone the tropics, there were all our present shallow sea-beds, then veritable plains; the English and Irish channels, and the whole space between us and Denmark, to a north coast running from Jutland to the Orkneys. But of the Mediterranean, not only the parts thus shallow (including the

whole Adriatic and Ægean) but, as Dawkins observes, vast lower slopes, down and yet down, to even 2,000 feet lower than those, were dry. The Gibraltar lip was not yet overflowed. The ocean was not high enough to enter there, nor yet to enter the Red Sea, nor the Persian Gulf. Those were valleys below its level, but each draining into its own evaporating lake, as the little Jordan valley, or those of the Caspian and Aral do now. Of course the Black Sea was another such vale, that of Marmora another, and the two depths of the east and west Mediterranean two greater ones. In Gen. i. we read, "the gatherings of the waters called He seas," a plural phrase hardly applicable to the present world, wherein the Caspian and Aral are the biggest apart from the outer ocean; but which perfectly applied when the gatherings of all those several basins, each except that of Marmora, was a sea comparable to the Caspian. All these were at various levels (below that ocean which was 600 feet lower than at present) and thus the shores of most were lower than, and probably nearly as warm as, the hot palm-groves of Jericho. Though all present Europe were glacial then, there were for civilization these vast lowlands, besides all the tropical regions, low and high.

Now the drift phenomena everywhere compel us to see that whatever event ended that "glacial age," and introduced the present, it was but about fifty centuries ago, and yet was something prodigiously violent and sudden; more so, indeed, than divines used to gather from Noah's story, or, perhaps, that any reader would gather from that very quiet prose narrative alone. The lately-found Assyrian tablet, as interpreted by G. Smith, where the destroying goddess comes thundering with sudden darkness, and the other deities, planets, have to take refuge beyond Saturn; the Hindoo accounts that make the patriarch and his seven companions in the vessel stand aghast in utter darkness a whole night of Brahma; the Zend story that the downpour was such as to cover even hillsides to the height of a man; even the Talmudic, that says single drops were big enough to slay animals, and boiling hot,—all these hints supplementing but not clashing with Genesis, seem to me to assist us to a better conception of what could explain the visible drift geology. So, too, may the single remark of Christ, that "the day that Noë entered the ark," [not the month, but the very day] "the flood came and took them all away."

Consider the gravel-bed whereon most of London stands (nearly all its buildings prior to this reign, but comparatively little of the new suburbs.) The stones hereof are by no means pebbles, as the eocene ones of Blackheath, or those of any sea-beach. Most river stones, even of the quiet Thames, are more rounded, and all that the sea has rolled but for a few days. These have hollows untouched, edges merely blunted, are all called by some the "sub-angular drift." Very few days' rolling can they have had; and yet have all come at least the thirty or forty miles from the Chilterns, and many from the midland oolite hills, twice as far. Even those, much softer than the flints, and travelled twice as far, were not fully pebbled. What could bring stones in such quantity, so far, in so little time? The question applies equally to all valley gravels remote from glaciers, or where glaciers have plainly not been. They were once probably moraine, but since that the agent that swept them to where they are, and in a few days, must have been tremendous.

Every vale, however, on the globe that is not in new volcanic hills, testifies by its sculpture, if not by gravels like these, that its stream was once swollen, for a little time, not long, to many—shall we say hundreds, thousands,—no, to *many myriads* of times its utmost modern volume. This has happened once to the Thames, once to the Wandle, Brent, Colne, and each of its other tributaries. Was it at different times to each, I wonder? or simultaneously to them all? And if simultaneously, was there another date for the Trent and all its tributaries, and another for the Rhine system, another for that of the Ganges, and another for the Mississippi? Something may be learnt from what Lyell records of borings in the deltas of the two latter. The depth of impalpable alluvial silt, since their highest gravelly layer, compared with the regular centurial rate of each river's deposit, shows this process to have gone on about fifty centuries. It is the same with every delta examined, even down to the miniature ones of brooks entering lakes. At that particular date, every stream's present habit began. Then, and never since, did each great continental river roll down coarse gravel, hundreds of miles or leagues further than it can ever now convey more than the finest silt.

This event, the last world-wide one, cannot be described as less than the fall of a fresh sea from the sky—a single storm wherein enough water fell to raise the general ocean-level some 600 feet; to bring it up to our present coast-lines, from that old coast-line where, Prof. Dawkins says, all land relics of the glacial ages are dredged. The nearer sea-beds, all between our coast and that former one, were the lowlands inhabited by the cultured or rich contemporaries of our poor "flint folk," but retain no relics of them, because "the day that Noë entered the ark, the flood came," and swept them all away! Still more did it of course clear and scour down the lower lands around the inland seas, which the ocean, raised enough to pour through the Gibraltar Strait, now drowned and made the Mediterranean. It still has to inflow briskly, we know, to supply the evaporation; and there may be grounds to doubt whether to this day the Mediterranean be quite full, by some few inches.

The tremendous biological effects have been by nobody more appreciated, perhaps, than by the late C. Darwin, especially (but not only) while young, before becoming Selection Darwin, or Ascidian, or whatever name his later extravagances may earn. Of the extinct South American mammals (including the identical horse that Europeans have re-introduced!) he said in his "Voyage of the *Beagle*," they "lived at a late period, and were the contemporaries of the existing sea-shells. Since they lived, no very great change in the form of the land can have taken place. What, then, has exterminated so many species and whole genera? The mind at first is irresistibly hurried into the belief of some great catastrophe; but thus to destroy animals, both large and small, in Southern Patagonia, in Brazil, on the cordillera of Peru, in North America, up to Behring Straits, we must shake the entire framework of the globe." (By no means necessary.) "It appears from the character of the fossils in Europe, Asia, Australia, and in North and South America, that those conditions which favour the life of the *larger* quadrupeds were lately co-extensive with the world: what those conditions were, no one has yet even conjectured. It could hardly have been a change of temperature which, at about the same time, destroyed the inhabitants of tropical, temperate, and arctic latitudes, on both sides of the globe. In North America, we positively know from Mr. Lyell, that the large quadrupeds lived subsequently to that period when boulders were brought into latitudes at which icebergs now never arrive. From conclusive but indirect reasons, we may feel sure that, in the southern hemisphere, the *Macrauchenia* also lived long subsequently to the ice-transported boulder period. Did man, after his first inroad into South America destroy, as has been suggested, the unwieldy *Megatherium* and the other Edentata? We must at least look to some other cause for the extinction of the little *Tucutuco*, and of the numerous fossil *mice*, and other small quadrupeds. No one will imagine that a drought, even far severer than those which cause such losses in the provinces of La Plata, could destroy every individual of every species, from Southern Patagonia to Behring Straits. What shall we say of the extinction of the *Horse?* Did those plains fail of pasture, which have since been overrun by thousands and hundreds of thousands of the descendants of the stock introduced by the Spaniards? Have the subsequently introduced species consumed the food of the great antecedent races? Can we believe the *Capybara* has taken the food of the *Toxodon*, the *Gnanaco* that of the *Macrauchenia*, the existing small Edentates of their numerous gigantic prototypes? Certainly no fact in the long history of the world is so startling as the wide and repeated exterminations of its inhabitants."—"Journal of Researches," 1845, c. viii.

Equally insoluble he found afterwards, in the famous book so oddly misnamed "Origin of Species," at least two world-wide facts of the present life-distribution. Our planet has, for organisms, three mansions—land, sea, and fresh water. Now the sea is practically all connected; and so is a majority of the land in one mass, or even its whole main bulk, all but the islands, in three masses. But the fresh waters are in thousands of disconnected basins, and the very largest hardly perhaps a tithe of the whole. Creatures that cannot leave fresh water can no more reach the Thames from the Trent than reach another planet. "We might expect then," said Darwin, "that the existence of ubiquitous tribes, or even very wide-spread ones, would be common only in the ocean, or the air-breathers of the dry land; and that the fresh-waters would be characterized by the absence of these, and prevalence of localized species peculiar to their own small districts. But the case is exactly the reverse. Not only have many fresh-water species an enormous range, but allied species prevail in a remarkable manner throughout the globe. When first collecting in the fresh waters of Brazil, I well remember feeling much surprize at the similarity of the freshwater insects, shells, &c., and at the dissimilarity of the surrounding terrestrial beings, compared with those of Britain." He takes all the rest of the chapter in illustrating the extreme difficulty of accounting for any migration, even the smallest, of freshwater beings from one river to another,—the elaborate ingenuity he needed to make it at all possible; which of course is not even touching his problem, why the freshwater mansion should be the *most* uniformly stocked, instead of the *least* so! According to Genesis, however, as the diluvial waters fell from heaven, they were fresh; and so all the fresh waters of the globe

have been in temporary connexion *more recently* than the lands, or even seas of different climates.

His other grand puzzle was about oceanic islands; but first it seems that, before immigrants brought mammals, no such island contained any, except in some cases bats. These, he remarks, could fly thither, but other mammals could not naturally be conveyed from the continents. Now, as he insists that many islands are as ancient as the continents, we must ask, on his principle of "*evolution*," why has there not been time for as many or as fit species to be developed on each of those islands as on the continent itself? Why talk of the difficulty of any getting *to* the island *from* the continent? Why more need of migration to island than to continent?

But the grand fact was that, in each island, the few species native there, and even the still fewer, in some cases, peculiar thereto, are so far from being the fittest to it that the world contains, that these insular species, animal or plant, are often quickly exterminated by the competition of some mere come-by-chance from a continent. "He who admits the doctrine of the creation of each separate species, will have to admit that a sufficient number of the best adapted plants and animals were not created for oceanic islands; for man has unintentionally stocked them far more fully and perfectly than did nature." What is this but denying that his principle of natural selection has operated on islands at all? But why has it not? No shadow of a reason appears. But Genesis explains perfectly. The few species on each island from Noah's time till it is colonized by ships, have no fitness, because mere accident threw them there. They are simply those, fit or unfit, that, in the rush of the deluge, happened to be stranded thereon. It is the rule for most of these ultimately to be superseded by some that our ships introduce; each island thus repeating in small the marvel of those American horse-pastures, where the fossils show that the Horse swarmed with the Mastodon and Megatherium, as it swarms again now, but between the ages of Noah and Columbus was unknown.

I submit that nothing has the least explained the drift and valley phenomena, but the old ubiquitous tradition that, on a certain day, a general cataract descended. It was a work of but minutes for all timber, vegetation, and human structures to be uprooted and afloat, with all the matted forests of the tropics, all glaciers, and the ice of both poles. Now wherever such a vessel as Noah's might be built, the effect would plainly be to hurry it, at railway speed, along the directest waterways to the ocean. In whatever continent it was, if at all central, its course and issue to sea would be by that continent's present chief river mouth, or one of the chief. And once at sea, we can hardly conceive a chance of its ever grounding, unless on some newly upheaved volcanic shoal, providentially stopping it when its momentum was just about spent. Can we find any of the few great continental streams with a rather straight issue to sea, and in the direction of which issue, continued some few hundreds of miles, recent volcanic land appears? Only one great river and one post-tertiary volcano can be found, I believe, thus related; and the situation of the latter is most unique, in the very heart of the old world, overtopping all mountain chains within 1,000 miles, and almost every point of them, yet not itself belonging to any, and the centre of such a circle as I proposed the enigma of finding, but which none of your readers has attempted, an unequalled circular area, chiefly of land, yet which no fresh water flows *from*, but many rivers from all the three continents enter it, and terminate in inland seas within it, seas that, though inland, mostly have connection with the remote ocean.

There is no upheaved mass but historical ones, like the Jorullo of last century, that geologists venture to pronounce newer than Ararat. It had an eruption as lately as 1840, destroying many lives, a monastery and a village, traditionally the site of Noah's first vineyard. In Johnston's Atlas, it is coloured as the very largest patch of the newest sort of volcanic land. Now the chief of those rivers running so strangely towards it, is the longest of Africa and most famous of history. We are almost compelled to infer that, if Noah's story have any foundation, his original country was on the upper Nile. From any of its upper reaches or tributaries anything movable would, by the "palœozoic flood" (as the drift catastrophe has lately had to be named) be swept in very few days along that stream, out of its old eastern mouth, over Pelusium, and so north-eastward, with gradually slackened speed, till gently stopped by whatever part of northern Syria or Armenia first emerged. "*Emerged*" is a word I think applicable to that region, but to no other; because the tremendous downpour (averaging some 400 feet universally) though not causing disturbance of the earth's crust elsewhere, was most likely to do so around the newly filled basins of the inner Mediterranean, the additional weight now accumulated on them, the Euxine and Levant, as also in the enclosed Caspian basin, being (according to Dawkins) five or six-fold what was added to the oceans generally. This would be likely to produce in Western Asia, if *nowhere else*, depression and submergence of those countries, which I suppose to have been some months rising again.

Away from that region, here and generally, everything indicates that "all the high hills under the whole heaven were covered," but not, as vulgarly fancied, with *standing*, but with *running* water; which in a few days scoured their rocks bare, swept off to sea all movables lighter than boulders (many of which were carried miles) scooped new valleys, and filled the hundreds of bone-caves. But Buckland and Cuvier certainly erred in calling it "the birthday of our continents." It was only a baptism, and not generally one of immersion; only that, I hold, for the region round Ararat. There, as Noah recorded, when grounded, on Easter Day, the 150th he had been afloat, "all the high hills that were under the whole heaven" remained still covered *from him*, as they are now from any ship in mid-ocean. Meanwhile plenty of remote lands, perhaps the majority of those beyond his ken, were reclothing themselves with whatever seedlings the vast flotillas of matted forest happened to bring them; and many a small animal may have clung thereto and sustained life, though "all in whose nostrils was the breath of life, of all *in the dry land* died."

There are many caves however, the habitations either of day birds by night, or of bats and night birds by day, and any such cave whose entrance has no descent, might, I suppose, preserve some of its inmates if they found food. The reclothing of some lands we know proceeded so (under what must have been the hottest summer they had known for ages) that, on the 198th day from the catastrophe, a day afterwards to be known as Pentecost, an olive-leaf was brought to Noah by one of his birds. Why Jews now keep their Feast of Weeks the 51st day from their Passover, when the Bible seems to make it the 50th, I know not. But certainly the memorable Pentecost of Acts ii. cannot have been the 52nd, like our Whitsunday. If it was, as Exodus seems to require, the previous Friday, that would correspond to Noah's dove's return.

But when the surrounding lowlands, with wreckage of a drowned world, were to emerge under that broiling summer, it was essential to our ancestors to have purer atmosphere. They were kept imprisoned then, waiting the order to disembark, while the shoal that had stopped them, at our present sea-level, by repeated little shocks and heaves ("the waters were going and returning" says the Hebrew) gradually hoisted them above the clouds. The whole upheaval was little faster than that of the new Aleutian isle in this century, and no single hoist may have approached that of the Monte Nuovo in one night, far less that of Jorullo. Of course such a mountain may have changed considerably since, but it is plain that Noah never knew of a higher than it was when he descended. He could not otherwise have held that all hills had been covered. He probably believed they had been with *standing* water, and so may all have been that he afterwards knew. And all on the globe, including his native ones, had been so with *running* water.

Now if he came from the upper Nile, whether Abyssinia or the rainless parts, it was from lands of no rainbow practically. To his family, if not himself, that seen on Ararat would be the first. But there is much traditional ground for holding the above was his voyage. Ham, afterwards worshipped as Jupiter Ammon, was held to have founded Thebes, whose name is the very one Genesis gives the ark; and down to Plutarch's time divine honours were paid there to a gilded ship 250 cubits long. Was not Ham's the very family to begin such worship, at the place where he either knew or believed that he had worked at his leader's ark? But further, their legend of Osiris, annually celebrated, was that the corpse of that symbolic person, representing humanity destroyed and revived, was launched in a boat down the Nile, when the sun is in Scorpio and the seventeenth day of the moon (the very month and day of the deluge), and being carried out of its eastern mouth, grounded on Northern Syria, there to be cut in pieces that repeopled the world.

E. L. Garbett.

International Health Exhibition.—We are reminded that this is not the first Health Exhibition held in London, for one was opened last June, under the auspices of the National Health Society, at Humphrey's Hall, Knightsbridge. That Exhibition, though small, was a great success, and no doubt suggested the idea of the larger and more comprehensive one now at South Kensington. The National Health Society is represented at Kensington by a modest stall in the Eastern Quadrant, near the Conservatory, where cheap books and leaflets of a popular description are displayed. They relate to ventilation, house sanitation, prevention of the spread of fever, the management of infants, food, cookery, &c. A conference on school hygiene will be held at the Exhibition in August by the National Health Society. The meetings in relation to this conference are being held at the society's rooms, 44, Berners-street, W.

"Let Knowledge grow from more to more."—ALFRED TENNYSON.

Only a small proportion of Letters received can possibly be inserted. Correspondents must not be offended, therefore, should their letters not appear.

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THE RECENT EARTHQUAKE.

[1297]—The shock of the earthquake was experienced very strongly and distinctly by the occupants of one of the top flats (No. 37) of Oxford Mansion.

The water to this mansion is supplied from an artesian well, and for several days after the earthquake the water was considerably mixed with a white earth, of chalky appearance. Being quite unfit for any purpose whatever, men were sent for to find out what was wrong, but all their examinations were useless—the water was simply composed of half chalk. Suddenly it righted itself just as mysteriously as it went wrong. C. L. H. WALLACE.

CO-INCIDENCES.

[1298]—The correct answer to the question in Letter 1283 (KNOWLEDGE, No. 135), is "Viscountess Beaconsfield," of whom all the assertions contained in that letter are literally true. It is, perhaps, not necessary to observe that Mr. Disraeli was not raised to the peerage till some time after the death of his wife.—Yours, L. E. B.

[1299]—I was much struck by the curious coincidence forming the subject of letter 1283. I saw at once that the person described could not be George Eliot, as she was said to have "married" a gentleman of the name of Lewis (not Lewes); but I was completely at sea as to who else could suit the other points of the description. Doubtless many other readers of KNOWLEDGE had exactly the same experience, but mark what follows. I had carried home with me from a public library Foster's "Members of Parliament for Scotland, 1357-1882," which I had borrowed with the view of ascertaining the representatives in the seventeenth century of a certain Scotch burgh. Laying down KNOWLEDGE, I took up this book, and on opening it, my attention was at once caught by an advertisement (inserted at the beginning) of the same author's "Collectanea Genealogica," including a specimen page. This page contained the descent of the Disraeli family, and actually the first words that caught my eye were:—

"Benjamin, Earl of Beaconsfield = Mary Anne, Viscountess Beaconsfield in her own right, so created 30 Nov., 1868; widow of Wyndham Lewis, esq., M.P., of Pantwynglass Castle, co. Glam., and only dau. of Capt. John Viney Evans, R.N., of Bampford Speke, Devon, m. 28 August, 1839, d. 15 Dec., 1872, bd. at Hughenden, M.I., aged 76.

Of course, in the text of Foster's "Members of Parliament for Scotland" there is no reference whatsoever to Disraeli. P. J. A.

MAN'S BRAIN, &c.

[1299] Will you kindly permit me to ask two questions of Mr. Edward Clodd?

(1) In KNOWLEDGE for to-day (May 30) he states, under the heading "Dreams":—

"The cranial capacity of the modern Englishman surpasses that of the aboriginal non-Aryan Hindu by a difference of sixty-eight cubic inches, while between this Hindu skull and the skull of the gorilla the difference in capacity is but eleven inches."

Will Mr. Clodd kindly state his authority for this fact?

(2) He also declares it a "fond delusion" to "place man in a kingdom by himself and deny his pre-human ancestry."

Is not such "fond delusion" the deliberate judgment, based on adduced evidence, of Professors Owen, Carpenter, and de Mivart, Alfred Wallace, and J. W. Dawson?—Yours, &c.,

S. STRINGER BATES.

LETTERS RECEIVED.

S. The binocular microscope gives a pleasant form of vision with low powers; but its value rapidly diminishes with higher ones. Every notable discovery hitherto made has been with the monocular form of instrument.—W. GOWINGS, T. A. E. SANDERSON, and others. Thanks for all the trouble you have taken, but see Mr. Dodgson's letter.—A. J. W. Your reminiscences of George Eliot extend to rather too great a length, in the terribly overcrowded condition of our pages; a remark which extends to the "coincidence" of meeting your sister's former *fiancé*.—W. O. AINSWORTH. If I could only spare you half a number of KNOWLEDGE to yourself! Your "Nonsense attached to the Flat Earth Theory," would, doubtless, make Parallax tear his hair, and drive Mr. Hampden into an asylum; but——.—W. HUME suggests that "Phoro" (letter 1245), should exercise his hand; and tells a story of a cat that imitated a bird's note. (This was probably for a-mews-meant.)—ENGLISHMAN. I suppose that German gardeners work for less than our own countrymen.—O. B. Ob-viously so.—H. W. CLARKE. I know of no one who gives instruction in practical astronomy. Loomis's work on that subject is most explicit; and nothing but actual practice with the instruments described in it will make you expert in their use.—C. H. C. The *South London Press* simply stated "the thing which is not" in the farrago which you quote. "Mr. Richard Proctor" never "assigned fifteen years"—nor fifteen hundred, nor fifteen thousand years—"as the residuary term of human existence in our sphere." He has an abiding conviction that the world will last at least as long as the newspaper which you quote.—JOHN F. WILKINSON. I do not know what books are read for the B.A. Exam. of the London University, but Godfray's "Astronomy" (a Cambridge text-book) ought to enable you to answer any question in the least likely to be put.—R. H. R. If you reflect for a moment that the heavens seem to rotate round an axis inclined to the horizon, you will see at once that what is the top, or north point of the sun when he is on the meridian will appear to be on one side of him as he nears the horizon. Get a celestial globe, elevate it to the latitude of Eastbourne (about 51°), cut a circular disc out of note-paper, and stick it on to the globe. Bisect this by the brass meridian and make a pencil-mark at the top of it. Now turn the globe round its axis until your disc is some hours east or west of the meridian, and note in what direction your pencil-mark lies. Or when the moon is nearly full, hang a plumb line in front of her, and note where it cuts her limb. Do the same thing four or five hours afterwards, and observe how differently she seems to lie with reference to your line.—JAMES P. CALDERON. Less than nothing is known as to the *cause* of gravitation. No more information on the subject was given, because there was none to give.—JNO. S. HEPBURN claims to have invented a telegraph cable in which kinking is rendered impossible by swivel couplings.—MRS. C. L. H. WALLACE thinks that "very valuable vegetable victuals" ("Spell it with a 'We,' Samivel") have been spoken of too slightingly in these columns. I regret if this is so; but meanwhile shall, I am afraid, continue to flavour my asparagus with some lamb, take duckling as an occasional adjunct to peas, and relieve the monotony of spinach and potatoes with a modicum of beef and mutton.—H. A. B. I believe that it was the heliacal rising of Procyon—the "Canicula" of the Romans—which determined their dog-days. These lasted from July 3, to Aug. 11; dates still retained in some of our almanacs; albeit it is the end of August now ere Procyon rises with the sun.—THOS. FIELDING. See if you can pick up Braid's "Magic, Witchcraft, Animal Magnetism, &c." in any public library, or at a second-hand scientific bookseller's.—J. There can be no objection to the adjective you employ as describing the book of which you speak; but it is equally applicable to Plato, to Shakespeare, Newton, and others their fellows. How can you possibly prove a miracle by documentary evidence? —WILLIAM CLARK. C. F. Hodgson & Son, Gough-square, Fleet-street, publish the works on the Hamiltonian system.—J. C. MURRAY AYNSLEY. You appear to be unable to discriminate between what is absolutely opposed to irrefragably established scientific facts, and what is, so far, unexplained. Granted that the "homing" faculty of birds (by no means so infallible though as you assume) has not yet been quite unriddled; what argument does that supply for the existence of death wraiths? I don't see the sequence myself.—R. The earth is an oblate spheroid; *i.e.*, it is flattened at the poles, and more convex at the equator. Consequently the polar region must form an arc of a larger circle than the equatorial one, and

they quite obviously could not be struck from the same centre, the polar *radius* being greater (although the polar axis is the less) of the two. But then a degree is the 360th part of a circle, and the 360th part of a big circle is greater than the 360th part of a less one, Q. E. D.—H. Marfleet. You are very kind indeed; but neither Mr. Slack nor Mr. Mattieu Williams seems to care about having your two bottles of mouldy fruit. — C. Hurst. Your very plain and practical ethical conclusion that "things are only in their own nature bad in so far as they are injurious," should commend itself to popular acceptance. — Reginald N. Rogers. The phenomenon you describe is one of the most familiar ones possible to all navigators and residents by the seaside. I have seen it at Worthing over and over again. It is, as you say, a kind of mirage, and has its origin in the unequal heating of the strata of air immediately above the surface of the sea.—John Bell. The only feasible explanation that occurs to me is that your uncanny windows were glazed with very inferior and bent glass, and that as you varied the angle at which a line from your eye met the crooked panes, part of them reflected the light of the sky and part of them did not. *Re* the so-called "muscle-reading." A modification of it has been a fashionable game in hundreds of drawing-rooms for a long time.—W. I sincerely regret that the great length of your investigation of the question put in letter 1274, compels me to exclude your pretty proof of the general rule. If you could only see the piles of communications lying before me as I write!—J. W. The picture given was remarkable for the singularly rich display of *faculæ*. In your picture there was nothing unusual; but assuredly no offence was intended, as you seem to surmise. I acted according to my knowledge of what is or is not likely to be interesting.—H. A. B. *Real* origin of the term Dog Days not known. Probably name was given at a time when sun and Sirius were in conjunction at the time of year to which the term is now applied.—Diogenes. Am very much of your mind.—William Clark. Regret that space will not permit insertion of your rather too elaborate demonstration.—Pro bono publico. There is no real resemblance between the two systems. Were not the terms of your letter purposely offensive, I would have answered at greater length.—H. H. Tankard. See advertisement on last page of each number.—T. W. P. Eternal quarrels! My dear sir, what *are* you thinking of? I have not been engaged in anything of the nature of a quarrel for years. There are not ten men in the world, I believe, who even imagine that I entertain feelings of hostility towards them; and the few who do are mostly mistaken. Possibly if there were many who wrote as—well as *crisply*—as you do, some quarrelling might arise. And yet, I know not: it takes two to make a quarrel. To turn to the sensible part of your letter,—it has been by no means my fault if the papers whose style and quality you so justly admire have appeared lately at longer intervals than before. Matter cannot be set up before it is received, nor engravings prepared without the necessary "copy." On another point,—if I were to draw up, for you a short and true account of the sums respectively received from Knowledge by the "others" whose cause you take up, and by the "one" who has, you think, taken the lion's share (he has of the *work*, I can assure you), you would probably change your tone a little. I could point to numbers which brought him, for all his work, scarce anything at all. —A Factory Hand, Enfield. I cannot help you; I can give you a reason; but what can you do with it, having so little understanding?—T. H. I scarcely needed your long letter to know how you view the books to which you refer. I view them quite differently, and regard them as quite unworthy of the attention you wish me to give them.—H. Goldziher. The question is one of nomenclature. In the words, billion, trillion, quadrillion, and so forth, we have the numbers two, three, four, and so forth: the idea conveyed is that a billion represents some number to the *second* power, a billion the same number to the *third* power, a quadrillion the same number to the *fourth* power, and so on. Accordingly our English usage gives:—

$$\begin{aligned}\text{A billion} &= (1{,}000{,}000)^2\\ \text{A trillion} &= (1{,}000{,}000)^3\\ \text{A quadrillion} &= (1{,}000{,}000)^4\end{aligned}$$

and so on. The only way of working in the numbers 2, 3, 4, &c., in connection with the American system is thus

$$\begin{aligned}\text{A billion} &= (1000) \times (1000)^2\\ \text{A trillion} &= (1000) \times (1000)^3\\ \text{A quadrillion} &= (1000) \times (1000)^4\end{aligned}$$

and so forth. Your plan which would make a billion equal to a million million, a trillion equal to a billion billion, a quadrillion equal to a trillion trillion, and so forth, seems hopelessly to lose the effect of the bi-, tri-, quadri-, &c., in the compound words; for you have

$$\begin{aligned}\text{A billion} &= (1{,}000{,}000)^2\\ \text{A trillion} &= (1{,}000{,}000)^4\\ \text{A quadrillion} &= (1{,}000{,}000)^8\end{aligned}$$

and so on.—F. M. Billings. The work of which you speak as "recently published" was stereotyped in 1877! I certainly found nothing in Mr. Clissold's remarks to cause me to have the plates altered. The statements with which I dealt were taken directly from a work published by Swedenborgians, and bearing their imprint. I believe now as I believed when I wrote the article, that Swedenborg was misled by wild and visionary dreams, the offspring of a partly-diseased imagination, stirred by such astronomical facts as were known to him and such astronomical ideas as were prevalent in his day. It is a reasonable and, withal, a kindly interpretation. Mr. Clissold maintained a different opinion, on grounds which seem to him sufficient, to me otherwise. I really am not aware of any law of "courtesy" requiring me to insist on the flaws, or what seem to me such, in criticisms made on my works. As an illustration of my position, consider your remark that "Mr. Clissold proves" (*sic*) "that unless Swedenborg had suspected the existence of Neptune he could neither desire nor obtain intercourse with spirits therefrom," "and yet" you say, I "persist in" my "objection" that if knowledge were obtainable by Swedenborg's vision method he might have been expected to tell us something about undiscovered planets. Mr. Clissold's proof is no proof for me. He believes in Swedenborg's imagined power of communicating with spirits from other planets, and believes in such limitations as are absolutely essential if that belief is to have any logical stability; I neither believe that Swedenborg had any such power, nor that Swedenborg himself recognised any limits to the power he supposed he possessed. Interpretations put by Mr. Clissold on the words which Swedenborg himself wrote, are simply valueless for one who prefers to take those words as they stand. You can by suitable interpretations show that Swedenborg knew all that modern science has discovered, just as Mr. Kinns by suitable misinterpretations has done for still older writings, or as Professor Symth has done for the Great Pyramid. But if these interpretations are simply rejected—as they must be by every one who looks on them without prejudice—they count for nothing.

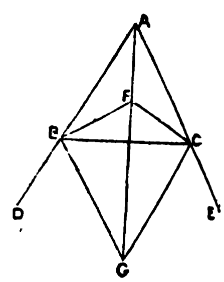
Our Mathematical Column.

NOTES ON EUCLID'S FIRST BOOK.

By Richard A. Proctor.

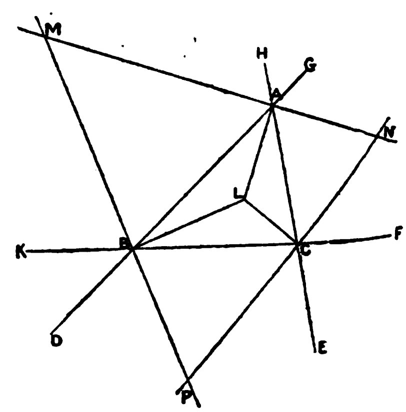
(*Continued from p. 403.*)

Prop. XXXIII.—*In the triangle* A B C, *let* B F, C F *be the bisectors of the angles* A B C, A C B; B G, C G *the bisectors of the exterior angles* D B C, B C E: *then shall the points* A, F, *and* G *lie in a right line.*



For by Prop. XXX. the point F lies on the bisector of the angle B A C, and by Prop. XXXII. the point G lies on the same bisector. Hence the points A, F, and G lie in one straight line.

Prop. XXXIV.—*Let the sides* A B, B C, C A *of the triangle* B A C *be severally produced both ways, to* D, E, F, G, H, *and* K, *and let* A L, B L, *and* C L *be the bisectors of the angles* B A C, C B A, *and* A C B: *then the lines* M A N, M B P *and* P C N, *drawn at right angles to* A L, B L, *and* C L, *shall bisect the angles* H A B *and* G A C, A B K *and* D B C, B C E *and* A C F.



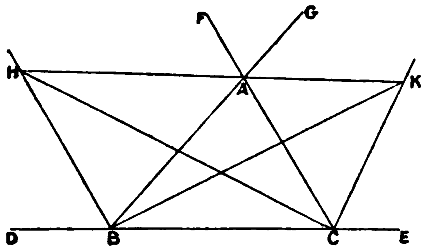
For the right angle L B M is equal to the right angle L B P, whereof the portions A B L, L B C are equal. Hence the remaining angle M B A is equal to the remaining angle P B C. But the angle M B A is equal to the vertical angle D B P, and C B P to M B K (Euc. I. 15). Hence the four angles A B M, M B K, D B P, P B C are all equal, or P M bisects both the angles A B K and C B D

Similarly P N bisects both the angles A C F and B C E; and M N bisects both the angles C A G and B A H.

COR. 1.—A line which bisects any angle bisects also (when produced) the vertical angle.

COR. 2.—The bisectors of the two pairs of vertical angles formed by two intersecting lines are at right angles to each other.

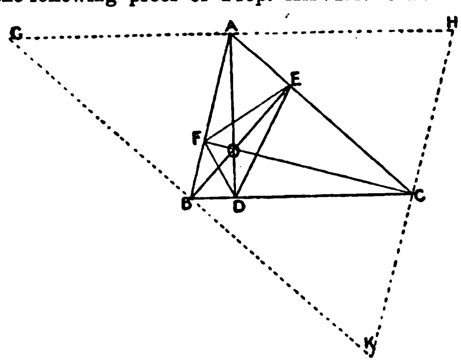
PROP. XXXV.—*Let* B C, *a side of the triangle* A B C, *be produced either way to* D *and* E, *and let* B A, C A, *be produced, respectively, to* G *and* F: *then, if* B H, C H, *the bisectors of the angles* A B D, A C B, *meet in* H, *and* C K, B K, *the bisectors of the angles* A C E, A B C *in* K, *the points* H, A, *and* K *lie in a straight line, and this line bisects the angles* F A B *and* G A C.



For, by Prop. XXXII. Cor., A H bisects the angle F A B and A K bisects the angle G A C. But the angle F A B is equal to the angle G A C. Therefore the angles H A B, G A K being the halves of these equal angles are equal. But H A B and G A K are vertical angles. Therefore H A and A K are in the same straight line, (Euc. I. 15, *Conv.*), and it has been shown that H A K bisects the angles F A B and G A C.

The *method* of the following proof of Prop. XXVIII. is worth noticing.

First, let the triangle A B C be acute angled. Draw A D perpendicular to B C and B E perpendicular to A C: then if it can be shown that C O produced cuts A B at right angles it will be obvious that Prop. XXVIII. is established, since there is only one perpendicular from C on A B.



Join D E. Then the angles of the quadrilateral O E C D are together equal to four right angles (Euc. I., 32, Cor. 1.); therefore since the angles O E C, O D C are right angles the angles D O E and D C E are together equal to two right angles. But D C E is acute (*hyp.*) therefore D O E is obtuse; and the angles O E D, O D E are, therefore together less than a right angle (Euc. 1., 32). Hence if we make the angles O E F, O D F equal to O E D, O D E respectively, the two angles D E F, E D F are together less than two right angles (being double of the two angles O E D, O D E together). Hence E F and D F meet, as shown in the figure. Join O F. Now in the triangle F E D, O E and O D are the bisectors of the angles F E D, F D E; therefore A E C and B D C are the bisectors of the angles external to F E D, F D E (Prop. XXXIV.). Hence by Prop. XXXV., F lies on the line A B and A F B is the bisector of the angles external to D F E. Also F O is the bisector of the angle E F D (Prop. XXIX.). Therefore A B is at right angles to F O (Prop. XXXIV., Cor. 2). But the points C, O, and F are in one straight line (Prop. XXXIII.). Hence C O F is at right angles to A B.

Secondly take the case of the obtuse-angled triangle A O B. Produce A O to D and draw B D perpendicular to A D. Produce B O to E and draw A E perpendicular to B E. Then the angles A E D and B D E are each greater than a right angle, so that A E and B D must meet if produced towards E and D. Let them meet in C. Then we have to show that a perpendicular from O on A B will, if produced, pass through C. Now since O E C and O D C are right angles and D O E is obtuse, the angle E C D is acute (Euc. I. 32, cor. 1); also since A E B and A D B are right angles the angles E A B and D B A are acute (Euc. I. 17). Hence in the acute-angled triangle A B C, the perpendiculars B E and A D intersect on the perpendicular from C on A B. That is the perpendicular from C on A B passes through O; or in other words, the perpendicular from O on A B, produced beyond O passes through C, the point of intersection of A E and B D produced.

SCHOLIUM.—It is worthy of notice that if we take any triangle G H K, bisect its sides in the points A B C and form the triangle A B C, and again draw the perpendiculars A D, B E, and C F, and form the triangle D F E, then the three important properties contained in Props. XXVII., XXVIII., and XXX. are illustrated together; since the same three lines A D, C F, and B E are at once the bisectors of the angles of the triangle D F E, the perpendiculars from the angles on the opposite sides of the triangle A B C, and the rectangular bisectors of the sides of the triangle G H K.

(*To be continued.*)

Our Whist Column.

BY FIVE OF CLUBS.

WONDERFUL LUCK AT WHIST.

THE following from an old number of the *Westminster Papers* will be found interesting. Mr. Proctor would probably find it rather difficult to determine the odds against the singular series of hands described :—" In the course of the last month we played Whist at the Whitehall Club. Three members of the club took part in the rubber. In the third hand it was our deal; the pack had therefore been played with once. We dealt ourselves Ace, King, Queen, Knave, ten, eight, and another trump. In due course it again became our deal, and we gave ourselves the same trumps. The third time we dealt we had Ace, King, Queen, Knave, ten, and another trump, but in a different suit. The fourth time the Quint Major and one trump; the fifth time the Quint Major; on the sixth, Quint Major, eight, and another trump; on the seventh deal our partner dealt for us. In our hand we found Ace and eight of trumps, and in our partner's, King, Queen, Knave, ten, and nine. Probably such a combination never occurred before and may never occur again. The odds against it seem so great that we commend the example to Mr. Proctor, assuring that gentleman that he can have the names of the other players for his own satisfaction. Now, this is pure luck; no skill could avail against such hands as these. This week we saw a player lose seventeen consecutive bets on seventeen even events, and we think it is because of occasional runs of luck such as these that many players look on Whist as a game of luck, and ignore the subject of skill. The same luck exhibits itself in the more scientific game of Double Dummy, only here the skill can be demonstrated to any one's satisfaction. A player may fancy that he is as good a Whist player as any one else; the fact that he loses a trick here and a trick there makes no impression, because he thinks that others make as many or more blunders, or he does not believe that he loses a trick at all. The actual and respective skill of any given players cannot, therefore, be ever demonstrated to the satisfaction of either. But take a Double Dummy player, and tell him to make the most out of a given hand, and then let his supposed equal try the same hand against the same opponent; if it turns out that the one player habitually does better with the same hand, he might acknowledge his inferiority. This could be tested by natural hands, and could be further demonstrated with the assistance of problems set by a good composer. But players say, although I play Double Dummy, I am not a problem solver. Every Double Dummy hand is a problem of more or less difficulty. It is not Whist, it is true, but the ending of a Whist hand is usually a problem. It has never been tried, to our knowledge, to have a real match, in which a sufficient number of hands have been arranged and reversed, so that each pair of players play with the same hands and at the same score. The trouble would be great, but the lesson to be taught would be of value to the card world.

ELECTRIC LIGHTING AT HULL.—At a recent meeting of the Lighting Committee of the Hull Corporation the subject of the electric lighting contract with Messrs. Siemens was gone into. The borough engineer laid before the committee the following letter from Messrs. Siemens :—" We are quite willing to let the electric lighting plant remain at Hull on hire after the expiration of the present contract (July 28, 1884), subject to the following conditions :—1. That the plant be handed back to us at the end of the period of hiring in the same condition, fair wear and tear excepted, as it is handed over by us to the corporation at the end of the present contract, unless the plant is purchased by the corporation during or at the expiration of the period of hiring. 2. That the plant is hired for at least one year from the expiration of the present contract, and that after that time either party can give three months' notice to determine the contract. 3. That the corporation pay us a sum of £476 per year in four equal instalments, payable at the end of each three months." The borough engineer expressed the opinion that the terms contained in this letter were favourable, and should be accepted. The committee having discussed the matter, resolved to endorse the borough engineer's opinion.

Our Chess Column.

By Mephisto.

"I URGE pupils to learn Chess, and enjoy it myself, to the point of its becoming a temptation to waste of time often very difficult to resist; and I have really serious thoughts of publishing a selection of favourite old games by Chess-players of real genius and imagination, as opposed to the stupidity called Chess-playing in modern days. Pleasant 'play,' truly! in which the opponents sit calculating and analysing for twelve hours, tire each other nearly into apoplexy or idiocy, and end in a draw or a victory by an odd pawn. "John Ruskin."

SOLUTIONS.

Problem No. 119, by H. W. Sherrard, p. 404.

1. Kt to Q4 if 1. K × Kt — 2. Q × P, mate
if 1. K to Q3 — 2. Q to R 3, mate
if 1. P to Q3 or B4 — 2. Kt to B3, mate
if 1. B moves — 2. Q to R2, mate

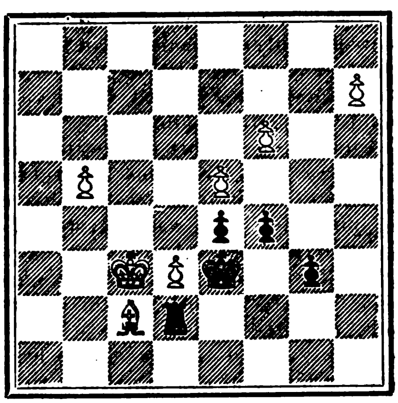
Problem by S. Loyd.

1. Q to Kt4 (L) if 1. P to B4 — 2. P × P en pass, mate
if 1. K to K2 — 2. B × P mate
if 1. K to K4 or Q 4 — 2. Q to K4 mate

The following position occurred in a game played in the Divan Tourney:—

Mr. Hirsh.

White.



Black.

Chess Editor.

White can only draw by playing

1. P to Kt5	1. R to R2
2. P to Kt6	2. R × P
3. P to Kt7 (a)	3. R to B7 (ch)
4. K to Kt6	4. R to Kt7 (ch)
5. K to R6	5. R to R7 (ch)
6. B to R5	6. R to Kt7
7. B to Kt6.	

If, instead of 7. B to Kt6, White plays 7. K to R7, then K × P. 8. P to Kt8 (Q), R × Q. 9. K × R, P × P. 10. P × P, K to B4. 11. B to B6, K to B5. 12. B × P, K to K6, and draws.

	7. R to R7 (ch).
8. K to Kt5	8. R to Kt7 (ch).
9. K to B5	9. R to B7 (ch)
10. K to Kt4	10. R to Kt7 (ch)

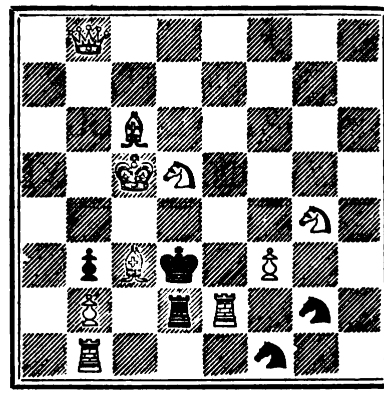
Drawn game by perpetual check.

(a) This is a good position, and yields some interesting play, notably if White on his third move plays K to Kt7. In every case, however, a draw will result, with best play.

PROBLEM (SELECTED).

By G. E. Carpenter.

Black.



White.

White to play and mate in two moves.

ANSWERS TO CORRESPONDENTS.

*** *Please address Chess Editor.*

W.—Many thanks for Problems. They are welcome. In the Problem referred to, the conditions were unintentionally omitted.

Correct solutions received from W., Caissa, George Gouge, A. W. Overton, W. Hanrahan, John Watson.

Notice to those of our readers conducting games by correspondence through the medium of Knowledge. We shall be glad to receive and publish for the purpose of criticism any position of general interest requiring analysis, either in the opening, middle, or end game.

Erratum.—In Knowledge No. 135, p. 393, second column, line 7, for "tons" read "foot-tons."

Contents of No. 136.

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CONTENTS OF No. 139.

DREAMS:

THEIR PLACE IN THE GROWTH OF PRIMITIVE BELIEFS.

By EDWARD CLODD.

III.

RACES which have names for different kinds of oaks, but none for an oak, still less for a tree, and who cannot count beyond their fingers, may be expected to have hazy notions concerning the objective and the subjective—or, to put these in terms less technical, concerning that which belongs to the object of thought, and that which is to be referred to the thinking subject. Although primitive religion and philosophy are too nearly allied to admit of sharp definitions, the former may be said, if the slang is allowed, to be one of funk, and the latter one of fog. There are those amongst us who say that the terrorism which lies at the base of the one and the mist which is an element of the other, linger yet in extant belief and metaphysics. What man cannot understand, he fears; and in all primary beliefs the powers around which seem to him so wayward are baleful, to be appeased by sacrifice or foiled by sorcery. And the confusion which reigns in his cosmos extends to his notion of what is in the mind and what is out of it. He cannot distinguish between an illusion and a reality, between a substance and its image or shadow, and it needs only some bodily ailment, as indigestion through gorging, or delirium through starving, to give to spectres of diseased or morbid origin, airy nothings, a substantive existence, a local habitation and a name.

The tangle between things and their symbols is well illustrated in the barbaric notion that the name of a man is an integral part of himself, and that to reveal it is to put the owner in the power of another. An Indian asked Kane whether his wish to know his name arose from a desire to steal it; the Araucanians would not allow their names to be told to strangers, lest these should be used in sorcery. So with the Indians of British Columbia; and among the Ojibways, husbands and wives never told each other's names, the children being warned against repeating their own names, lest they stop growing. Dobrizhoffer says that the Abipones of Paraguay had the like superstition. They would knock at his door at night, and when asked who was there, no answer would come, through dread of uttering their names. Mr. im Thurn tells us that, although the Indians of British Guiana have an intricate system of names, it is "of little use, in that owners have a very strong objection to telling or using them, apparently on the ground that the name is part of the man, and that he who knows it has part of the owner of that name in his power." In Borneo, the name of a sickly child is changed, to deceive the evil spirits that have tormented it; the Lapps change the baptismal name of a child for the same reason; and among the Abipones, the Fuegians, the Lenguas of Brazil, the North-West Indians, and other tribes at corresponding low levels, when any member died, the relatives would change their names to elude Death when he should come to look for them, as well as give their children horrid names to frighten the bad spirits away. All over the barbaric world we find a great horror of naming the dead, lest the ghost appear. An aged Indian of Lake Michigan explained why tales of the spirits were told only in winter, by saying that when the deep snow is on the ground, the voices of those repeating their names are muffled; but that in summer the slightest mention of them must be avoided, lest the spirits be offended. Among the Californian tribes the name of the departed spoken inadvertently caused a shudder to pass over all those present. Among the Iroquois the name of a dead man could not be used again in the lifetime of his oldest surviving son without the consent of the latter, and the Australians believe that a dead man's ghost creeps into the liver of the impious wretch who has dared to utter his name. Dr. Lang tried to get the name of a relative who had been killed from an Australian. "He told me who the lad's father was, who was his brother, what he was like, how he walked, how he held his tomahawk in his left hand instead of his right, and who were his companions; but the dreaded name never escaped his lips, and I believe no promises or threats could have induced him to utter it." Dorman gives a pathetic illustration of this superstition in the Shawnee myth of Yellow Sky. "She was a daughter of the tribe, and had dreams which told her she was created for an unheard-of mission. There was a mystery about her being, and none could comprehend the meaning of her evening songs. The paths leading to her father's lodge were more beaten than those to any other. On one condition alone at last she consented to become a wife, namely, that he who wedded her should never mention her name. If he did, she warned him that a sad calamity would befall him, and he would for ever thereafter regret his thoughtlessness. After a time Yellow Sky sickened and died, and her last words were that her husband might never breathe her name. For five summers he lived in solitude, but, alas, one day as he was by the grave of his dead wife an Indian asked him whose it was, and in forgetfulness he uttered the forbidden name. He fell to the earth in great pain, and as darkness settled round about him a change came over him. Next morning, near the grave of Yellow Sky, a large buck was quietly feeding. It was the unhappy husband."

The original meaning has dropped out of the current saying, "Talk of the devil and you'll see his horns," but savage philosophy recovers it for us. And the shrinking from naming persons is still more marked as we ascend the scale of principalities and powers. In the South Sea Islands, not only are the names of chiefs tabooed, but also words and syllables resembling those names in sound. The Tahitians have a custom called *Te pi*, which consists in avoiding in daily language those words which form a part or the whole of the names of the King and Royal family,

and in inventing new terms in their place. The King's name being *Tu fetu*, "star," had to be changed into *fetia*, and *tui*, "to strike," became *tiai*. In New Zealand, knives were called *nekra* because a chief's name was *Maripi*, or "knife." It is, Professor Max Müller aptly remarks, as if with the accession of Queen Victoria, either the word *victory* had been tabooed altogether, or only part of it, as *tori*, so as to make it high treason to speak of *Tories* during her reign. The secret name of Pocahontas was Matokas, which was concealed from the English through superstitious fear, and in the mythical story of "Hiawatha" the same metonymic practice occurs, his real name being Tarenyawagon. A survival of the dislike to calling exalted temporal, and also spiritual, beings by their names, probably lies at the root of the Jews' unwillingness to use the name of Yahweh (commonly and incorrectly spelt Jehovah*), and in the name "Allah," which is an epithet or title of the Mahommedan deity, and not the "great name;" whilst the concealment by the Romans of the name of the tutelary deity of their city was fostered by their practice, when besieging any place, to invoke the treacherous aid of its protecting god by offering him a high place in their Pantheon. And in the title of Eumenides, or the "gracious ones," given to the Furies by the Greeks, may be noted a survival of the verbal bribes by which the thing feared was "squared." For example, the Finnish hunters called the bear "the apple of the forest," "the beautiful honey-claw," "the pride of the thicket;" the Laplander speaks of it as "the old man with the fur coat;" in Annam, the natives call the tiger "grandfather," or "lord;" and the Dyaks of Borneo speak of the small-pox as "the chief," or "jungle leaves."

The confusion between ideas and objects which these examples illustrate is shared by us, although in a remote degree. If the initials of any well-known name are transposed, for example, let W. E. Gladstone be printed E. W. Gladstone; or if some familiar name is altered, for example, let John Bright be misprinted James Bright, it is curious to note how for a moment the identity is obscured in one's mind. Another personality, indistinct and bewildering, rises before us, showing how we have come to link together a man and his name even to the details of his initials. That which we feel momentarily the uncivilised feels constantly. He cannot think of himself, of his squaw, of his children, or of his fellow-tribesmen, apart from names which are more significant to him than ours are to us. With us the reason which governed selection is forgotten or obscured, the physical features and conditions no longer correspond to ancestral names, but with barbarous peoples those features and conditions are more apparent. Besides which, children are often named by the medicine-man, and the name is thus endowed with a charm which may roughly be analogous to the halo round a name confirmed by baptism to one simply recorded in the office of a Registrar of Births.

* The peculiar feature of the Semitic languages is that the consonants are everything and the vowels nothing, every word consisting, in the first instance, merely of three consonants, which form, so to speak, the soul of the idea to be expressed by that word. And as in ancient times the consonants only were written, the name Jehovah appeared as JHVH. Its exact pronunciation is utterly lost, and such veneration gathered round it that when the Jews came to it they substituted some other name—usually Adonai. Afterwards, when vowels were added to the Hebrew text, those in Adonai, or its phonetic form Edona, were inserted between the letters of the sacred name, and thus JHVH was written Jehovah.

We hear that Mr. F. Pitman, of Paternoster-row, is about to issue a Musicians' Directory, under the patronage of Sir G. A. Macfarren. This ought to be useful to musical societies and concert-givers.

NOTES ON FLYING AND FLYING-MACHINES.

By Richard A. Proctor.

It would be difficult to say how many centuries have elapsed since the first attempt was made to solve the problem whether man can fly. Ages before the "philosopher's stone" was ever sought for, or before the problem of perpetual motion had attracted the attention of mechanists, men had attempted to wing their way through the too unresisting air, by means of more or less ingenious imitations of the pinions of birds or insects. It has even been suggested (see Hatton Turnor's *Astra Castra*), that King David referred to successful attempts of this sort, when he cried, "O that I had wings like a dove, then would I flee away and be at rest." But without insisting on this opinion,—which indeed may be regarded as not wholly beyond cavil,—we have abundant evidence that in the earliest ages, the same problem has been attacked, which the Aëronautical Society of Great Britain took in hand but a few years since, and which, still more recently, the beleaguered Parisians sought earnestly, but in vain, to solve.

By the invention of the balloon the problem of aërial *floatation* has been solved; but the problem which has hitherto proved so intractable, is that of aërial *navigation* or flight,—whether by means of flying-machines capable of supporting many persons at once, or by means of contrivances enabling a man to urge his way alone through the air. There can be little question that this problem is one of great difficulty. It has, indeed, been long regarded by nearly all practical mechanicians as really insoluble. But of late years careful researches have led competent men to entertain doubts as to the validity of the objections which have been urged against the theory that it is possible for men to fly. Facts have come to light which seem, to say the least, highly promising. In fine, there are not a few who share the convictions of a recent president of the Aëronautical Society, that before many years have passed men will have learned how to navigate the air. The time may not be at hand, indeed, when Bishop Wilkins's prophecy will be fulfilled, and men will call as commonly for their wings, as they now do for their boots; but it does not seem improbable that before long the first aërial voyage (as distinguished from aërial drifting in balloons) will be successfully accomplished.

It may be interesting to inquire, what are the principal facts on which this hopeful view of the long-vexed problem has been founded. In so doing, I shall have occasion to touch incidentally on the history of past attempts at flight; and this history is, indeed, so attractive, that the reader may be disposed to wish that it were entered upon more at length. But my subject is such a wide one, that it will be necessary to avoid discussing, at any length, those strange, and sometimes apocryphal narratives, which are to be found in the records of aëronautics. For this reason I propose to consider only such accounts of past attempts, as appear to bear on the subject of the actual feasibility of flying.

In the problem of aërial navigation, four chief points have to be considered—buoyancy, extent of supporting surface, propulsive power, and elevating power. At first sight, buoyancy may seem to include elevating power and supporting power, but it will be seen, as we proceed, that the term is used in a more restricted sense.

In the balloon we have the perfect solution of the problem of securing buoyancy. The success with which men have overcome the difficulty of rising into the air is complete; and this being their first, and seemingly, a most important

success, we can perhaps hardly wonder that further success should long have been looked for in the same direction. The balloon had enabled men to float in the air; why should it not enable them also to direct their course through the air? The difficulty of rising into the air seemed, indeed, much the more serious of the two before the balloon had been invented; and all who had failed in their attempts to fly, had failed in precisely this point.

Yet all attempts to direct balloons have hitherto failed. It seems clear, indeed, when we inquire carefully into the circumstances of the case, that such attempts must necessarily fail. The buoyancy of balloons is secured, and can be secured, only by one method, and that method is such as to preclude all possibility—so at least it seems to me—that the balloon can be navigated. A balloon must be large,—many times larger than any machine to which it can be attached. If we take even the case of one man raised by a balloon, and inquire how large the balloon should be, we at once see how disproportioned the size of a balloon must needs be to the bodies of a heavier nature which it is intended to raise. We know that a man can barely float in water, so that he is about equal in weight to an equal volume of water. But a volume of water is more than eight hundred times heavier than an equal volume of air, even at the sea-level, where the air is densest. So that the weight of a man is more than eight hundred times greater than that of the air he displaces. It follows that if a very light hollow vessel could be made, which should be more than eight hundred times as large as a man, and which could be perfectly exhausted of air without collapsing (a thing wholly impossible), the buoyancy of that vessel would barely enable it to support the weight of a man. But the balloonist is unable to obtain any vessel of this sort. He cannot employ the buoyancy of a perfect vacuum to raise him. What he has to do, is to fill a silken bag with a gas lighter than air, but still not weightless, and to trust to the difference between the weight of this gas and that of the air the balloon displaces, to raise him from the ground. So that such a balloon, in order to raise a man, must be considerably larger than the hollow vessel just referred to. But further, the balloon must rise above the denser parts of the air; it must carry its own weight as well as that of the man; the balloonist must take a supply of ballast; and other like considerations have to be attended to, all of which render it necessary that the balloon should be larger than we have hitherto supposed. Apart, however, from all such considerations, we find the very least proportion between the size of the balloon intended to carry one person, and the size of the human body to be about as one thousand to one. Buoyant vessels constructed on such a scale must needs present an enormous surface; and therefore, not only must they strongly resist all attempts made to propel them in any direction, but the slightest wind must have more effect upon them than any efforts made by those they carry. As for any power which should avail to propel a balloon against a strong wind, the idea seems too chimerical to be entertained. Until men can see their way to propelling a buoyant body (one thousand times larger than the weight it supports), at the rate of fifteen or twenty miles an hour through calm air, they cannot expect even to resist the action of a steady breeze on a balloon, far less to travel against the wind. But even if it were possible to conceive of any contrivance by which a balloon could be propelled rapidly through calm air, yet the mere motion of the balloon, at such a rate, would sway the balloon from its proper position, and probably cause its destruction. A power which could propel the car of a balloon through calm air at the rate of twenty miles an hour, would cause precisely the same effect on the balloon itself, as though the car were fixed, while a heavy wind was blowing against the balloon. We know what the effect would be in this latter case; the balloon would soon be made a complete wreck: and nothing else could happen in the former case.

But it may be seriously questioned whether buoyancy is a desirable feature in any form of flying-machine. We have seen that a degree of buoyancy sufficient to secure actual floatation in the air is incompatible with aërial navigation. We may now go further, and urge that even a less degree of buoyancy would be a mischievous feature in a flying-machine. M. Nadar, the balloonist, makes a significant, though not strictly accurate observation on this point, in his little book on flying. Passing through the streets of Paris, during the ædileship of Haussman, he heard a workman call from the roof of a house to a fellow-workman below, to throw a sponge up. "Now," says Nadar, "what did the cunning workman, who was to throw the sponge up, do? The sponge was dry, and therefore light and buoyant. Was it in this condition that he threw it up to his fellow? No; for it would not have been possible to send it above the first floor. But he first wets the sponge, and so makes it heavy; and then, when it has been deprived of the lightness which is fatal to flight, he throws it easily to his fellow on the house-roof?" M. Nadar infers, that the first essential in a flying-machine is weight.

Now what is true in the above reasoning is that buoyancy renders flight—as distinguished from aërial floating—impossible, or at least difficult. It is not true, however, that the flight of the wet sponge exemplifies the kind of flight which the aëronaut requires. The sponge, in fact, was neither more nor less than a projectile; and most assuredly, the problem of flight is not to be solved by making projectiles of our flying machines or of our own bodies. It may be, and indeed we shall presently see that it probably *will* be necessary, that some form of propulsion from a fixed stand should have to be applied to the flying-machines of the future. But after such propulsion has been applied, the flying machine must be *supported* in some way, not left —as an ordinary projectile is left—to the action of unresisted gravity. M. Nadar's wet sponge is no anologue, then, of the flying-machines we require.

Before leaving the subject of buoyancy, however, it will be desirable to inquire whether buoyancy is, in any marked degree, an attribute of the flying creatures we are acquainted with—birds, bats, and insects. The structure of such creatures has been supposed by some to be such as to secure actual buoyancy, to a greater or less degree; and many would be disposed, at a first view of the matter, to regard the hollow bones and the quill-feathers of birds as evidences that buoyancy is essential to flight. I have even seen the strange theory put forward, that during life, the quills of birds, as well as their hollow bones, are filled with hydrogen. "Flying animals," says a writer in *All the Year Round* for March 7, 1868, "are built to hold gases everywhere—in their bones, their bodies, their skins; and their blood is several degrees warmer than the blood of walking or running animals, their gases are probably several degrees lighter. Azote, or hydrogen, or whatever the gas held in the gaseous structures may be, it is proportionately warmer, and therefore proportionately lighter than air."

This of course is nonsense. But on a careful consideration of the structure of flying creatures, the hollow portions of their bodies will be found to fulfil a purpose quite distinct from that of imparting buoyancy. If we examine a quill, we find that the most remarkable feature which it presents to us, is the proportion which its strength, especially as respects resistance to flexure, bears to its weight. It would be difficult, indeed, to construct any bar, or rod, or tube, of the same length and weight as a portion of a bird's quill,

which would bear the same pressure without perceptible flexure; and it is scarcely conceivable that any structure appertaining to a living creature could possess greater strength with an equal degree of lightness. In the hollow bones, again, we see the same association of strength and lightness. Precisely as a tubular bridge, like that which spans the Menai Straits, is capable of bearing far greater strain than a solid metal bar of equal weight and length, so the hollow bones of birds are far stronger than solid bones of equal weight would be. We see, then, that *lightness* is secured in these parts of a bird's structure. But lightness and buoyancy are different matters. We can understand that it is absolutely essential that the weight of a machine intended for flight should be as small as may be, due regard being had to strength and completeness. But there is little in the structure of flying creatures, which points to buoyancy as a desirable feature in a flying machine.

(To be continued.)

PLEASANT HOURS WITH THE MICROSCOPE.

By Henry J. Slack, F.G.S., F.R.M.S.

A FEW days ago, on stroking a pet cat, a little round swelling was felt, and at once suspected to be what it proved, a tick of the Ixodes sort. Persons living in the country near open heaths or woods often find dogs, cats, and other animals infested with creatures of this kind, which attack them as they wander through furze or brushwood. The females are the assailants, and if it were correct to speak so of eyeless animals, they might be described as waiting in the herbage on the look-out for such prey. At certain times and places these creatures swarm, and thousands can never find an opportunity of exchanging their parasitism upon plants for a feast upon animals. In a young stage, Mrs. Dog Tick is of pin's head proportions, but if lucky enough to get on to an animal she fortwith buries her proboscis in its skin, and devotes the remainder of her days to sucking, growing, and maturing her eggs, which are very numerous. She soon swells out to the size of a pea, and even bigger. She is anchored so tightly to her victim, and her skin is of such leathery toughness, that even when she is within reach of claw or mouth she is not likely to be displaced. Such an accident is also rendered less probable by her habit of selecting a spot the animals cannot reach. One or two of them do not seem to occasion their victims much annoyance, but when they swarm, they are very mischievous, the irritation they set up producing constitutional disturbance, accompanied with ulcerations.

Upon cats and dogs the writer has usually found what he believes is "generally known" as the Dog Tick, and on one occasion only a specimen of *Ixodes marginatus*, as figured in Andrew Murray's "Economic Entomology." In Murray's book *Ixodes erinaceus* is said to be commonly mistaken for *I. ricinus*, and he considers it the one "generally known" as the Dog Tick, but his figure refers to a different species to what I have found on that animal. The "Micrographic Dictionary" describes *I. ricinus* as having the "body oval; in the gorged condition becoming globular, and blackish violet; legs and appendages brown." It also states that *I. Dugessii* is found on dogs, and it figures it of a leaden blue tint, but omits what is characteristic of all the family, the hard, brown, chitinous shield, or carapace, on the back, behind the head. The Ixodes have all a very remarkable piercing, anchoring, and sucking mouth-apparatus. It consists of a central rostrum and two mandibles closing upon it, and all barbed to give the creature a firm hold. Two palpi, when folded together, can enclose the barbed organs, and are opened widely when they are inserted in their victim's skin. A moderate-sized specimen is about a quarter of an inch long from the top of the rostrum to the end of the abdomen. The eight legs are crowded together just below the head. At each side, behind the last legs, is a stigma of elegant pattern, and at about one-third up the abdomen (underside) is an organ supplied with numerous tubes like spinnerets. The legs are terminated with two claws and a caruncle. The colour of those the writer has found is a sort of drab, sometimes inclining to bluish. Specimens mounted in balsam turn reddish brown. The species *I. marginatus* has a half-round narrow moulding of lighter colour round the abdomen.

Although these animals are not, like many of the mites, difficult to investigate on account of size, their internal organs require for their display more skill than any beginner is likely to possess, and they are certainly not desirable creatures to commence with. What is easy is to slit up the tough skin with fine scissors, and squeeze all the insides out by pressure between blotting-paper. The mouth organs make a good object when separated. The stigmas must be cut out and freed from fat and other matter by scraping with a fine knife, such as surgeons use for operations on the eye, and washing with water and a small, soft brush. They can then be soaked in carbolic acid, and mounted in Canada balsam. The organ in the abdomen can be treated in the same way.

Mr. Michael, in his valuable work "The British Oribatidæ," lately issued by the Royal Society, doubts the so-called stigmata of the Acarina having respiratory functions, and supposes they are sense organs. The reader who wishes to pursue this question, may be referred to the important reasons he adduces in support of that opinion. In the Ixodes genus the stigmata are very different in appearance from those which he figures of the *Oribatidæ*, but their functions may be identical, and, as will be shown, their structure is very complicated. The only popular or generally accessible work on these Acarina is that of Andrew Murray,* and it is much to be regretted that he did not live to continue his studies of them.

The order Acarina contains a great number of small creatures more or less related to spiders, but differing from them by the abdomen not being joined to the thorax by a narrow attachment, or segmented, as in the scorpions. The Acarina have eight legs in their adult stage. They are divided into numerous groups, about which the different authorities have not yet agreed. The Ticks (*Ixodes*) are not difficult to recognise when any one of them has been seen; but it should be borne in mind that the so-called Sheep Tick is not related to the Ixodes Ticks, but is a true insect belonging to the diptera (*Melophagus*).

The Ixodes Ticks, the gnats, and the bed bugs, are all provided with mouth-organs especially adapted to pierce the skin of various animals and suck their blood, and yet millions have to pass their lives without any opportunity of enjoying such food. A great many of the ticks must either continue vegetarians, as they are said to be in their babyhood, or pass their adult days in a long fast. Myriads of gnats swarm in localities, including desert islands, where there are no animals for them to torment, and the common bug will multiply in uninhabited houses without a chance of enjoying itself at the expense of human beings. With regard to the ticks, although particular kinds attack certain

* "Economic Entomology."

animals by preference, only a few seem confined to one sort. Many will assail the human species if they find an opportunity, and the Indians say of an American species, *I. bovis*, "bite all same as a piece of fire." *I. transversalis* is a little comical kidney-shaped thing that devotes its attention to serpents. The male of these creatures is by no means the equal of his wife. Murray says of him, "his mission is done as soon as he has impregnated the female, and he may go about his business and die as soon as he pleases. Nature has no further use for him, but the female has still to lay her eggs, therefore she must be fed." How does she get on if she cannot attach herself to any animal? Can she bring up a family as a vegetarian?

It is difficult to pull an Ixodes out of the skin of a cat or dog without breaking off part of the barbed, piercing tools, and if much remains behind it may make a sore.

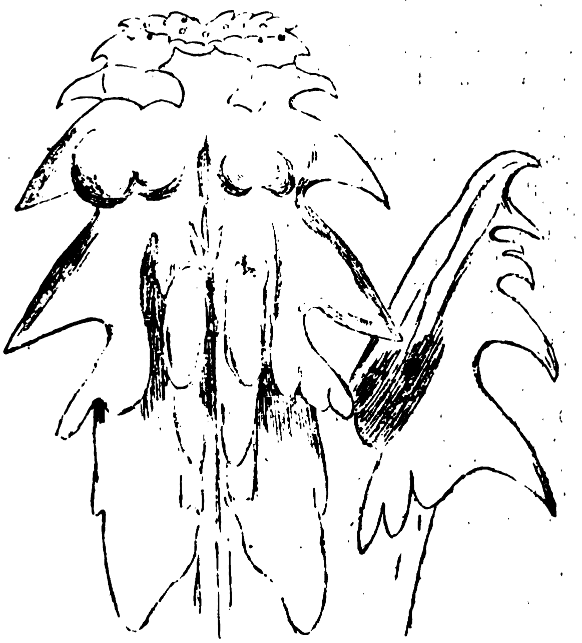


Fig. 1. Rostrum and one mandible of Ixodes marginatus × 320.

The annexed sketch (Fig. 1) shows the rostrum and one of the two (so-called) mandibles of *Ixodes marginatus*, magnified 320 times linear. The stigma looks to the naked eye a little brown spot about 1-80th of an inch in diameter. Under a magnification of from 50 to 100, its markings bear a rough resemblance to those of one of the circular diatoms,

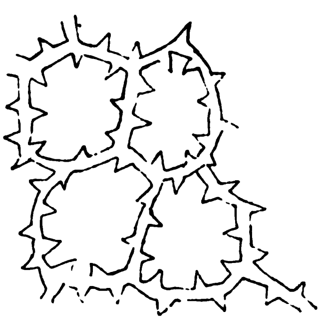


Fig. 2. Network in Stigma of Ixodes—Dog-tick × 600.

but an oil immersion 1-12th focussed carefully down, first brings into view a network of cells as shown in Fig. 2, Five rows of these are concentrically disposed round a central part in which no definite structure is discernible. Focussing lower gives a view of the interior of the cells, the walls of which may be corrugated, and seem to be supplied with numerous small projections. These remarks are made chiefly to suggest further investigation. Whatever may be the use of these organs, they are elegant and complicated structures, and the details could only be made out by many careful dissections and the use of high powers.

ERRATUM.—In last paper, p. 431, line 2, read "*exuviae*" for "exuves."

ELECTRIC PROJECTORS ON BOARD YACHTS.*

WE learn from our contemporary *Le Yacht* that the *Julie* has last summer made a voyage in very intricate navigation, and by the use of an electric light condensed into a powerful beam, has been enabled to continue her course after dark, when otherwise she must have anchored, or run very considerable risks. The electric apparatus consists of a Gramme dynamo, of the type A B, giving a current of 25 ampères and an electro-motive force of 50 volts. It is driven by a Brotherhood engine having cylinders 4 in. in diameter, and running at 900 revolutions. The two are mounted upon one bedplate fixed upon the bridge in a box, so that they are easily accessible and under perfect control. The installation weighs 1,080 lb., and was supplied by Messrs. Sautter, Lemonnier, & Co.

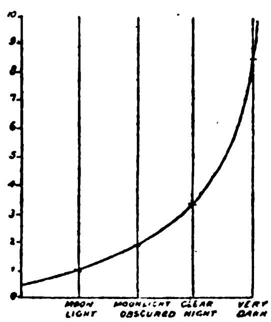
A Mangin projector is employed with an aplanatic mirror $15\frac{3}{4}$ in. in diameter. It is suspended by a block from the mizen-mast, and is held by two branches jointed to the bridge in such a way that it can sweep 270° without the steersman being inconvenienced by the light. The lamp is automatic, of the Gramme pattern, and takes carbons of 13 mm. in diameter. The beam proceeding from the projector is very bright; under favourable conditions it renders objects visible at a distance of $2\frac{1}{2}$ miles, and its intensity may be placed at five million candles, when in its most concentrated form. It can, however, be spread over a wider field when desirable.

The *Julie* entered the basin at Ostend at 8.45 p.m., on July 14, 1883, and then the light was set in action. By its aid, as no pilot was to be found, the vessel was steered between the moles, and right up the centre of the channel into the harbour, although no one on board was acquainted with the navigation. On the 20th she left Rotterdam at two o'clock to go to Amsterdam by the Yssel and the Amstel, through the fertile country of Gouda. The canal was entered at seven o'clock, and its difficult navigation commenced. The shallowness of the channel, and its numerous turnings greatly interfered with the progress, while at each sharp corner the steam capstan had to be employed. At 9.30 the electric lamp was lighted, and, thanks to its illumination, the voyage was continued through more than twenty bridges and locks, without any accidents, to Amsterdam. A somewhat grotesque phenomenon was noticed. The animals in the pastures upon the borders of the canal awoke and stared at the vessel, and in their eyes there was seen the reflection of the light which took different tints according to the kind of animal. At certain times it seemed as if they were the signal lights of boats approaching in the opposite direction. The vessel left Amsterdam at 8 p.m. on the 27th by the North Canal, whose dimensions

* From *Engineering*.

recall those of the Suez Canal. At 9 p.m. the lamp was lighted, and the beam being elliptical, the two banks were illuminated for a hundred yards before and behind the bows.

In every case the employment of the electric light was a success. In the previous summer Mr. Gaston Menier found many instances, in the course of a cruise on the coast of Normandy, where the electric light was most useful, if not indispensable, as in the entry of ports, and the navigation of tortuous channels, and the like. The state of the sky has an important effect upon the penetration of the light. A calm dark night is the most favourable season, and any general illumination in the atmosphere diminishes the intensity proportionably. The annexed diagram, drawn by Mr. Menier, represents graphically the intensities produced by the luminous beam, the conditions of power and distance remaining constant.



The eye perceives an object by the light which is reflected by that object. In the case we are considering that object receives and reflects the light of the luminous source, and the light diffused in the atmosphere. The object therefore should be the more visible the more luminous is the atmosphere, but, as a matter of fact, it is the contrary which happens. In reality our eyes behave like lenses; they perceive all the light reflected in the field of vision, and that in proportion to the surfaces illuminated. As the object occupies but a very small extent of the field of view of our eyes, the light which it reflects finds itself swamped in the light emitted by the atmosphere, and which, although feeble, acts over the whole area of the eye. It results that when the atmospheric light is augmented it acts over the whole circle of vision, and consequently more powerfully than the intensity of the light reflected from the illuminated object.

From a military point of view this fact is important, because it permits of torpedo-boats being discerned at a greater distance on a dark night, whereas on a bright night they will be seen later. The state of the weather makes a great difference. The slightest fog diminishes greatly the visibility, because the vesicles become lighted up and diffuse the rays, putting the observer under unfavourable conditions. In any case the electric light is a most valuable agent, even if it does not furnish absolute safety against night attacks.

THE ENTOMOLOGY OF A POND.

By E. A. BUTLER.

THE MIDDLE DEPTHS (*continued*).

THE large and important group of water-beetles now calls for notice. They are readily divisible into two sections, which differ considerably both in structure and habits. One of these, called the Hydradephaga, is a carnivorous group, and contains, along with a multitude of minute species, some large and highly-predaceous insects. They are, in fact, the aquatic representatives of the most highly carnivorous of all the Coleoptera, the active and rapacious ground-beetles, which are, to beetles generally, as lions, tigers, wolves, and jackals are to the rest of quadrupeds. The Hydradephaga, therefore, are to the ground-beetles as seals, sea-lions, and walruses are to the above-named terrestrial carnivora—viz., a section of the group specially modified for an aquatic existence, and having as their appointed duty the repression of the superabundance of aquatic life, just as their terrestrial brethren do their best to prevent an excess of population on land. This function they fulfil admirably, for they are extremely voracious, especially the larger kinds—*e.g.*, one large insect, found on the Continent, was observed on one occasion to devour two frogs within the space of forty hours. They will also attack young fish, as well as other insects. The other group, called the Philhydrida, contains fewer large and conspicuous insects, though one of its members is the largest of all our British water-beetles; they are to a great extent vegetarian in diet, at least in the perfect state, and so remind one of the dugongs and manatees of the mammalian world. To gain a clear notion of the difference between these groups, it will be well to consider a typical example of each; fortunately there are two large insects which are common and well-known, and will very well serve to illustrate the points of distinction. They are *the* Water-beetles, *par excellence*, *Dytiscus marginalis* and *Hydrophilus piceus*. The former is the carnivore and the latter the herbivore, for which reason Dytiscus is eschewed and Hydrophilus welcomed as an inhabitant of an aquarium.

Looking first at the general appearance of the two insects, we see that while both are of an oval shape, an obvious advantage to creatures that have to cleave their way through the water, the former is a good deal flattened and the latter more convex; in colour, too, they differ, Dytiscus being olive brown with a yellow border, and Hydrophilus uniformly black or olive-black, a difference which is hinted at in the specific names "marginalis" and "piceus." Descending now to structural details, we find the greatest differences in the appendages of the head (Fig. 1). In the carnivore the antennæ are long and thread-like, but in the

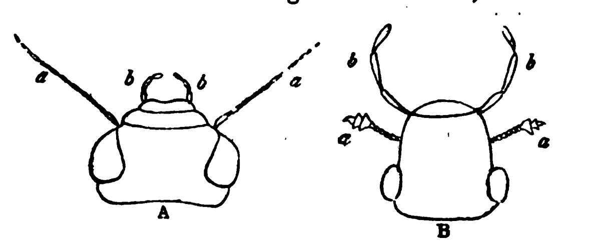


Fig. 1.—Heads of (A) Dytiscus and (B) Hydrophilus.
a. Antennæ. *b*. Maxillary palpi.

herbivore short and clubbed; this point, however, may not be made out at a first glance, as Hydrophilus frequently carries its antennæ packed away close to the body out of sight, and flourishes instead a long pair of thread-like organs very similar in appearance to the antennæ of Dytiscus, but different in function, differently placed, and composed of fewer joints. These organs are the maxillary palpi, and are attached to the maxillæ or secondary jaws, and correspond to the organs terminating in a hatchet-shaped joint we referred to when considering "ladybirds." Dytiscus has similar organs, but not so conspicuously developed, and hence they are apt to escape observation, the long, thread-like antennæ being the first things to attract attention.

Examining now the legs in our two typical insects, we see that while the hind pair in each are fringed with hair, and compressed so as to become natatorial, this modification is carried out most completely in Dytiscus; again, whilst the first two pairs are near together in the brown beetle, and the third is placed much farther back, thus giving plenty of room for an extended backward and forward movement in swimming, those of its black cousin are much more regularly disposed. There is a curious point about the hind legs that deserves notice. In beetles, generally, the legs are attached to the body by a rounded joint, which is "let in" to a corresponding perforation in the chitinous armature with which their under surface is protected, and is capable of more or less free movement therein, an arrangement which permits motion of the legs in various directions. If now the hind legs of Dytiscus be compared with those of other beetles, this basal joint seems to be wanting, and the leg therefore seems to have one joint fewer than usual. But it will be observed that each leg is attached to a broad plate (Fig. 2), the pair of which stretch right across the body, and are prolonged in the centre into a bifid spine, which is differently shaped in different species.

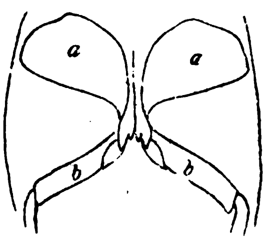


Fig. 2. Part of hind-legs of Dytiscus. *a*. Coxa; *b*. thigh.

Now these plates are really the much-expanded and greatly-modified *coxæ*, or basal joints above-mentioned. Their enormous enlargement provides a large area for the attachment of the muscles that move these limbs, and thus enables vigorous and powerful strokes to be made, though their immobility considerably impairs the freedom of movement of the limbs, and in fact limits it to the horizontal strokes which are most useful in swimming. The coxæ of Hydrophilus also are considerably enlarged, but do not attain the proportions of those of Dytiscus. The former, moreover, may be said to paddle rather than swim, moving its legs alternately, while the latter moves them both together, like a frog. Thus, in every respect Dytiscus is of the two much the better adapted for an aquatic life. Though the smaller insect, too, it has been known to attack and make a meal of its black cousin.

The distinctive peculiarities which characterise these two insects are exemplified more or less clearly in the majority of the members of the two groups. A large number of the Philhydrida, however, have ordinary ambulatory legs, and, indeed, are more given to crawling over subaqueous plants than to independent swimming, and some of the Hydradephaga even are somewhat similarly circumstanced, while the Gyrinidæ, which also belong to this group, are, as we have already seen, an exceedingly aberrant family.

A practical difficulty now suggests itself. Here are air-breathing creatures which spend their existence almost wholly in the water; how is their respiration to be conducted? It is well-known that the air necessary for the oxygenation of an insect's blood is taken in, not at the mouth, or any other part of the head, but through certain openings in the sides, which lead by short tubes to two long ones running the whole length of the body and sending out branches to the different parts. If an insect be cut open, these tubes appear as so many minute silvery threads, branching sometimes like the roots of a tree. Most of the spiracles, or entrances to these tracheal tubes, are, in beetles, situated on the upper surface of the back, under elytra and wings. The back is flat, and the elytra being somewhat arched, but fitting closely to the body at their outer edges, except at the extreme apex, a hollow chamber is thus formed over the spiracles, which can be filled with air, but to which the water has no access. In order to breathe, therefore, the insect repairs to the surface, and, thrusting the tip of its body just out of the water, with head sloping obliquely downwards, balances itself by means of its outstretched oars, whilst it receives the outer air into its air-chamber. The supply thus taken in enters the spiracles as required, and is sufficient to meet the demands of the insect for some time, so that it is perfectly free to enjoy its subaqueous life till the complete vitiation of this store renders another visit to the surface necessary. An advantage following this arrangement is that the wings are always kept dry and ready at any moment to bear their owner *per auras*, if the spirit of migration should come upon it. A similar arrangement holds good for the bugs described in the last paper, as well as for the Gyrinidæ.

(*To be continued.*)

SEA-CLOUDS.*

By Richard Jefferies.

Far out over the sea there is a peculiar bank of clouds. I was always fond of watching clouds; these do not move much. In my pocket-book I have several notes about these peculiar sea-clouds. They form a band not far above the horizon, not very thick but elongated laterally. The upper edge is curled or wavy, not so heavily as what is called mountainous, not in the least threatening. This edge is white. The body of the vapour is a little darker, either because thicker, or because the light is reflected at a different angle. But it is the lower edge which is singular; in direct contrast with the curled or wavy edge above the under edge is perfectly straight and parallel to the line of the horizon. It looks as if the level of the sea made this under line. This bank moves very slowly—scarcely perceptibly—but in course of hours rises, and as it rises spreads when the extremities break off in detached pieces and these gradually vanish. Sometimes when travelling I have pointed out the direction of the sea, feeling sure it was there, and not far off, though invisible on account of the appearance of the clouds, whose under edge was cut across so straight. When this peculiar bank appears at Brighton it is an almost certain sign of continued fine weather, and I have noticed the same thing elsewhere; once particularly it remained fine after this appearance despite every threat the sky could offer of a storm. All the threats came to nothing for three weeks, not even thunder and lightning could break it up,—"deceitful flashes," as the Arabs say; for, like the sons of the desert, just then the farmers longed for rain on their parched fields. To me, while on the beach among the boats, the value of these clouds lies in their slowness of movement, and consequent effect in soothing the mind. Outside the hurry and drive of life a rest comes through the calm of nature. As the swell of the sea carries up the pebbles, and arranges the largest farthest inland, where they accumulate and stay unmoved, so the drifting of the clouds, and the

* From an article on "Sunny Brighton" in *Longman's Magazine*.

touch of the wind, the sound of the surge, arrange the molecules of the mind in still layers. It is then that a dream fills it, and a dream is sometimes better than the best reality. Laugh at the idea of dreaming where there is an odour of tar if you like, but you see it is outside intolerable civilisation. It is a hundred miles from the King's-road, though but just under it.

THE INTERNATIONAL HEALTH EXHIBITION.

V.

We now come to a consideration of the final exhibits in the "Doulton section," viz., those which are concerned with the so-called "sanitary ware" and "filters." For the sake of convenience we shall here confine our observations to the former, and notice their types of filter when we come to speak of water and water supplies.

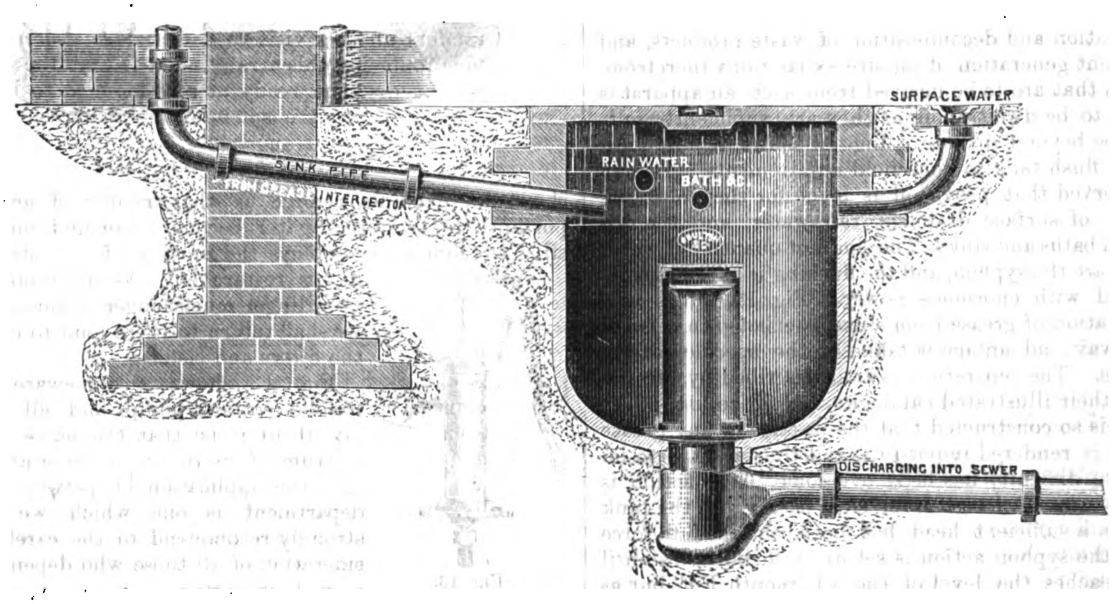


Fig. 10.

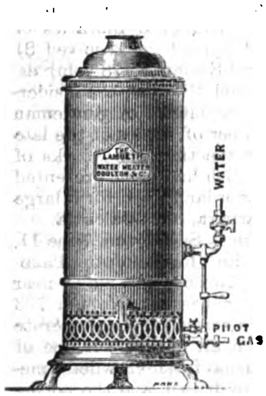


Fig. 8.

Fig. 9.

The value of the bath in houses where easy access cannot be gained to some good public institution, need not be insisted upon. What, however, is of importance here is that a comfortable bath may be obtained by most persons without much ado through the use of the "Lambeth" instantaneous water heater. We take it for granted that we are addressing a majority of our readers—those who cannot or do not care to undergo the ordeal of a cold-water bath in winter. The instrument provided by Messrs. Doulton to meet this want, is shown at Fig. 8. It consists of an outer case of copper, which contains a coiled flattened tube of large calibre of the same metal. Cold water is permitted to pass into this tube from a tap attached to one side, and a kind of Argand flame, composed of several jets of gas circularly disposed, is then lighted from beneath. The coiled arrangement is so constructed as to offer the largest possible warming surface, and, as the gaseous fumes do not come into contact with the water at any time, an intensely heated and pure bath may be relied upon at the shortest possible notice. It is estimated that thirty gallons of water can be heated in this way in about ten minutes, and when we bear in mind in addition to the inconvenience attendant upon hot-water pipes, and sources of possible contamination which are thus got rid of, we may confidently leave this simple yet effective instrument to assert its own virtues to those who are interested in this portion of domestic economy.

The instantaneous water-heater may be seen in action in the Sanitary-ware Annexe, and so full of interest to the health inspector is this department, that we would like to draw special attention to each and every article in turn. We cannot do more, however, than devote a passing word to some of the more important exhibits. Fig. 9 shows the "Lambeth" patent combination closet, which includes a slop-sink, urinal, and closet. It is so constructed that all the parts

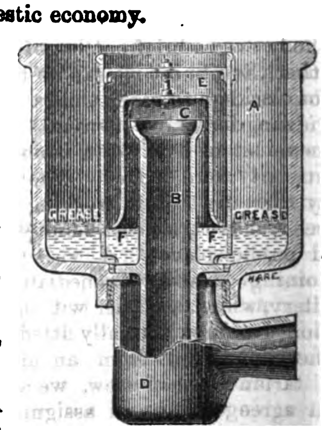


Fig. 11.

may be rapidly inspected and cleaned, whilst offensive accumulations become impossible. A small quantity of water, about $1\frac{3}{4}$ in. in depth, remains constantly within the basin, and as the superficial area of this water

is equal in size to the seat, the risk of soiling the latter is reduced to a minimum. Attention may be here drawn to the "vacuum" water-waste preventer, which has been specially designed to work with the patent automatic flush apparatus of Messrs. Doulton. Its value rests chiefly in the fact that it cannot get out of order (as it does not contain any discharge-valves) through internal gatherings of foreign particles, such as straw, leaves, &c., and that a flush of two gallons is secured, without having to hold the chain, which becomes greatest at the end of the discharge. The apparatus is fixed above the closet or sink, as shown at Fig. 9.

One of the greatest advances, however, which has been recently devised in sanitary ware, is Messrs. Doulton's patent automatic flush tank. The principle upon which this tank is constructed may be gathered from the sectional views here given (Figs. 10 and 11). It provides for the constant and effectual flushing of drains, and thus prevents the accumulation and decomposition of waste products, and the consequent generation of impure exhalations therefrom. The benefits that are to be derived from such an apparatus do not need to be dwelt upon, as they are sufficiently self-evident to be beyond dispute. Fig. 10 shows the applicability of the flush tank to household requirements. It may be here observed that provision is made for the collection and disposal of surface drainage, rain-water, and the discharges from baths and sinks. The smallest quantity of water suffices to start the syphon, and the flushing is automatically accomplished with enormous power. Fig. 11, shows how the accumulation of grease from sinks is effected in a novel yet simple way; advantage is taken of the specific gravity of the grease. The separation is thus described by Messrs. Doulton in their illustrated catalogue and price list, p. 32. "This tank is so constructed that the passage of any grease to the drain is rendered impossible, and thus a hitherto insurmountable difficulty has been overcome. The action is as follows:—The discharge from sink passes into the tank A, and when a sufficient head has been obtained to force the trap D the syphon action is set up, and continues until the water reaches the level of the bell mouth F F, but as there is an outer casing, E, open at the top, which descends some inches further down into the tank than F F, the grease—*at all times floating*—never reaches the outlet, and can be periodically removed. The water and *sludge* alone are sucked under the bottom of the casing E, the grease being left."

A description of the large variety of sanitary wares exhibited by Messrs. Doulton would require a treatise of itself to embody even a brief outline of each class of goods displayed, so that the utmost we can do here is to direct the attention of our readers to other things which we are precluded from reviewing for want of space. Before we leave the sanitary annexe let us look for a moment within the screened compartment there lodged. The automatic flushing apparatus may there be seen in working order, as applied to a large three-seat trough closet adapted for use in schools, factories, and public places. A visit should also be paid to the adjoining lavatory immediately outside of the Central Gallery, which, together with all the lavatories in the Exhibition, has been specially fitted up by Messrs. Doulton & Co. Whether viewed from an artistic, economic, or strictly utilitarian point of view, we are confident that every one will agree with us in assigning to this eminent firm the highest place of honour for innovations and improvements in household sanitary ware.

But we would be sorry to bid Messrs. Doulton adieu without according to them a vote of thanks in the interests of laboratory workers, both in the physical and natural sciences. In the filter annexe there are numerous articles which deserve more than ordinary notice. The annoyance of imperfect stoppers, corks, &c., to apparatus such as chemical vessels, laboratory sinks, &c., is entirely avoided in the perfectly fitted acid-proof ground stoppers of stoneware. The retorts, stills, &c., of which Fig. 12

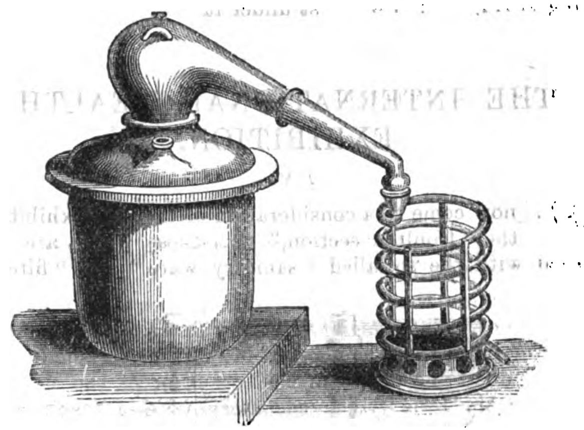


Fig. 12.

shows an example, remind us that results of great importance to the investigator become more and more possible to him who employs the most perfect instruments in his researches. We must not, however, linger much longer in this section; we shall return to it ere long to examine the filters and cisterns.

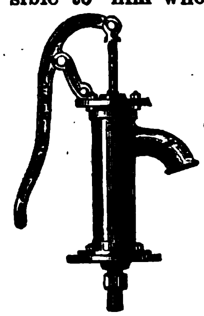


Fig. 13.

Fig. 13 shows a stoneware incorrodible cottage pump, and all we can say about it is that the name of this instrument is most appropriate, and that the application of pottery to this department is one which we would strongly recommend to the careful consideration of all those who depend upon a well or spring water-supply, and the maintenance of healthy habitations in the country.

THE seventy-fourth annual meeting of the Swedenborg Society was held at 36, Bloomsbury-street, London, on Tuesday, the 17th inst., the Rev. J. Presland presiding. The report of the Committee states that 2,387 volumes of the works of Swedenborg have been sold, and 1,287 presented during the year. The Free Libraries of Cheltenham, Gateshead, and Newcastle-on-Tyne have received 81 volumes, and various other Institutions and Societies 178 volumes. In the Transvaal, New Zealand, Canada, and Sweden, a considerable number of volumes have been put in circulation. A gentleman residing in Dinapore has distributed a number of copies of the late Mr. Dadoba Pandurung's "Reflections respecting the Works of Swedenborg." Copies of the works in English had been presented to Baboo J. K. Mukharji (a large Zemindar or landlord), for a large free library, which he has founded at Uttarpara, near Calcutta.

AT the ordinary meeting of the Geological Society on June 11, Mr. H. J. Eunson, F.G.S., read a paper on the "Range of the Palæozoic Rocks beneath Northampton." In two borings made near the town by the local water company, after passing through 738 feet of the upper, middle, and lower lias, a series of conglomerate sandstone and marls were found resting on an eroded surface of carboniferous dolomite, passing into the usual fossil-crowded limestone. 46 feet of carboniferous strata were drilled, and the boring was discontinued at 851 feet. A second boring at a place called Gayton, after passing through various strata, came upon an eroded surface of carboniferous limestone at a depth of 699 feet. In this fossils were found down to a depth of 889 feet, the boring being continued to a total depth of 944 feet. At Orton, near Kettering, a boring was made through white lias, rhœtic, sandstone, and breccia, into quartz-felsite in a futile search for coal. As none was found down to a depth of 789 feet, the boring was discontinued.

DICKENS'S STORY LEFT HALF TOLD.

By Thomas Foster.

IN "Leisure Readings," the fifth volume of the Knowledge Library Series I have dealt with the "Mystery of Edwin Drood" somewhat after the scientific manner. The problem seems to me well worth considering by all who love Dickens for the simple reason that once it is solved—as I am sure it can be and has been—the incomplete work need no longer be left unread, as it has been by many lest an interest which cannot be satisfied should be excited. I now add some remarks suggested chiefly by a recent rather feeble article on the subject in the *Cornhill Magazine*.

The "Mystery of Edwin Drood" is regarded by many as the least interesting of Dickens's novels, though many, among others the poet Longfellow, recognised it as giving promise of being among the very finest of his works. I am convinced that the want of interest complained of is in reality due to an entire misapprehension of the nature of the mystery from which the story takes its name. The idea entertained by the average reader (whose ways Dickens thoroughly understood) is naturally that the mystery is of a commonplace kind. Edwin Drood has been murdered, they suppose, by Jasper. This is too obvious they conceive to leave any real mystery. And truly the story would be uninteresting enough—however sensational —if this were its main drift. No one could care particularly to learn how the murder had been committed or the body disposed of, even if Dickens had left either point much open to doubt. Nor would there be much interest in noting how the murder was to be detected, even if he had not clearly shown—and even told Forster—that the ring given to Drood by Mr. Grewgious was to bring Jasper to punishment, being on the body which Jasper consigned to quicklime that flesh and bones and clothes might all by destroyed. Of course the love scenes connected with the story would have given it interest—thinks the ordinary reader—as the story advanced towards its close. But the mystery of Edwin Drood was no mystery at all, for the average reader.

To any one, however, who has studied the development of Dickens's work, or who has noted how in his later stories certain ideas were more and more dwelt upon, and certain devices more and more relied upon for misleading the ordinary and interesting the thoughtful reader. The "Mystery of Edwin Drood" not only promised to be but *is* a most interesting story. One can tell what the real nature of the mystery is, but it is no commonplace mystery as commonplace readers imagine. In a recent number of the *Cornhill Magazine* the commonplace solution of the mystery is given. I propose here to examine the story from this point of view, beginning with the interpretation of the matter of the ring, which undoubtedly gives the correct clue, though many have been deceived by it.

A jewelled ring is given by Mr. Grewgious to Edwin, which he was either to give to Rose or return to Mr. Grewgious, according to the event of his interview with Rose. He decides to return the jewels to Mr. Grewgious. "Let them be," he thinks, "Let them lie unspoken of." "However distinctly or indistinctly he entertained these thoughts, he arrived at the conclusion, Let them be. Among the mighty store of wonderful chains that are for ever forging, day and night, in the vast ironworks of time and circumstances, there was one chain forged in the moment of that small conclusion, riveted to the foundations of heaven and earth, and gifted with invincible force to hold and drag."

How obvious to the ordinary reader the interpretation of this! "We know," says the *Cornhill* writer, "that Jasper had a precise knowledge of Edwin's jewellery, and exactly in accordance with that knowledge, Edwin's watch, chain, and shirt-pin were found at the weir. But Jasper could have had no knowledge of the ring, kept as it was in a case in Edwin's breast, unless indeed he examined his pockets after despatching him; which is unlikely, as plunder was by no means his object. It is almost certain, then, that the ring was buried on the body, and even if the action of the quicklime could destroy the case and the gold setting of the stones, it could not possibly affect the stones themselves, which were diamonds and rubies. These Mr. Grewgious could readily identify, and Bazzard could prove that the ring was delivered to Edwin. The ring or the stones, once found and identified, the accumulated evidence of Mr. Grewgious, Mr. Datchery, Durdles, Deputy, Mr. Crisparkle, Rose, and the opium-woman, would, we think, assuredly convict Jasper of Edwin Drood's murder, while his conscience-stricken appearance at the prospect of detection, when the first breath of suspicion fastened on him, would at once popularly condemn him."

All this is so obvious that one wonders how even a common-place reader can have imagined for a moment that Dickens would have introduced so common-place a feature into what he clearly intended to be an impressive story. So well worn an incident as the detection of a murder by some piece of jewellery or the like, remaining when body and clothes were destroyed by quicklime, would not have been introduced by Dickens at a time when, as we know from his letters and life, he recognised keenly the necessity for novel and striking points in the novel he was writing.

It is, as I have said, the idea that the "Mystery of Edwin Drood" was constructed on such commonplace lines which has led many, even among professional critics, to regard the novel as uninteresting. To one who feels the sort of end towards which the story was really tending, it is—even unfinished—among the most interesting of all the stories Dickens ever wrote. Studied in its true light, the story may be regarded as really telling all we need know to enjoy every page and every line; though the very interest thus found in it makes us the more regret that Dickens did not live to finish it. Though the actual end of the story is obvious to true students of Dickens (not obvious in the way suggested by the *Cornhill* writer), its very obviousness makes the mystery surrounding it deeper, and suggests the singular interest which would have resided in the circumstances attending the progress of the story towards its end.

I have already indicated, in my Essay on this subject (reprinted in Leisure Readings) the chief features of the conclusion for which Dickens was preparing, and which all who really understood him would be sure,—he knew,—to foresee. I here therefore only sketch the sequel in outline. It is clear enough that Jasper's plot has in reality failed, though he knows nothing of its failure. Edwin Drood is alive, and resolute to punish the man who meant to murder him. He means to work out this punishment himself, and in such a manner as shall make Jasper suffer infinite torture. He sets himself to watch over the coming in and the going out of the wretch who had planned his death while pretending an infinite regard for him. He seeks out all who can help, not to make Jasper's guilt clear—that is easy enough—but to make Jasper's punishment more terrible.

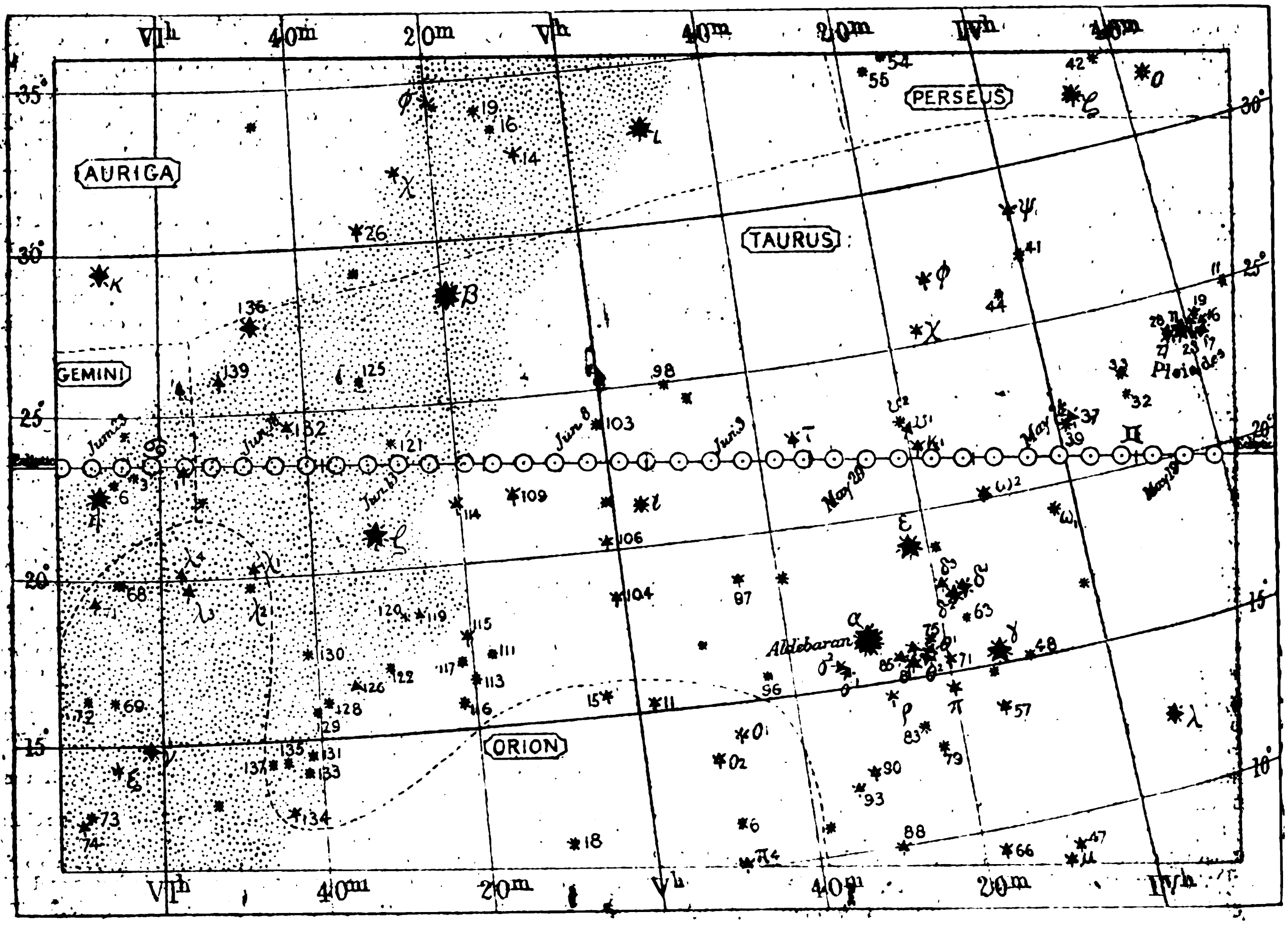
A reader can understand very little of Dickens's manner who does not feel that "Mr. Datchery" has an intense personal interest in bringing Jasper to punishment of the

condignest sort. All the *Cornhill* writer sees in Datchery is a detective. "Mr. Datchery we take to be a detective," he says, "employed by Mr. Grewgious to keep a watch on Jasper"; and that writer is so keen as to observe that Mr. Datchery's "white hair is unusually thick and ample, and he has black eyebrows, which is strange." Strange indeed if Dickens, who knew the ways and habits of detectives, had presented in Datchery so utterly incorrect a picture of so familiar a character. Datchery is manifestly a gentleman, to begin with; Inspector Bucket of the Detective Force comes about as near to being a gentleman as Dickens would have suffered a detective to do, and he is a long way off. But not to insist unnecessarily on so obvious a point, Mr. Datchery also feels deeply for Edwin Drood, not only in regard to Jasper's villany but also in another matter for which assuredly no detective would care. How confident Dickens must have been in the dulness of apprehension of commonplace readers, and in the sympathy of those who understood him, when he described the interview between Datchery, and the opium woman, so meaningless (it seems!) to one set of readers so full of meaning to the other:—"'What's the medium?'" asks Mr. Datchery, "'I'll be honest with you beforehand as well as after. It's opium.' Mr. Datchery *with a sudden change of countenance* gives her a sudden look. 'It's opium, deary. Neither more nor less. And it's like a human creature so far, that you always hear what can be said against it, but seldom what can be said in its praise.' Mr. Datchery begins very slowly to count out the sum demanded of him. Greedily watching his hands, she continues to hold forth on the great example set him. 'It was last Christmas Eve, just arter dark, the once that I was here afore, when the young gentleman gave me the three and six.' Mr. Datchery *stops in his counting, finds he has counted wrong*, shakes his money together, and begins again. 'And the young gentleman's name, she adds, 'was Edwin.' Mr. Datchery *drops some money, stoops to pick it up, and reddens with the exertion* as he asks: 'How do you know the young gentleman's name?' 'I asked him for it, and he told it me. I only asked him the two questions, what was his Chris'en name, and whether he'd a sweetheart? And he answered, Edwin and he hadn't.' Mr. Datchery *pauses with the selected coins in his hand, rather as if he were falling into a brown study of their value, and couldn't bear to part with them.* The woman looks at him distrustfully, and with her anger brewing for the event of his thinking better of the gift; but he bestows it on her *as if he were abstracting his mind from the sacrifice*, and with many servile thanks she goes her way." Note also especially what follows,—"John Jasper's lamp is kindled, and his lighthouse is shining when Mr. Datchery returns alone towards it. *As mariners on a dangerous voyage, approaching an iron-bound coast, may look along the beams of the warning light to the haven lying beyond it that may never be reached, so Mr. Datchery's wistful gaze is directed to this beacon and beyond.*" (The word "wistful" is applied to Edwin Drood in regard to the very circumstances about which Mr. Datchery is here thinking so wistfully,—"His wonted carelessness is replaced by a wistful looking at and dwelling on all the old landmarks." Chapter XIV.)

That any one could read these lines and for a moment imagine that Datchery is a detective, is difficult to understand. Yet Dickens himself knew well that among a hundred of his readers ninety at least would fall into just such a blunder. An idea even more cruelly silly has been started—that Datchery is Bazzard. Because Bazzard is away from Mr. Grewgius's office on special business, some readers cleverly jump at the notion that this business is no other than what Dickens called (in speaking of his story to Forster) the Datchery assumption. That any one could so thoroughly miss the force of Dickens's description! And description was Dickens's forte! Bazzard a "pale, puffy-faced, dark-haired person of thirty, with big dark eyes that wholly wanted lustre, and a dissatisfied, doughy complexion that seemed to ask to be sent to the baker's—a gloomy person with tangled locks, and a general air of having been reared under the shadow of that baleful tree of Java, which has given shelter to more lies than the whole botanical kingdom." Datchery, on the other hand, evidently a favourite with Dickens, quaint, humorous, genial, and kindly. Bazzard, caring for no one but himself, confounded with a man who thinks wistfully of Edwin's troubles!

We have received the eleventh annual report of the Lunacy Law Reform Association, which comes opportunely after the recent *exposé* in the case of Mrs. Weldon, of the way in which the proprietors of private lunatic asylums can, at present, obtain patients. That the Lunacy Commissioners' work is, under existing circumstances, inefficiently performed is less the fault of the Commissioners themselves than of the *régime* under which they act. There can be little doubt that, after what has recently been brought so prominently before the public, some material change in the law must perforce be made, and that speedily. It seems incredible that so gross an infringement of the liberty of the subject can have taken place as has recently come to light under Statutes dating only from the present reign; and that any two personal friends of the owner of a private lunatic asylum (whose knowledge of psychological medicine may be about equal to their acquaintance with provincial Sanscrit) are able to insure a perfectly sane man or woman for the private pecuniary benefit of such owner, if their names only appear on the Medical Register.

Globe Lightning.—A peculiar case of globe lightning is related by M. C. Decharme, whose original investigations, in the region of hydro-dynamics and electro-magnetism, of this problem so ingeniously studied in other ways by Professor Bjerkness and Mr. Stroh, will be remembered by our readers. (See *Engineering*, vol. xxxvi., pp. 378 and 404.) On the evening of February 24th, a rain-and-hailstorm passing over Amiens, suddenly startled the inhabitants by one most vivid and unexpected electric flash immediately followed by violent thunder. This flash appears to have resolved itself into a number of glowing but harmless globules. For this division of the lightning M. Decharme has only one witness, who states that he saw the brilliant mass burst asunder and descend in various directions. The globules, however, were observed at seven different places, the most extreme being almost a mile apart, and were always seen under very similar circumstances. The lightning struck the theatre and pierced one window with an oblong hole about an inch long, the edges of which were very brittle, but not fused; it passed then as a small glowing ball of a bluish tint behind the scenes, narrowly missing several actors. It finally disappeared with a slight explosion, doing no further damage, and leaving no traces whatever. A similar flame was observed in two houses, which it entered probably through the open windows. At another place a woman standing at the open door saw a bluish flame, as big as an egg, fall from the rain-pipe; it exploded like a gun. The terrified woman rushed into the kitchen, and had time enough to sit down before she heard the violent thunder which startled the whole town. In a fifth house it came down the chimney, lifted up one of the stove hobs, and traversed half the kitchen in the form of a fiery globe of the size of an orange; there was again an explosion, but no damage, and no smell of ozone, just as in the other cases; the thunder followed afterwards. The blue flame was further noticed in a restaurant, and finally in another house, where it stretched a telephone wire, deranged the electric bell, then with a report passed into the house itself, and along the supper-table, where it hurt the hand of the master of the house, rendering it useless for some minutes, and finally disappeared mysteriously. The lightning thus behaved like a water jet falling upon some solid body, and spirting out in little globules in all directions; these globules were hurled away and showed no preference for good conductors in their descent. The whole occurrence is very peculiar, but seems to point to a certain relation between electric and liquid currents, for which Professor Decharme has brought many an unexpected experimental proof. We hope on an early occasion to acquaint our readers with the results of further recent investigations of M. Decharme.—*Engineering*.



The Day Sign for the Month.

ZODIACAL MAPS.

By Richard A. Proctor.

We give this week both the day sign and the night sign for the month, one showing the zodiacal sign now high in the heavens at midnight, the other showing the region of the zodiac athwart which the sun pursues his course at this part of the year.

OPTICAL RECREATIONS.

By a Fellow of the Royal Astronomical Society.

(*Continued from p. 437.*)

We will make two more experiments in the reflection of light, and with them conclude our general exposition of that branch of our subject. The study of Figs. 6, 10, and 11 will have familiarized the student with the method of drawing incident and reflected rays. Let him then describe an arc of a circle and from a point at the greatest convenient distance outside of its centre of curvature, draw rays to all parts of the interior of the curve. These will be divergent rays incident upon part of a cylindrical concave surface, of which the arc is a representation. Now (by erecting perpendiculars at the points where these rays impinge on the curve) draw their reflections, and prolong these reflected rays until they intersect. It will be noted that these intersections take the form of a kind of double crescent. This peculiar curve is known to mathematicians as a "caustic." Having satisfied himself from his figure that a curve of light of this form should theoretically appear under the circumstances, the beginner should procure a small piece of board and a short bit of watch-spring.

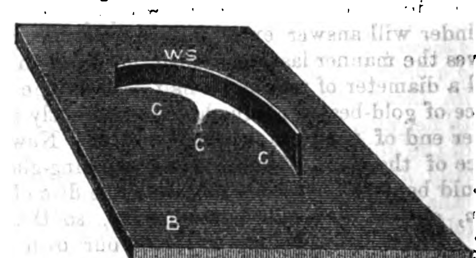
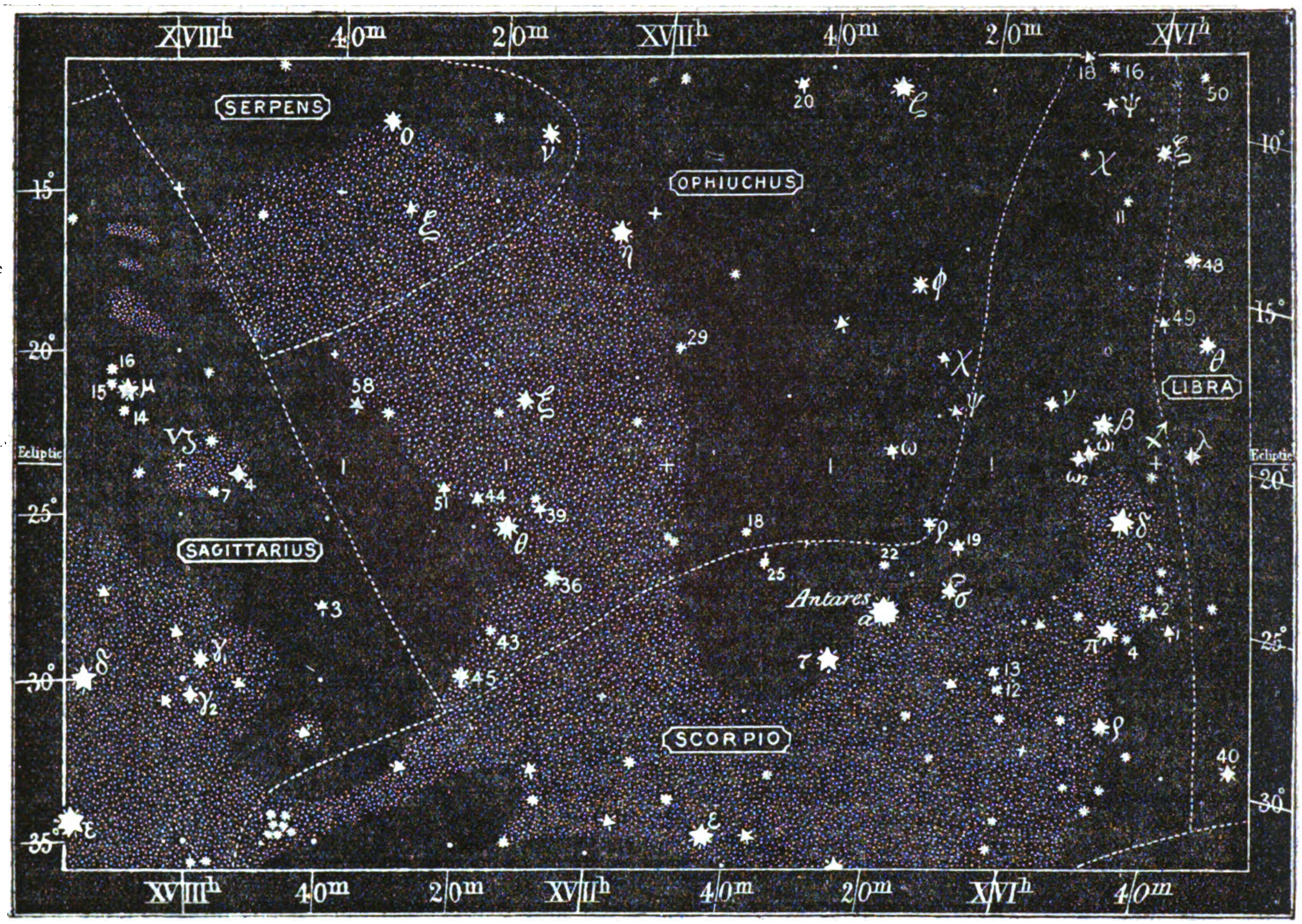


Fig. 13.

The board (B, Fig. 13) must have a sheet of note-paper pasted down on to it, and a curved groove should be cut on it, when the paste is dry, to hold the piece of watch-spring, W S. If now the board be held edgeways to the light, the beautiful curve C, C, C, will at once become visible. Caustics may also be seen on the surface of milk or weak tea, nearly filling a white cup, when the sun is shining upon that narrow portion of the inner edge of it above the surface of the fluid. They may also be made to appear in the interior of an empty china bowl by suitably inclining it; and are formed, too, by refraction—of which, though, we have as yet to speak.

Our final experiment will be performed by the aid of a little bit of apparatus, which the student may also construct for himself without any difficulty. The first thing to make



The Night Sign for the Month.

or obtain is a piece of tubing. One of the tin tubes in which plants, &c., are sent through the post, or a pasteboard one made by rolling pasted brown paper round a wooden cylinder will answer excellently. One constructed by ourselves the manner last mentioned, has a length of five inches and a diameter of two and a-half. Over one end of this a piece of gold-beater's skin should be tightly stretched, the other end of the tube being left open. Now a very small piece of the thinned and lightest looking-glass procurable should be gummed in the middle of the disc of gold-beater's skin, of course by its silvered side, so that the reflecting surface faces outwards. In our own case, failing to obtain a sufficiently light piece of looking-glass, we have made one by taking one of the smallest circular discs of thin glass used for covering microscopic objects. This we cleaned carefully, dropped a single drop of mercury on to a piece of new tinfoil, gave it one brush over with a feather, and rapidly placed the thin glass cover upon it, superposing a weight to squeeze out the superfluous quicksilver. After a few hours, the quicksilver-and-tinfoil back was firmly adherent to the glass cover, and we had a little mirror of unsurpassable lightness. This we gummed on to the gold-beater's skin, as mentioned above, and our apparatus was complete. It is used by so arranging it that the sun may be reflected from the little mirror on to a wall or ceiling as a spot of light, and then singing into the open end of the tube. This may involve the experimenter lying on his back on a couch, or even on the floor; but he must select his own position, the only requisite being that as he holds the tube before his mouth, the piece of looking-glass, at his distal end, should reflect the spot of sunlight on to a white wall or ceiling. Let him now, as we have just said, sing into the tube, and the result, if witnessed for the first time, can scarcely fail to surprise and gratify him, for the spot of light will at once be transformed into a beautiful re-entering curve, varying with every note that is sung, the fantastic forms assumed being as pretty as they are instructive to regard.

Having thus, then, as we hope, experimented upon the reflection of light in a number of sufficiently varied ways to familiarize the student with its characteristic phenomena, it only remains, before passing to the subject of refraction, to see how far such phenomena are explicable in the two hypotheses of the nature of light, of which we spoke in p. 352. Now, the way that Newton accounted for the phenomena of reflection, in his "Corpuscular Theory of Light," was this. He supposed that all the material particles in the universe possess, in addition to the attractive force of gravitation, other forces of attraction and repulsion; the repulsive force extending farther from the particle than the attractive, so that light was repelled while passing through the outer or repulsive sphere, and attracted when it got through it. To put the matter in a very homely way, it is as though every material surface were covered with a perfectly elastic film of repulsive varnish, from which the particles of light rebounded; in which case, as every billiard-player knows practically, the angle of reflection would be equal to the angle of incidence, and the velocity of the light would remain unchanged. Thus, then, may reflection be explained. But, as Sir John Herschel most

pertinently remarks, it is explained *too well;* for, if the theory were sound, how in the world could there be such a thing as partial reflection? But, as we know well, nearly all the reflections we witness is partial. Everybody is aware that the image of his own face in a looking-glass is duller or less luminous than his face itself; that the reflection of the landscape from the surface of still water is much less bright than that landscape; and that, in the case of repeated reflections such as those which occur in the toy figured 7, on p. 390, the light is so degraded by its passage backwards and forwards as to be ultimately extinguished altogether. Sir Isaac Newton, however, got over this difficulty (sooth to say in a somewhat lame fashion) by the hypothesis that the particles of light were, as it were, inconceivably minute magnets rotating about axes non-coincident with the direction of their motion through space. This being so, quite obviously some of them would arrive at the reflecting surface with their attractive poles towards it, and some with their repulsive poles. The former would pass through the repulsive or reflective layer; the latter would be repelled or reflected by it. We have—following Sir John Herschel—put Newton's hypothesis into a modern and apprehensible form. What he really did assume was that the light particles travelling through space passed periodically through what he called "fits of easy reflection and easy transmission"—which we can only hope (without being at all sanguine) the reader finds satisfactory and intelligible. It is only needful to say in concluding our reference to this hypothesis that, were it true, the light which passed through the repulsive medium would, in virtue of the joint attractive force of its own particles and those of the medium (supposed denser than that filling space) be accelerated; whereas it is now known as the result of actual experiment that the velocity of light is *retarded* in passing from a rarer medium into a denser one—as from air into water or glass or the like—thus showing demonstrably the inaccuracy of the corpuscular hypothesis. Under certain circumstances, to which more appropriate reference will be made in treating of refraction, total reflection does take place; and we shall hereafter show how it is observable. How reflection is explicable on the Undulatory Theory of Light it will need but few lines indeed to point out. Assuming light to consist of a wave in the ether (p. 352) running out in all directions from the luminous point, a ray of light will be an imaginary line from such point square to the front of the advancing wave. Evidently if the source of light be near to the eye, the perpendiculars from it to the wave surface will diverge conically; but in the case of bodies at a practically infinite distance, like the stars, the rays will reach the eye sensibly parallel. If we drop a pebble into the middle of a calm pool we shall see a gradually enlarging ring travel outward from the spot where the stone entered the water. If this ring impinges against a perpendicular dam, or lock gate, or against a board purposely held vertically in the water, the ring will be reflected the instant it touches such perpendicular surface, and from the point of contact will spread back another ring, whose centre will be as far in on shore as the original centre of disturbance was from it in the water. This very simple experiment, which may be performed in any pool (or even big trough or tank) whatever, should be made by the reader himself, as from it he will gather a more vivid idea of the propagation and reflection of a wave than from a column even of illustrated description. So much for the reflection of light. In our next paper we shall commence the consideration of its refraction or bending, and show how such bending is at the bottom of some of the most curious and important optical phenomena with which we are familiar.

Reviews.

THE SAGACITY AND MORALITY OF PLANTS.*

Any one who may be labouring under the delusion that botany is a dry and unattractive study, or who may conceive that it only busies itself with the classification and nomenclature of dried plants in herbaria, should forthwith procure the perfectly delightful work whose title appears above. It will be strange indeed if, having once opened it, he does not read it straight through from the preface to the colophon. At first sight the title may appear of a figurative—not to say hyperbolical character; but the perusal of a very few of Dr. Taylor's most interesting pages will show that he is in earnest in his application to the vegetable kingdom of attributes commonly assigned to the animal world only. Nor is his idea so far-fetched as may be supposed. Every microscopist is familiar with forms of vegetable life so perfectly simulating lowly animals in their form and actions, as to have been classed by Ehrenberg and others among infusoria or animalculæ. In fact, some of them (like the Volvox Globator) have been tossed from the botanist to the zoologist and back again, like shuttlecocks. Who, on his first view of the pretty Euglena viridis, with its green body, transparent neck and red eye, threading its way through the water and carefully avoiding all obstacles, has doubted that he was observing a true animal? While, on the other hand, the so-called Amœba, which consists of a structureless mass of protoplasm flowing round its food and digesting it indifferently with its outside or inside, might be a drop of coagulating sap for all the structure or intelligence it shows; and yet we know it to be an animal, and the euglena to be a vegetable. By a thoroughly legitimate use of the scientific imagination, then, our author propounds the idea that "there can be no life absolutely without *psychological* action," and proceeds to discuss the manners and customs of plants upon this assumption. In fourteen chapters, one of them being introductory, he treats successively of "Misunderstandings," "Stating the Case," "Wood-craft," "Floral Diplomacy," "Hide and Seek," "Defence, not Defiance," "Co-operation," "Social and Political Economy of Plants," "Poverty and Bankruptcy," "Robbery and Murder," "Turning the Tables," and the "Geographical Vicissitudes of Plants." A hundred excellent woodcuts in the text, and a coloured lithographic frontispiece representing the edge of the leaf of the carnivorous Drosera Rotundifolia, amply illustrate this charming book. We had marked some passages for extract, but upon second thoughts will not deprive the reader of the pleasure which he will derive from their fresh perusal in the pages of a volume which we heartily recommend him to obtain as soon as he possibly can.

SOME BOOKS ON OUR TABLE.

Strains on Girders, Arches, and Trusses; with a Supplementary Essay on Economy in Suspension Bridges. By E. W. Young. (London: Macmillan & Co.)—To everyone who remembers (and who does not) the ghastly accident to the Tay Railway Bridge on the 28th December, 1879, the importance of the subject of Mr. Young's work must at once become manifest. In it he has treated, as exhaustively as he has simply, the subject of the strains on girders, &c., in the most practical possible way. A know-

* "The Sagacity and Morality of Plants. A Sketch of the Life and Conduct of the Vegetable Kingdom." By J. E. Taylor, Ph.D., F.L.S., F.G.S., &c. (London: Chatto & Windus. 1884.)

ledge of some elementary algebra, and almost equally elementary trigonometry, will enable the reader to work out every one of the problems contained in the book; which should be upon the shelves of all concerned with the use of iron as a constructive or architectural material.

Handbook of Competitive Examinations for Admission to every Department of Her Majesty's Service. By W. J. CHETWODE CRAWLEY, LL.D., corrected for 1884. (London: Longmans, Green, & Co.).—This able and honest compilation will be invaluable to that vast number of parents who are just now asking themselves "What shall we do with our boys?" containing as it does the most exhaustive information as to the pay, &c., and conditions of admission into every branch, civil and military, of Her Majesty's service. The portion relating to admissions to various posts in the Science and Art Department, on pp. 115 *et seq.*, is most instructive; and the British tax-payer may well compare the salaries given to (and the acquirements demanded from) the working bees of the giant hive at Brompton, with the sums received there by the relatives, friends, and hangers-on generally, of the late Cole, C.B, as given (not even to their full extent) on pp. 138 and 139 of *Whitaker's Almanack* for the current year.

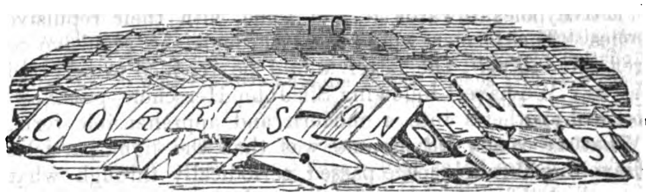
Celestial Motions. A Handy-book of Astronomy. By W. T. LYNN, B.A., F.R.A.S. (London: Edward Stanford, 1884.)—The chief value of this really handy little book of Mr. Lynn's lies in the fact that it furnishes the student with the very latest and most trustworthy numerical data of the bodies composing our own solar system. It gives also useful *précis* of all that is known concerning comets, meteoroids, and the fixed stars. This is followed by a short sketch of the history of astronomical discovery, and the work concludes with a glossary of the technical terms employed in it. We note two little slips, or misprints, which Mr. Lynn will doubtless correct in his inevitable second edition. On p. 6, line 2, "elliptic" should be ecliptic; and in the fifth line of p. 33, "slow" appears, when it is pretty obvious that the author meant quick. Such tiny blemishes as these, however, obviously in no way detract from the value of a work which should be in the possession of every student of astronomy.

Workshop Receipts. Third Series. By C. G. WARNFORD LOCK. (London: E. & F. N. Spon, 1884.)—This third series of Spon's well-known "Workshop Receipts" forms a kind of handy encyclopædia of electrical and metallurgical matters generally. The work is eminently practical, the descriptions are clear, and the illustrations, if not works of the highest art, abundantly suffice to elucidate the text. The explanations of the modes of construction of phonographs, photophones, microphones, and the like, are very well given and interesting, while the articles on lacquering, tempering, &c., will be found useful to the amateur. This volume should certainly find a place in his library.

Practical Guide to Photography. By MARION & Co. (London: Marion & Co., 1884.)—The work before us, handsomely got up, and well illustrated, leaves nothing to be desired as a tutor for the incipient photographer. In addition to explicit directions for the production of negatives and the subsequent printing from them, clear explanations are given of various photographic "dodges," such as retouching, printing in skies, combination printing generally, vignetting, and so on. The careful reader of Messrs. Marion's book should experience no difficulty whatever in his earlier essays in the art of photography.

The Civil Service History of England. By F. A. WHITE, B.A. Revised and enlarged by H. A. DOBSON. (London: Crosby, Lockwood, & Co.)—Here are the facts of English History, boiled down in a way little short of marvellous. The diligent student of Messrs. White and Dobson's quintessence of our national annals would doubtless be a match for any Civil Service examiner. Whether though, having triumphantly answered all the questions set, and gone on his way rejoicing, he would in any legitimate sense have learned or studied *History*, is a matter on which a legitimate difference of opinion may exist.

The Civil Service English Grammar. By W. V. YATES, C.M. 2nd Edition. (London: Crosby Lockwood & Co., 1884).—This is another "cram" book; but it is something more; containing, as it does, a mass of information on the structure of the English language. Having let off the knowledge with which he has been loaded up to the muzzle from Mr. Yates's book, the victim of the present quasi-Chinese craze for competitive examination may revert to it with profit, as there is more in it than he will have been able to utilise in the examination room.



"Let Knowledge grow from more to more."—ALFRED TENNYSON.

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THE FLINT-FOLK'S FLOOD.

[1309]—Two words in my "*Noah's Rainbow*" argument, kindly allow me to correct. In p. 442, thirteenth line from bottom, "palæozoic" should be *palæolithic;* and in 440, fourth line from bottom, "veritable" should be *habitable.* Higher in that column I said it was not possible more than five degrees outside the tropics to see the moon with both horns on a level, but it is possible, though extremely rare, in higher latitudes. It may happen to the setting new moon of February or March, if at her greatest north latitude when about three days old; or to the rising morning moon of October or November, if at the greatest north latitude when twenty-six days old—in short, on a couple of evenings and of mornings in eighteen years.

You think Sir G. Airy's ideas on Genesis admissible, because he "cannot doubt the Flood to have been an overflow of the Nile." I have just as little doubt of that fact; but if it was the real Deluge, as plain from surface geology everywhere, then it was a simultaneous overflow of both the Nile and every other river on the globe. *That* is the hard fact to be dealt with, and not Noah nor Genesis, which may be solar myths if anyone likes. Their being so will not, I suppose, make the Flint-folk and *their* "palæolithic Flood" of fifty centuries ago, solar myths. Sir G. Airy's amazing ideas, coolly ignoring all facts of drift geology, really look as if all turned on the singular fact of Greenwich Park happening to be, like no equal tract within many miles, destitute of diluvial drift.

I was not aware of any "Egyptian account" of the Flood, but supposed they were the only nation in classical times who denied such an event; their priests having, to make out their fabulous antiquity and preposterous tales of long dynasties, allegorised away all the facts about Osiris (who at first was identical with Noah, but at last with the sun), and that was why I cited from Plutarch those particulars so exceeding un-solar, but rather Noachian. I should much like to hear it explained how the sun came to be credited with such an adventure as getting murdered in a box of wood, carried and thrown into the Nile on a certain day of the second moon of autumn—no lunar phase day, but the 17th—drifted out to sea, picked up after stranding at Byblus, in Phœnicia, and finally cut

into pieces, which became various nations. How very like the sun! But Noah, in Genesis, is ordered to enter his ark, and "because of the waters of the Flood" he does so, and is then "shut in" by the downpour of them, on the 17th day of Chesvan. On the same day of the same month, Osiris was said to be allured by a treacherous enemy, Typhon (a name for the Flood), to get into a chest, whereupon the said Typhon shuts him in, murders, and hermetically seals him by pouring in melted lead; and the chest is sent down the Nile, drifts out of its Tanaitic mouth (which, for this reason, was abominable to name), and only was rediscovered in Northern Syria, long after stranding at Byblus. Now, that port was exactly where the straight issue of the Tanaitic mouth, continued to Ararat, would cross the present coast, or enter upon Asia. I cannot see why, then, this should not be a genuine tradition, so far as it goes, of part of Noah's real course; probably the middle part only. He may have come from as far south of Thebes (where his son, Ham, afterward settled) as his grounding was north of Byblus, but must apparently have drifted over these two places.

The length of the ship that was venerated at Thebes, by the way, is not given by Plutarch as 250 cubits roundly, but an exact number of "Royal cubits," I think 258, which would be 301 common, the Royal being to the popular cubit as seven to six.

The only instance I ever heard of a story being made to explain pre-existent pictures, as you suppose Genesis made to suit the constellation pictures, was the poem of "Doctor Syntax." But if there were fifty such, I should adhere to the old view of constellation-making, that it was to illustrate things remembered or believed, not *vice versâ*.

In reference to a probable source of the diluvial waters, which might of course be, as Halley said, from a comet (and, for aught that yet appears, from the smallest known), a marvellous error of computation is ascribed to Faye, in Guillemin's "Heavens" (English edition, 1871, p. 261), where a supposed comet of "the seven hundredth part of the *bulk*"—which must mean the *mass*—"of the earth," is said to be only equivalent to "the weight" (a bulk cannot be equal to a weight, which would be nonsense) "of a sea of 40,000 square miles, 100 yards deep." If the miles are English, the comet would weigh above 40,000 such seas; and, if German, about 2,000. In either case it would be about *sixty times* the quantity of water that seems to have fallen on the "Flint-folk"—namely, the top hundred fathoms of our present ocean.

E. L. Garbett.

[Mr. Garbett's theology is very much better than his geology. He writes as though all diluvial deposits were coeval, just as some people seem to imagine that the various geological formations surround the globe like the coats of an onion. Besides, if there were the slightest foundation for my correspondent's (wholly imaginary) "hard fact," how comes it that the loose scoriæ, cinders, and lapilli on the volcanic mountains of Auvergne and Languedoc still lie undisturbed on the sides of the cones? (vide Scrope's "Volcanoes of Central France," p. 206, Lyell's "Antiquity of Man," &c.) As for Halley's nonsense about a comet having caused the Deluge, it has yet to be shown that an aqueous comet ever existed. Mr. Garbett is assuredly behindhand in his study of the literature of this branch of his subject, and writes as though Adams, Schiaparelli, Huggins, &c., had never existed. With regard to constellation-making, it appears that every race has (in its childhood) imagined figures of things known to them, among the stars, just as most children imagine shapes of familiar objects—toys, animals, and so forth—among the same stars, or in the fire, or the like. After the pictures were made, stories would naturally be made to fit them, just as stories have been made to fit names of places, &c., whose origin has been forgotten. That the sun's course along the zodiac should be associated with the story made to explain the water-streams of Aquarius, the sea-monster, the ship *Argo*, the Raven, the man offering sacrifice, the Altar, and the Bow of Sagittarius, is the most natural thing in the world.—Ed.]

NOAH'S RAINBOW.

[1310]—In Mr. E. L. Garbett's letter on the above subject, he has, I believe, made at least one grave mistake. In Genesis xi. 2, it distinctly says that it was as they journeyed *from the east* that they arrived at the plains of Shinar, so that the modern Mount Ararat could not have been the spot where the ark rested after the flood. This passage evidently points to a district east of the Euphrates. The traditions of the Persians, the Hindoos, the whole Aryan family, point to this district, and the Chinese traditions also point to the mountains of Little Bokhara and Western Thibet, as the place whence the human race issued. If this was really the place, and both the Bible and tradition point to it, then the theory of the Nile having anything to do with the Flood must be erroneous.

I have always imagined that the Flood occurred at the close of the last Glacial Epoch, but whether it was 10,000 or 50,000 years ago that it happened I have never been able to make out. Man had probably appeared during one of the inter-glacial periods (perhaps evolved out of the monkey tribe by the survival of the fittest during the previous Glacial Epoch, but the discussion of whether man was created by the Almighty, or evolved out of lower forms of life by laws made and sustained by the Almighty, is outside the range of the subject) at a time when the Northern hemisphere was clothed in a tropical climate and the south covered with a thick coating of snow and ice. With the whole of the northern hemisphere enjoying a temperature equal to that of India at the present time, man would have no need of clothing other than his natural covering of hair, and would be able to satisfy his hunger's desires with the natural fruit of the district.

Cultivation of the ground was unnecessary, and therefore unknown. Everything being favourable to his existence, man must have rapidly increased, and spread over the greater part of Asia and Europe, and even penetrated into North America by Behring's Straits. But when the glaciers began to disappear from the south and appear on the northern hemisphere, mankind would be obliged either to retreat southward to the equator or to remain further north, and only the strongest and hardiest survive. The struggle for existence would be so severe in the northern countries that civilisation could make no headway, while in India and on the south coast of Asia it was gradually increasing. The tribe or nation to which Noah belonged may, and probably was, far ahead of the cavemen in intelligence and civilisation. The first effect of the reduction of temperature in the northern hemisphere would be the necessity for clothing. Tradition has afterwards invented the theory of the fall of man to account for mankind commencing to wear clothing. I do not mean that man has not fallen, for he undoubtedly has, but that man's sinfulness and immorality had nothing to do with his commencing to clothe himself. The most degraded and immoral tribes are just those who don't wear clothing at all. All through the glacial epoch the struggle for existence rapidly thinned out the population. At the close of the epoch gigantic floods, caused partly by the melting of the glaciers, and partly by earthquakes and alterations in the level and height of the land, swept over the country, floods that covered half a continent at once, drowning every living being in their path. One of these floods, sweeping over the South of Asia, destroyed and annihilated every tribe, every man, every beast, with the exception of one family who escaped with some animals on a raft. This was another instance of the survival of the fittest; Noah was the best or fittest specimen of mankind at that time, as he could, by being warned by the Almighty, make preparations to avert the danger. This was one out of thousands of instances in which nature has interfered in an important manner to promote the welfare of mankind. The tradition handed down from generation to generation of the far distant time when man had not to struggle for existence would, by contrast with the severity of the Glacial Epoch, be exaggerated into a perfect Paradise into the Garden of Eden. Distance would lend enchantment to the view. These traditions would also naturally say that the earth had become unfruitful as punishment for the fall of man, as among savage nations calamities are always considered to be sent as punishment for their misdeeds.

I have always taken exactly the opposite view to that entertained by Mr. Garbett concerning the likelihood of Noah having seen the rainbow before the flood. It was only the token to show Noah that the earth would not again be destroyed by a flood.

Whenever in ancient times an agreement was settled between two persons or parties, some token was required to remember it by. It was said to Abraham that circumcision was to be a token of the covenant between Me and thee. When the promise was given to Noah he would expect some token, some outward symbol of the promise, and what could be more appropriate than the rainbow? If he had not seen it before, whenever a storm occurred in which a rainbow did not appear, they would at first believe that a second flood was impending, until, finding that the weather always cleared, whether the rainbow showed or not, they would soon begin to despise it and the covenant. But if he had often seen it before, and knew that it did not always appear during a storm, he would not afterwards expect it every time the sky became cloudy, but it would simply have a fresh meaning for him and his descendants.

Harold Rowntree.

[Having inserted Mr. Garbett's letter I do not like to exclude yours. On it I would however make two remarks. The first is that you speak as though the Flood were absolutely universal; which it certainly was not; the traditions of deluges in various countries certainly referring to cataclysms which occurred at very different times; and you must remember that you had already peopled North America *viâ* Behring's Straits. And next I would

remark that as the last Glacial (or rather cold) period in Northern latitudes occurred between 12,000 and 13,000 years ago, I fail to see how you can reconcile this with the perfectly definite chronology to be derived from the genealogies of Noah's descendants given in the Bible. I have spoken of a "cold period" having occurred between 12,000 and 13,000 years ago. The last periods of *intense* cold, however, or Glacial epochs proper, happened as nearly as may be 100,000 and 210,000 years ago; and man was, as you rightly surmise, pre-Glacial or anterior—at all events—to the former of these.—Ed.]

[1311]—Your correspondent Mr. E. L. Garbett has a very ingenious account of Noah and the Flood; however, your simple record of tracing it to the solar myths of Eastern civilisation appears to me the only rational and the only possible explanation of it. Mr. E. L. Garbett does, according to his own showing, make the date of the Flood come about fifty centuries ago, if I understand him right; now, the ancient monuments of Egypt which are still standing can be traced clearly up to just about the same time, if not to an age even more remote. Does Mr. E. L. Garbett maintain that these monuments date from before or since the Flood? If before, do any one, of them show that they stood submerged for over a twelvemonth under a flood which covered the highest mountains in the world? If built since the Flood, would that not bring the date of this event, if ever it took place, at the very least, back to another fifty centuries? That time would be required if Ham had to develop into an Egyptian nation after the Flood.

F. W. H.

[1312]—There are certain people nowadays who, while conscious of the force with which the battering-ram of Science is thundering against the battresses of ancient theology, endeavour by the aid of extraneous props to bolster up a structure which is rapidly crumbling to dust. They seem, however, to overlook the fact that in stepping beyond the letter and the spirit of that which is written, in their vain endeavour to repair an ever-widening breach, they are actually stultifying the very Scripture they are so anxious to support. This they do not only by insisting upon things which are not in the Bible at all, but by insisting upon things which are directly contrary to the teaching of the book itself. Mr. E. L. Garbett's letter is a very fair specimen of this kind of work, which just now forms a prominent feature in polemics. As it is inconsistent with science that Noah's rainbow was the first rainbow, though the Mosaic account clearly teaches this, Mr. Garbett labours to show that it was simply the first that Noah had seen, or, at least, the first that had been seen by his "*young* disciples," those infant sons of his of a hundred years of age. Yet, what saith the Scripture? God said to Noah, "And it shall come to pass, when I bring a cloud over the earth, that the bow shall be seen in the cloud. . . . . And the bow shall be in the cloud, and I will look upon it that I may remember the everlasting covenant," &c. (Gen. ix., 14, 16.) Is it not perfectly clear that the sapient Hebrew who wrote this fully intended it to be understood that this was actually the very first rainbow? Had he thought that rainbows were (then) as common as sunsets would he have made God say "I will look upon it that I may remember," &c.? It is inconsistent with science that all the creatures on the face of the earth to-day are the direct descendants of the various pairs Noah took with him into the ark. Mr. Garbett finds it difficult, by this theory, to account for indigenous species existing on remote islands, so he conveniently concludes that "they are simply those . . . . that in the rush of the Deluge happened to be stranded thereon." Again, he speaks of animals clinging to "flotillas of matted forests," and of others preserving life in caves. Yet what saith the Scripture? "And behold I, even I, do bring a flood of waters upon the earth, to destroy all flesh wherein is the breath of life, from under heaven; and everything that is in the earth shall die" (Gen. vi. 17.) "And every living substance was destroyed which was upon the face of the ground, both man and cattle, and the creeping things and the fowl of the heavens: and they were destroyed from the earth: and Noah only remained alive and they that were with him in the ark" (Gen. vii. 23). There is not the slightest foundation in "Holy Writ" for the wild hypothesis that the ark was built in equatorial Africa, and was sent, rushing with railway speed, down the valley of the Nile; or that its stranding was caused by the volcanic upheaval of a shoal. What saith the Scripture? "And God made a wind to pass over the earth and the waters assuaged . . . . And the ark rested in the seventh month on the seventeenth day of the month upon the mountains of Ararat" (Gen. viii. 1 and 4.) Mr. Garbett speaks of the Mosaic account of the Deluge as a record of Noah. If it is so, the expression, "Mountains of Ararat," is most significant. It shows that in those days, as in these, there were at least two mountains; and, as the lesser of the two is 13,093 ft. above the level of the sea, the fact of the ark resting there would prove the words of the account—"And the mountains were covered" (Gen. vii. 20). That Noah (if he is the recorder) had a proper appreciation of the difference between "mountains" and "high hills" is proved by the use of both terms in the text; and is it at all likely that he would have used either if he had landed on a shoal at our present sea level?

No amount of straining will make the Biblical account of the Deluge accord with science or with the light of modern research; and those who feel that it is imperatively part of a whole which is necessary to their happiness must accept it as it is, reconciling their faith with their reason as best they may. But to propose such hypotheses as those Mr. Garbett has put before us is, as I have said, to stultify the very scripture he is concerned to uphold, for he does not argue from the Book—he contradicts it.

A. M. D.

"TOO MUCH OF WATER HAST THOU, POOR OPHELIA."

[1313]—Mr. Garbett, in his letter on "Noah's Rainbow," says (speaking of the Deluge):—

"This event, the last *world-wide* one, cannot be described as less than the fall of a fresh sea from the sky—a single storm wherein enough water fell to raise the general ocean-level some 600 ft." . . . . "The tremendous down-pour (averaging some 400 ft. *universally*").

I have been told, sir (whether correctly or not I cannot say), that rain is caused by the aqueous vapour, evaporated from the surface of the earth and water by solar heat, and absorbed by the atmosphere, being condensed by cool air. When this occurs, as I am informed, rain falls.

Now, sir, it seems to me that, antecedently to the Deluge an extraordinary process of desiccation must have been going on all over the earth's surface for the amount of moisture represented by the Flood to have accumulated in the atmosphere.

That abnormal atmospheric conditions must have existed to enable the air to absorb such an enormous amount of moisture, and to cause such moisture to be *universally* precipitated, *at once*, as rain.

I believe, sir, that it is held that the existing physical laws which enable us to explain the natural phenomena we see around us at the present time are also sufficient to account for those which have occurred in the past. I am thus placed in a difficulty so far as understanding the cause of an universal deluge is concerned, and as I have not sufficient leisure time to allow me to study up such matters as these, I apply, sir, to you in your kindness, or to some one of the many readers of your valuable journal, for assistance to help me over what I doubt not is but a *pons asinorum*.

A Working Man.

COINCIDENCES.

[1314]—In the course of my life I have met with many remarkable coincidences. The following is one:—

A few years ago I was living at a country town in England, and had arranged with a friend to spend a certain day at a house in the neighbourhood, to which we were to go by train. On the morning of this day I awoke with a most unaccountable sense of oppression and impending evil, which affected me so strongly that when my friend came for me I told him I could not go with him, but would follow in the afternoon. I had no idea of fear or danger; simply an undefinable *something*, which seemed to paralyse me, and render me powerless to move without a very painful effort. My friend tried hard to persuade me to accompany him, but had to go without me. A few hours afterwards I heard that the train we should have gone by had met with a bad accident, and that the only second-class carriage (which fortunately was empty) had been smashed to pieces. Now, my friend always travelled second-class, and I used to do so too when we were together. To my great relief, I found that the time he had spent in trying to persuade me had made him miss the train, and he had had to wait for a later one. Thus a fit of the blues (a thing I am by no means subject to) was certainly the means of saving us both from severe injury, if not from death.

I have taken your paper from the first, and wish it every success.

Musafir.

[1315]—Seeing the interest which many of the readers of Knowledge are taking in strange coincidences, perhaps you will think the following worth insertion:—

About twelve months ago the captain of a gymnastic club, to which I belonged, resigned, and it was expected that a gentleman whom I will call A. would be elected.

I was returning from the club one evening, when the thought

suddenly struck me that he would refuse election, and I felt that my sister—who was at home at the time—had just expressed herself to the same effect. On arriving, I asked her if she had spoken about A. that evening; she said that about five minutes before she had said to her aunt she thought he would refuse the captainship. No other remark was made.

I may mention that A. and my sister have never spoken, and I only know him slightly. We were both wrong. C. E. DOYLE.

"PEASE-PUDDING IN THE POT NINE DAYS OLD."

[1316]—In No. 106 appeared one of W. Mattieu Williams' excellent articles on the "Chemistry of Cookery," in which he recommends the use of seven days' old oatmeal porridge (we call it "stirabout" here).

Well, sir, I have tried the experiment. I set aside a bowlful of good porridge. At the expiration of a week I brought it out, expecting a treat. Carefully removing the inch-long beard with which it was covered, I explored its interior, the state of which beggared description, the smell being such that I ordered its removal in (I fear) unparliamentary language. The proof of the pudding is, no doubt, in its eating; well, I did not eat it, but my pigs did, and to them I must refer for further information.

Sincerely hoping that the friend of whom Mr. Williams speaks may long live to enjoy his good skinful of ensilaged porridge.

Harryville, Ballymena. ALBERT DAWSON.

CAN THE SEVERED HEAD THINK? (1244)

[1317]—The following, which is taken from *Health*, No. 40, Vol. II., Jan. 11, 1884, may interest your correspondent:—"Dr. de la Pommerais," says the Paris *Figaro*, "was executed in June, 1864, for a murder of the Palmer type. On the night before his execution he was visited by Surgeon Velpeau, who, after a few preliminary remarks, informed him that he came in the interests of science, and that he hoped for Dr. de la Pommerais' co-operation. 'You know,' he said, 'that one of the most interesting questions of physiology is as to whether any ray of memory, reflection, or real sensibility survives in the brain of a man after the fall of the head.' At this point the condemned man looked somewhat startled; but professional instincts at once resumed their sway, and the two physicians calmly discussed and arranged the details of an experiment for the next morning. 'When the knife falls,' said Velpeau, 'I shall be standing at your side, and your head will at once pass from the executioner's hands into mine. I will then cry distinctly in your ear: Couty de la Pommerais, Can you at this moment thrice lower the lid of your right eye while the left remains open?' The next day, when the great surgeon reached the condemned cell, he found the doomed man practising the sign agreed upon. A few minutes later, the guillotine had done its work, the head was in Velpeau's hands, and the question put. Familiar as he was with the most shocking and ghastly scenes, he was almost frozen with terror as he saw the right lid fall, while the other eye looked fixedly at him. 'Again,' he cried frantically. The lids moved, but they did not part. It was all over." A. McD.

THE DIVIDED SKIRT.

[1318]—While cordially agreeing with much that is said in the article with this title in KNOWLEDGE for the 13th inst., there are one or two statements which I should be glad to be allowed to correct.

The writer states that the said skirt "is not, and never was, meant to be a divided *dress*." Now, as it was invented by Mrs. Charles McLaren, who is one of the Committee of the Rational Dress Society, of which I am President, and as I saw the first ever made, I am in a position to assert that it was solely as a *dress*, and *not* as a petticoat, that the costume was designed. It is, however, obviously in any one's power to wear it as an under petticoat if they choose. The writer is perfectly right, however, in asserting that this partial relief from the burden of the old-fashioned or primitive petticoat is a great gain; but I think if she could bring herself to try the dress, as it was intended, with a shorter overskirt, she would be forced to admit that the gain in comfort is quite doubled. It stands to reason that it must be so. One curtain suspended in front of and round the legs, is undeniably a great impediment to locomotion. Three or four curtains (or petticoats—the principle is the same) so suspended increase the impediment exactly in proportion to their number. Surely the readers of KNOWLEDGE do not require much thought to take in this proposition, and perfect comfort will not be attained until all such impediments are admitted to be as useless as they are uncomfortable. The writer furthermore states that, "the Turkish trouser form exhibited at the Health Exhibition is impossible." What she means is, I presume, that she feels that it would be impossible for her to bring herself to wear a dress so different in form to what others are wearing; and, doubtless, most people will agree with her. But I imagine one of the objects of this Exhibition is to shew how perfect, from a health aspect, clothing may be made, and petticoats distinctly do not come up to this standard. However much, therefore, people may desire to give reasons for following a custom which is based on unreason, it would be more satisfactory if they simply said they prefer dirt, discomfort, and fatigue, to bearing the odium which invariably attaches to any attempt to change a rooted custom. Possibly, at some point of the development of the species, their survival may have depended on their close and unquestioning following of the customs of each preceding generation, judging at least from the strength which remains of what I may call the instinct of custom. An instinct which, however, in the times of which we have any knowledge has added hugely to the miseries of the race of man, in every stage of savagery or civilisation.

But there may be some who, on this particular question of dress, think as I do, that the really impossible thing is that anything so dirty and truly unsuitable to be an article of clothing as the present dress-skirts should continue to be worn by reasoning beings as soon as they begin to allow their actions to be guided by reason instead of this irresponsible instinct. If any one does not perceive what is meant, I would suggest that they should examine their under garments after their next walk down a hill on a slightly dusty road, when the back of the skirt usually just touches the ground.

I fancy few will venture to assert nothing better in the way of dress is ever to be hoped for, even though they may prefer to sit with folded hands rather than to help on a movement which rightly claims to be a greater blessing to women than almost any other.

F. W. HARBERTON.

[The question would seem to be, Is the end to be best obtained by steps in the right direction, or by one great jump which not one in a hundred can or will take?—R. P.]

BRAIN-WEIGHTS, &c.

[1319]—In No. 135 of KNOWLEDGE Mr. Clodd makes certain statements as to cranial capacities, &c., which, in No. 137, Mr. Bates challenges. I have neither the ability nor the ambition to answer questions which are not addressed to me. I offer a few words, only in the hope of drawing some fuller information on a point which, in its remoter issues, Mr. Clodd may consider outside the question of dreams.

Dr. Bastian, quoting Thurnam, states that the brain of a modern *male* European, between 20 and 60 years of age, may be considered to weigh 49 oz.; that of a negro about 44 oz. He adds that the brain-weights of the Hindoo, Hottentot, and Australian black are almost certainly less than that of the negro, but the evidence is not very complete. The brain of a bushwoman, examined by Professor Marshall, was computed at 31·5 oz.; that of an Englishwoman of the same age and height would weigh about 40 oz. Upwards, the human brain has risen to those of Dr. Abercrombie and Schiller, 63 oz., and that of Cuvier, 64·5 oz. The highest brain-weights observed among the great four-handed apes range between 12 and 16 oz.

Professor Owen ("Anatomy of Vertebrates") says:—"Taking the lowest form of human skull that has come under my observation" (that of a native Australian), "the difference is great and abrupt from that of the highest ape, in the superior capacity of the cranium and small size of the face." The same authority states that a difference between the capacities of cranium and face is noticeable between savage and civilised races of mankind, but that it is immaterial compared with the contrast in this respect between the lowest form of human head and the highest of the brute species. A general idea of the extent of the cranial cavity and consequent grade of intelligence (we are still quoting Prof. Owen) may be gathered from the facial angle. In the dog this angle is 20°, in the gorilla it is 40°, in the Australian black it is 85°, in the cultivated European it is 95°.

One word more. Does not the now famous fragment known as the Neanderthal skull represent the lowest form yet found among ancient skulls? Yet its brain capacity is estimated at seventy-five cubic inches—a fair parallel for that of a Hottentot.

If this be so, is not the inference a just one that between the brain of the lowest known cave or drift man and the highest of the existing anthropoids there lies a great gulf across which the industry of the naturalist has yet to cast his spans?

T. C. BIRCH.

P.S.—Readers of KNOWLEDGE will probably be familiar with Mr. Grant Allen's interesting article, "Who was Primitive Man?" in the *Fortnightly*, September, 1882.

SAVAGE CHROMATICS.

[1320]—Will you allow me to ask Mr. Edward Clodd a question? He has made a general statement, in his second part of "Dreams," to the following effect:—"They (*sic* barbaric peoples) have a word for every familiar thing, sound, and *colour*, &c."

In Max Müller's "Lectures on the Origin of Religion" this is, I think, negatived. The following extract will suffice:—"We divide colour by seven rough degrees . . . . Even those are of late date in the evolution of our sensuous knowledge. Xenophanes speaks of Iris as πορφίρεον, φοινίκεον, and χόωρὸν. Aristotle speaks of the tri-coloured rainbow (φοινικῆ, ξανδῆ, πραβίνγ.) Blue is never mentioned as epithet of sky in the "Veda . . . . in the "Zendavesta" . . . . in Homer . . . . in the Old, and even in the New Testament . . . . Demokritos knew of black, white, red, and yellow. In China the number of colours was originally five . . . . In common Arabic the names for green, black, and brown are constantly confounded. It is well known that among savage nations we seldom find distinct words for blue and black . . . As languages advance, more and more distinctions are introduced."

Does not this negative the idea that savages, while they have "no word for animal, plant, sound, or *colour* as abstract terms," have a "word for every familiar thing, sound, and colour?"

I hope you will have space to insert this in a future issue.

E. B. Titchener.

THE "OTTO" TRICYCLE.

[1321]—With regard to the two-track tricycles, as to which Mr. Browning very kindly replied to an inquiry of mine;—did you notice the remarkable statements concerning "The Otto" in the lecture by Mr. Boys, reported in the "Society of Arts Journal" for May 9? and have you any knowledge of the contrivance?

It would be a bicycle, but for a little castor at the end of its diabolical looking tail; and may be described as a tricycle without the small wheel in front.

I am told that "gearing down" has no advantage for climbing hills in this machine, which is hard to believe. Treadmill.

LETTERS RECEIVED AND SHORT ANSWERS.

I have three requests to make to the enormous number of correspondents who favour Knowledge with letters and proffered contributions. First, that in writing upon business matters, or asking that back numbers, index numbers, and the like shall be sent to them, they will legibly address *the Publishers of* Knowledge. The editor doesn't keep a quire or two of this journal at his private residence for sale—he doesn't, indeed. Next that all Chess problems and questions shall be equally legibly addressed to *The Chess Editor*. And lastly (but by no means least) that correspondents will compress and curtail their communications as much as they possibly can. I am often, perforce, compelled to reject letters, &c., on matters of interest, just because they are spun out to about five times the length necessary for the presentation of the points they are intended to illustrate or insist upon. The amount of "fine" writing that finds its way into my waste-paper basket would astound if it did not appal any one who saw it for the first time. Many of my correspondents appear to sprinkle their adjectives over their letters as Mr. Swiveller did his supposititious tears over his, in writing to his aunt—out of a pepper-castor. *Verbum sat sapienti.*—Q. E. F. Thanks; but the question has already been sufficiently answered.—H. J. Madge (secretary to the Swansea Merchant Shipowners' Company) is of opinion "that, excepting during fogs, the majority of collisions occur through a bad look-out, and that 'the complexity of more lights and brighter ones' than those already in use, 'would, under all circumstances, increase the danger.'" I know, as well as my correspondent, that "the present Board of Trade regulations require steamers to keep out of the way of sailing-vessels;" but how if the steamer cannot discern in what direction the sailing-vessel is moving?—Frank O. Baird. I cannot undertake to recommend books on mesmerism. You should write to Mr. Burns, Southampton-row, London. My personal conviction is that the so-called "subject" is, so to speak, under *his own* influence, and that, beyond the induction in his mind of a belief that the mesmeriser can produce certain results on him, such mesmeriser can do nothing. As for any kind of subtle fluid or aura proceeding from the magnetiser to his patient, I regard such an idea as simply nonsense. Any one can resist the "Mesmeric influence" who is in earnest in wishing to do so.—T. Common. You compel me to exclude your letter by saying on three closely-written sheets of notepaper what might have been just as well expressed upon one.—Differential. There is nothing remarkable in your experience of a succession of suicides. Suicide seems to be, in some occult way epidemic, as in Austria at this moment. Nor is the name of the officer whom you met so strange a one that it might not reasonably be expected to coincide with that of the town you specify. It *was* odd that you should so immediately encounter a genuine believer in "Parallax's" rubbish.—D. R. No. You cannot make a patented article for your own use in that or any other way.—F. W. Rudler. Received with thanks.—A Friend who speaks so kindly of our "Gossip" column will find a photograph of the Editor forming a frontispiece to his "Science Byeways," published by Smith, Elder, & Co.—James Jones and Fry & Co. will perhaps kindly read the request with which this column commences.—H. Siggers asks for more geology. I have for some time past hoped to give a series of papers on common fossils and what they teach, and trust that it may not be long ere they appear.—W. Cave Thomas. I have given you as much space as I can at present spare to ventilate your ideas on the Diascope. Your last letter really adds little or nothing to what you said in the one (1302) on page 463.—R. J. Beevon. Yes, in a certain sense, it was a coincidence.—Vox e Deserto. Many thanks for your friendly and amusing letter.—W. T. Morrey. My deliberate opinion of the method of Professor Loisette may be read in No. 117 of Knowledge (page 58). You can judge hence how far it is applicable to your own case.—E. G. T. In the very first French Grammar I take off the shelf (Delille's) I find "mon," "ton," "le mien," and "le tien" all classed as possessive pronouns.—W. Irving Bishop. I can only regret that my multifarious engagements prevented me from being present; and wonder, hopelessly now, why Mr. Labouchere does not pay the £1,000 like an honourable man.—J. Greevz Fisher sends me yards of a "Fonetic" letter beginning "ei am sori." So am I.—Payshunt Lamm. In the beautiful words of the poet—

> "Decortications of the golden grain
> Are set to allure the aged fowl—in vain."

Dorrett sends a circular of Summer's Patent Hygienic Apparatus, to which he invites Mr. Ady's attention.—Thomas Ayers. Whether Mr. Garbett is right or wrong (and I hold a very definite idea indeed myself on that subject) it is perfectly certain that the inclination of the earth's axis has never varied more than 2° 37′ since she became solid.—Chas. J. Fox. I can hardly reprint what has already appeared in other journals, valuable as your practical experience is in the matter of ships' lights, &c. You are far from singular in having noticed the strange redness in the sky very long prior to the Java earthquake; but, for advertising purposes, the Krakatoa theory is being rammed down the public throat, "and the people love to have it so."—R. Jones. The number of superficial inches in this paper is a fixed and limited quantity, and I must allot it, as impartially as I can, among the great army of correspondents who address me on subjects of scientific interest.—John A. Jones. I really have no time either to act as a private tutor or to answer catch questions set in examinations.—Peter Scott is just the least thought vague. He tells me that his brother "has fallen upon an ingenious means of utilising fogs," and invented a "Fog-prevention apparatus." How a man can utilise anything by preventing it does not immediately occur to me. He further asks me if one of these machines may be sent to my address? With very many thanks, No. Furniture and books occupy too much space here to leave much room for heavy mechanism. I will, however, attentively consider any description of the invention sent to me, and say what I think of it.—J. Edwards. If "the present is never here," where is it?—Robert Mitchell. Many thanks. Nothing but an overwhelming press of engagements prevented my attendance.—Excelsior sends us as a coincidence the sequent announcements in a newspaper of the death of people named Barnes, Bell, and Brown, there being a firm of photographers named Brown, Barnes, and Bell. It *is* a coincidence, and that is all; there being very little that is wonderful in it when we consider how very common the names are. Had they been—for example—Pottlebury, Smifkins, and Haycock, the odds against such a concurrence would have been enormous.—C. W. B. Thanks for your kind and appreciative letter.

Combining Colours.—At the Physical Society, on Saturday, June 14, Mr. H. H. Hoffert exhibited an ingenious apparatus for the synthesis of colour, and which may be usefully employed in studying colour-blindness, though primarily intended for demonstrating the laws of colour combination. The apparatus consists essentially of a series of platinum wires, which are rendered incandescent by the current from Grove cells, and the intensity of the light they emit is capable of being graduated by rheostats in circuit with the wires. The light from several of these wires is passed through the prisms, and the refracted rays of a certain colour can be transmitted to the eye-piece through which the observer looks. The rays can either be compared side by side, or superposed so as to give compound colours, thus illustrating the production of tints.—*Engineering*.

Our Mathematical Column.

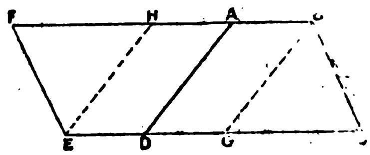
NOTES ON EUCLID'S FIRST BOOK.

By Richard A. Proctor.

(*Continued from p. 445.*)

Prop. XXXVI.—*The area of a trapezium is equal to half that of a parallelogram whose base is equal to the sum of the two parallel sides of the trapezium, and whose altitude is equal to the distance between them.*

Let A B C D be a trapezium, the sides A B C D being parallel. Produce A B to F, making A F equal to D C; and C D to E, making D E equal to A B, and join E F.

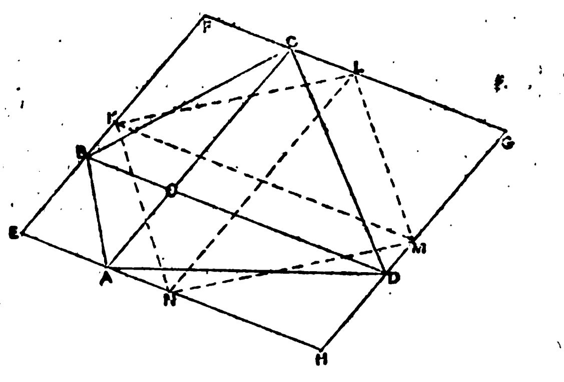


Then F B is equal to F A and A B together; that is, to D C and D E together (*const.*) that is to E C; and F B is also parallel to E C. Hence E F is equal to B C, Euc. I., 33; and F E is therefore the parallelogram, having a base equal to the sum of the parallel sides A B, D C, and an altitude equal to the distance of A B from D C. Now, it is obvious that the trapezium A B C D is equal to half the parallelogram F C, since F E D A is a trapezium equal to A B C D in all respects.

The equality of F E D A and A B C D is so obvious as scarcely to need demonstration. It is clear that the lines B G, E H drawn parallel to A D, divide F C into the pair of equal parallelograms H D, D B, and the pair of equal triangles E H F and B G C.

Prop. XXXVII. *The area of a quadrilateral is equal to half that of the circumscribing parallelogram, whose sides are parallel to the diagonals of the quadrilaterals.*

Let the parallelogram E F G H circumscribing the quadrilateral A B C D have its sides E F, H G parallel to A C, and F G, E H parallel to B D; then shall the quadrilateral A B C D be equal to half the parallelogram E F G H.



Let A C, B D intersect at O. Then E O is a parallelogram; therefore the triangle A B O is equal to the half E O (Euc. I., 34); similarly the triangles B O C, C O D, D O A are equal to half O F, O G, and O H respectively. Hence the whole figure A B C D is equal to half the parallelogram E F G H.

Cor. 1.—The parallelogram E F G H is equal to twice any quadrilateral as A B C D, so inscribed that the diagonals A C and B D are parallel to E F and F G respectively.

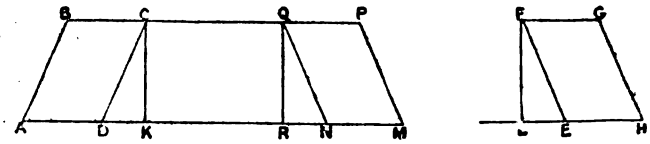
Cor. 2.—Let K, L, M, N, be the bisections of E F, F G, G H, and H E. Then K L and N M are each parallel to E G by Prop. XX., and therefore to each other; and similarly L M is parallel to K N. Hence K L M N is a parallelogram; and the diagonals K M, L N, are equally inclined to those of the quadrilateral A B C D. Now by Cor. 1, E F G H is double both of A B C D and K L M N; hence A B C D is equal to K L M N: that is, a quadrilateral is equal to the parallelogram having diameters equal to those of the quadrilateral and equally inclined to each other.

Cor. 3.—Quadrilaterals having equal diameters (each to each) equally inclined, are equal to each other.

Prop. XXXVIII.—*Parallelograms on equal bases and of equal altitude are equal to each other.*

Let A B C D and E F G H be parallelograms on equal bases A D, E H, and of equal altitudes C K and F L. The parallelogram B D shall be equal to the parallelogram F H.

Produce A D to M, take N M equal to E H or A D, and complete the parallelogram Q M equal in all respects to F H. Draw Q R perpendicular to A M and join C Q.



Then the triangles Q N R and F E H are equal in all respects (Euc. I. 26). Hence Q R is equal to F L, that is to C K. But since Q R and C K are each of them perpendicular to A M, they are parallel to each other. Hence C Q is parallel to A M, and the parallelograms B D and Q M are equal (Euc. I., 36). But Q M is equal to F H. Therefore B D is equal to F H.

Prop. XXXIX. *Triangles on equal bases and of the same altitude are equal to one another.*

The triangles are the halves of parallelograms on equal bases and of the same attitude; and are therefore equal by the preceding proposition.

Prop. XL. *Equal parallelograms on equal bases are of the same altitude.*

Prop. XLI. *Equal triangles on equal bases are of the same altitude.*

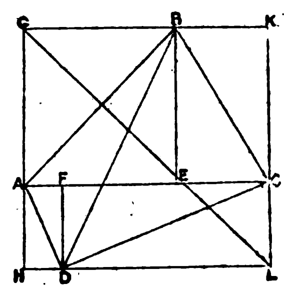
Prop. XLII. *Equal parallelograms of equal altitude are on equal bases.*

Prop. XLIII. *Equal triangles of equal altitude are on equal bases.*

These four propositions require no demonstration; for if we assumed that the attitudes in the two former, or the bases in the two latter were unequal, an obvious absurdity would result.

Prop. XLIV. *Let A B C D be a quadrilateral figure, and from B, D the extremities of one diameter let B E, D F be drawn, perpendicular to A C the other diameter. The area of the quadrilateral A B C D shall be equal to the area of a right angled triangle having one side equal to A C and the other equal to the sum of the lines B E and D F.*

Through A and C draw the lines G A H and K C L at right angles to A C; and through B and D draw the lines G B K, H D L parallel to A C, and therefore at right angles to G H and K L.



Then the rectangle G C is equal to twice the triangle A B C, and the rectangle A L is equal to twice the triangle A D C. Therefore the whole rectangle G K L H is equal to twice the quadrilateral A B C D. But the rectangle G H L K is equal to twice the triangle G H L. Therefore the quadrilateral A B C D is equal to the right angled triangle G H L which has one side H L equal to A C, and the other side G H equal to the sum of the lines B E and D F.

EASY RIDERS ON EUCLID'S FIRST BOOK.

With Suggestions.

Prop. 32 (*continued*).

122. Construct an isosceles triangle which shall have one-third of each angle at the base equal to half the vertical angle.

123. Construct a triangle having angles equal to those of a given triangle, and the sum of the sides containing a given angle equal to a given straight line.

Suppose A B C to be the required triangle, so that A B, and B C together may be equal to a given straight line. Produce A B to D making B D equal to B C. Then it will be found that enough is known about the triangle A D C to enable us to construct it, and hence to construct the required triangle.

124. The hypotenuse of a right angled triangle is equal to twice the distance separating the right angle from the bisection of the hypotenuse.

125. Perpendiculars are let fall from two angles of a triangle upon the opposite sides. Show that their feet are equidistant from the bisection of the side opposite the remaining angle of the triangle.

126. A B C is an equilateral triangle, and B D is drawn perpendicular to the base A C; a point E is taken in B D so that E A, E B, and E C are all equal. Show that the angle A E C is equal to four times the angle E A D.

127. With the same construction, if D B is produced to F so that B F is equal to A B or B C, then the angle F A C is equal to two and a-half times the angle A F C.

128. In the base B C of an isosceles triangle A B C, a point D is taken, and a point E is taken so that C E is equal to D C. Show that three times the angle A E F is greater than four right angles by the angle A F E.

Begin by showing that three times the angle A E F *is equal to four right angles increased by the angle* E C D *and diminished by the angle* E D C. *This follows readily from the fact that the angle* A E F *is equal to the two angles* E C D *and* E D C *together. The rest is obvious.*

129. In the triangle A B C the side B C is bisected at E and A B at F; A E is produced to G so that E G is equal to A E, and C F is produced to H, so that F H is equal to C F; show that the points G, B, and H are in one straight line.

It may be shown that the angle H B A *is equal to the angle* B A C, *&c.*

130. Construct a right-angled triangle, having given the hypotenuse and the sum of the sides.

Suppose the triangle A B C *to be constructed as required,* A B *being the hypotenuse; then if* A C *be produced to* D *so that* C D *is equal to* C B, *the triangle* B C D *has two angles each equal to half a right angle. Therefore in the triangle* A B D *we have given us* A B, A D, *and the angle* D. *This suffices for the solution of the problem*

131. Construct a right-angled triangle, having given the hypotenuse, and the difference of the sides.

The analytical treatment resembles that of Ex. 126.

132. Construct a right-angled triangle, having given the hypotenuse and the perpendicular from the right angle on the hypotenuse.

Construct first a right-angled triangle A B C, C *being the right angle,* A B *equal to half the hypotenuse of the required triangle, and* B C *equal to the given perpendicular; produce* A C *(both ways), to* D *and* E, *so that* A D *and* A E *may each be equal to* A B. *Then show that* D B E *is the required triangle.*

133. Construct a right-angled triangle having given the perimeter and an angle.

From the extremities of a line A B *equal to the given perimeter draw lines* A C, B C *inclined to* A B *at angles respectively equal to half a right angle and to half the given angle. Draw* C D *perpendicular to* A B. *Then* C D *is a side of the required triangle. The rest of the construction and the proof will readily suggest themselves.*

134. Construct a triangle of given perimeter, having its angles equal to those of a given triangle.

The method used in Ex. 129 *must be applied, with variations which will at once suggest themselves.*

135. Construct a triangle, having given one side, an angle opposite to it, and the sum of the remaining sides.

The method is the same as that of Ex. 130.

136. Construct a triangle, having given one side, an angle opposite to it, and the difference of the remaining sides.

The method is that of Ex. 131.

137. If in the sides of a square, at equal distances from the four angles, four points be taken, one in each side, the figure formed by joining these will be also square.

138. If the alternate angles of any polygon be produced to meet, the angles formed by these lines, together with eight right angles, are together equal to twice as many right angles as the figure has sides.

139. A P, B P, and C P are the internal bisectors of the angles of the triangle A B C. A P is produced to meet B C in D, and P M is drawn perpendicular to B C; show that the angle B P D is equal to the angle C P M.

140. Construct a right-angled triangle having equal sides, its right angle and one of the remaining angles upon two given parallel lines, and the third angle at a given point.

Draw a perpendicular from the given point to the nearest parallel, and from the foot of this perpendicular measure off along the parallel a distance equal to the distance separating the parallels. The point thus indicated is the right angle of the required triangle.

141. Construct a right-angled triangle having equal sides, its right angle at a given point and its other angles upon two given parallel lines.

This problem may readily be shown to depend on the preceding one.

M. CH. MONTIGNY, of Brussels, whose beautiful spectrometer was described and illustrated on pp. 180 and 181, has found that the presence of aqueous vapour in the atmosphere is always indicated by the preponderance of blue among the colours exhibited by a twinkling star, which is an infallible sign of rain. On the other hand, a deficiency of blue and the predominance of green—and also of violet—show that the air is in an abnormally dry state. On the strength of these indications M. Montigny predicted that 1883 would be a drier year in Brussels than those which had immediately preceded it—a prediction accurately fulfilled. Relying on the persistence of this same sign he now announces that we have emerged from the period of wet years, which commenced in 1876, and may look forward to a series of finer and drier ones. Meteorologists will regard this vaticination with considerable interest.

Our Whist Column.

THE HANDS.

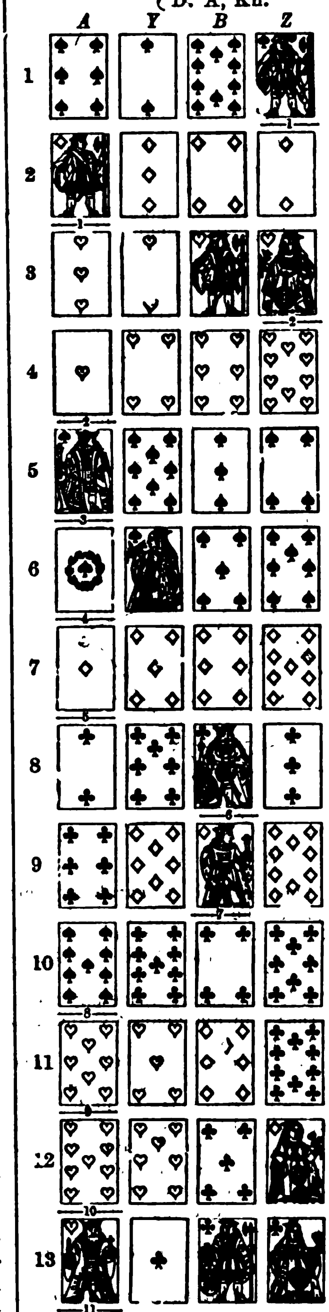
B { S. 10, 5, 3. H. Kn, 6. D. K, 7, 6, 4. C. K, Kn, 5, 4. }

Y { S. Q, 8, 2. D. 8, 5, 3. H. 7, 5, 4, 2. C. A, 9, 7. }

A-B, 0. B — Y — Y-Z, 0. — Tr. S4 — Z — A

Z { 4, 7, Kn. S. 2, 9, 10, Q. D. 10, Q. H. 3, 8, 10, Q. C. }

A { S. A, K, 9, 6. D. A, Kn. H. A, K, 9, 8, 3. C. 6, 2. }



THE GAME.

1. With his magnificent hand, *A* of course leads trumps. He is playing for "game," and it is desirable to ascertain whether his partner has an honour, in which case *A*'s task will be comparatively easy. However he finds *B* without an honour.

3. *A* now leads a small card, not the usual lead, from his strong plain suit, to learn what help he may expect from his partner in that suit, but again he is disappointed. *Y Z* have now made two tricks, and *A B* must make all the rest, if game is to be won.

4. *Z*, a keen player (the Editor of the *Westminster Papers*) leads the single card remaining in *A*'s suit, in the hope of making one of his small trumps and saving game. Unfortunately he thus gives *A* complete command of Hearts. *A* has now two by tricks *certain*.

5 and 6. *A* clears out trumps, and has now three by tricks certain.

7. The lead here is tentative. *A* wants to learn how his partner stands in Diamonds before leading Clubs (in the hope that *B* may get a trick and *A* a discard in Clubs). Of course, he cannot make sure of game, indeed the chances seem against it, but he now knows that *B* holds either King or Queen of Diamonds; for from his lead *Z must* have two more Diamonds, and these must be two of these three—King, Queen, Ten; but they cannot be King, Queen, or he would have led King.

8. *A* tries his chance for game. *Y* should have put on Ace. Despite *A*'s lead of a small card at trick 3, it is known to *Y* that *A* holds King of Hearts, and probably has entire command of the suit. In fact this is practically proved by *A*'s change of suit after trumps were exhausted. *A* evidently wants a discard, and it must be a Club he wishes to discard. *Y* should therefore have at once played the Ace to save game.

9. The rest of the game plays itself.

The above game is from the *Westminster Papers* (notes, as usual, by Five of Clubs). The leader, through whose skilful conduct of the hand the game was won, was our esteemed correspondent Mr. Lewis, and the game is a pretty illustration of his style. As the cards actually lie, if he had cleared out trumps after trick 2, and then gone on with his long suit, opening with King, Ace, he would have made four by tricks; but no more: and of course he had no reason to expect to establish his Hearts in two rounds.

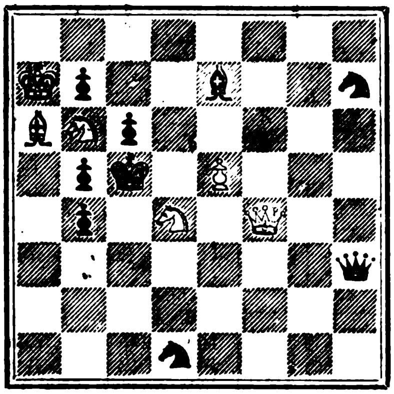
Our Chess Column.

By Mephisto.

SELECTED PROBLEM.

By G. E. Carpenter.

Black.



White.

White to play and mate in three moves.

THE RUY LOPEZ.

1. P to K4 / P to K4 2. Kt to KB3 / Kt to QB3 3. B to Kt5 / ——

This is one of the most useful openings known to Chess-players. It yields White some chance of attack, while, with correct play, Black can only draw. But either player may lose if, to force a win, an untimely attack is instituted. Any player acquainted with the principle of this opening has a comparatively easy task in hand. By choosing this opening, when meeting a strong antagonist, a player at once does away with the necessity of being well grounded in all the variations of different lines of play, and is, therefore, less liable to a sudden collapse in the early stages of a game.

The best reply now is 3. P to QR3, which alone we will examine. (Black may also play 3. Kt to B3, to which White will reply with 4. P to Q3 with a safe position, which only requires ordinary care to be developed into a fair game. There is less in another move played by Bird, viz., 3. Kt to Q5. After 4. Kt × Kt / P × Kt 5. Castles / —— the position is featureless, except that White is a move ahead in his development, and will be able to take care of himself.)

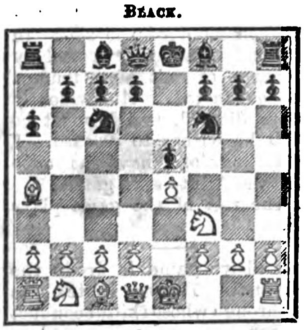
3. P to QR3

White would not gain anything by playing 4. B × Kt / QP × B 5. Kt × P / —— Black will recover the P by Q to Q5 with gain of time.

4. B to R4 4. Kt to B3

White has four moves at his disposal, viz., 5. Castles, 5. P to Q4, 5. Kt to B3, and 5. P to Q3. The variation mostly played is

Black.



White.

5. Castles 5. Kt × P
6. R to Ksq (*a*) 6. Kt to B4

White can now either play 7. B × Kt, QP × B. 8. R × P (ch), B to K2. 9. P to Q4, Kt to Q3 with a safe game, or

7. Kt × P at once 7. Kt × Kt
8. R × Kt 8. B to K2

In this variation it is of main importance to play B to K2, however White may vary his moves. After that Black will Castle, and the position is one that, so to speak, will take care of itself. To make things more safe, Black may in reply to 5. Castles at once play B to K2. If then 6. P to Q4, P × P. 7. P to K5, Kt to K5. 8. R to Ksq, Kt to B4, &c.

(*a*) Instead of 6. R to Ksq, White may play the attacking move of 6. P to Q4. It would be bad to reply with either P × P or Kt × P, as in the former case White would proceed with 7. R to Ksq, and, in the latter case, with 7. Kt × Kt, followed by R to Ksq, &c. In reply to 6. P to Q4, Black plays

6. P to QKt4
7. B to Kt3 7. P to Q4
8. P × P 8. B to K3

followed by B to K2, and Castling with a safe game.

(*To be continued.*)

SOLUTION.

Problem, p. 446.

1. Q to R2	if	1. Kt × Q	2. R × R, mate
	if	1. K × R	2. Kt to B4, mate
	if	1. R × R	2. Kt to Kt4, mate
	if	1. K to B7	2. Q to R7, mate

ANSWERS TO CORRESPONDENTS.

**** Please address Chess Editor.*

J. E. Kerrogan.—Pray consider firstly, the mind is assisted in grasping a position by optical means; secondly, that a great many of our readers never resort to a chess-board, but always solve Problems, &c., off the Diagram; thirdly, that Diagrams prevent mistakes, and you will come to the conclusion that they are useful.

J. de Soyres.—Thanks for communication.

G. Woodcock.—Pardon the ommission. We remember seeing the position, and could not see any way to force a win after exchange of Queens. Thanks for Problem.

Correct solutions received of S. Osborne. Problem p. 426: Punch, Mycetes, Caissa. Problem p. 446: G. Woodcock, A. W. Overton, M. T. Hooton. Problem p. 467: A. W. Cunard, A. C. Hurley, John Watson, M. T. Hooton, W.

A. W. Overton, S. Osborne.—Problem p. 426 seems to admit of another solution by 1. R to Q2.

W. Hamahan, Problem p. 446.—If 1. Q to K5, Kt (Kt7) to K6?

Uncle John—George Gouge, Problem p. 467.—If 1. B to K6, Kt × BP?

Donna—Edm. Stevens, Problem p. 467.—If 1. Q to B7, B to Rsq?

H. A. N.—G. Woodcock, Problem p. 467.—If 1. K to B8, Kt to K3 (ch)?

Hannah Fellows, Problem p. 446.—If 1. Kt to B4 (ch), Kt × Kt, and there is no mate.

Velocity of Sound in Air.—Mr. Blaikley, who read a paper some time ago before the Physical Society on the velocity of air in tubes of fine bore, has made some further experiments on the matter, and obtained several interesting results. The velocity of sound in air decreases with the bore in a fairly regular manner. Thus, with a tube of 11·4 millimètres in diameter the velocity was only 324·28 mètres per second, whereas with a tube 88·2 millimètres in diameter the velocity was 330·13 mètres, or very nearly that in free air. Mr. Blaikley finds that the best pipes or tubes to use for his purpose were those in which the upper proper tones are in harmonic order, or better still, those in which they were far removed from the harmonic order, or in other words, dissonant.—*Engineering.*

Contents of No. 138.

SPECIAL NOTICE.

Part XXXI. (May, 1884), now ready, price 1s. 3d., post-free, 1s. 6d.

Volume IV., comprising the numbers published from July to December, 1883 is now ready, price 7s. 6d.; including parcels postage, 8s.

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